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A Veteran Science:
Operations Research and Anglo-American Scientific Cultures, 1940-1960

A dissertation presented

by

Gerald William Thomas

to

The Department of the History of Science

in partial fulfillment of the requirements
for the degree of
Doctor of Philosophy
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**A Veteran Science:
Operations Research and Anglo-American Scientific Cultures, 1940-1960**

ABSTRACT

This dissertation traces the intellectual trajectory of the field of operations research (OR) from its origins in World War II to approximately the year 1960. It explores how OR transformed from an adjunct to military planning practices into a profession encompassing military, theoretical, and practitioner subcultures. It pays particular attention to the influence of the wartime experience of scientists working in OR groups on later manifestations of the field. In particular, it argues that maintaining relevance to actual acts of policymaking both drove OR to adopt a canon of mathematical theory in order to distinguish it from more general consulting professionals, and that many of its institutional innovations were designed specifically to ensure that those trained in theoretical techniques could apply their skills to practical situations.

This approach differs from other approaches in that it downplays any notion of operations research as a rationalizing agent in postwar policymaking. Prior explorations of the "expert" cultures in which OR is typically included stress that they reinforced the dominant American military-industrial power structure by creating tools of social control and by justifying policy decisions using the authority inherent in quantitative science. This dissertation argues against this historiographical approach, primarily by arguing against the division between scientific and non-scientific methods of policymaking. Because OR relied so strongly upon its compatibility with extant methods of

policymaking, emphasizing the status of OR as a special scientific approach seems fruitless. This point seems especially true given that most OR studies were not expected to settle political controversies, but to make mundane day-to-day policymaking more robust.

I offer an alternative analytical framework that eschews divisions between the rational and the intuitive, and replaces them with more appropriate divisions between the rational and the arbitrary. This framework focuses less on knowledge production and application, than on the trading of insights between distinct intellectual communities. The framework yields new information about why OR's proponents made the intellectual choices and built the institutions that they did, and what role OR historically played in military and industrial policymaking, and promises to shed new light on the nature of the policy-oriented sciences.

CONTENTS

for Gramz

Acknowledgements

I first heard those magic letters "OR" amid the gleaming futurescapes of the Year 2000. I was looking for a topic for a senior thesis in history at Northwestern University. I thought I might want to do something on science in the postwar period, and I thought I might do something that could get me funding to go to Britain. After some back and forth, Ken Alder, emailing from France, recommended I look into something called operations research, and that I look at an article by Mike Fortun and Sam Schweber, and a dissertation by Erik Rau. The world of operations research has haunted and delighted me since that moment.

My first thanks must go to the people back at Northwestern who put me on the trail of the history of science: Ed Muir, Peter Hayes, Ethan Shagan, Guy Ortolano, and especially Ken Alder, who has given me good advice even in the years after I left. Go 'Cats!

My advisor Peter Galison and David Kaiser have overseen this project and my education as an historian of science since 2002. I did not realize when I arrived at Harvard that the history of science was much different from history; they have helped me weather the transition. Allan Brandt, Sam Schweber, Jon Agar, Michael Gordin, and Steven Shapin have all offered good advice along the way. I also thank all of my colleagues at Harvard and down at MIT in all the venues in which we have conversed: Dave Kaiser's courses down at MIT, the Harvard History of Physical Sciences Working Group, the fishbowl (i.e. the Graduate Student Lounge), and may God bless PhunDay. I would especially like to thank Lambert Williams, whose intellectual ambition has been an inspiration to think big. Let the record show I still owe Nasser Zakariya a pot of

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The intellectual community has always been a worldwide invisible college, but with email it is all the more so. Erik Rau has been a great scholar with whom to share this too-big topic of OR; does anyone else want in? Conversations with Erik have helped me find my own perspective on this fascinating world. Encounters with Paul Erickson never disappoint. I also have had very useful correspondence with John Krige, Hunter Crowther-Heyck and Maurice Kirby. Comments received at the 2003 Midwest Junto for the History of Science, and the 2005 meeting of the History of Science Society, and referees for *Isis* and the *British Journal for the History of Science* have also been valuable.

In 2005-2006 I became a journeyman, and I can see why the old craftsmen did it. My time on the road was vital to my growth as a scholar. First, my thanks to the Department of the History and Philosophy of Science at Cambridge University and the Centre for the History of Science, Technology, and Medicine at Imperial College for taking me on as a visiting scholar in the spring of 2006. Thanks to Marina Frasca Spada and St. Catharine's College, Cambridge for arranging for a reasonably priced place for me to stay. In this period I had the benefit of conversations with Martin Collins, Ted Porter, Norton Wise, Malcolm Llewellyn-Jones, Gary Werskey, and Simon Schaffer, who also offered good advice as the editor of the *BJHS*. Special thanks are owed to Andy Warwick and all the faculty and students at Imperial for their immense hospitality and the great conversations at the Queen's Arms. Imperial is an amazing place to do the history of science. I owe my greatest intellectual debt to David Edgerton of Imperial. This

dissertation would be very different and much inferior if not for his unsparing encouragement, equally unsparing criticism, and sage guidance on issues both broadly methodological and narrowly factual.

Although I have not drawn extensively on oral history in the finished draft of this dissertation, interviews with John Little, Freeman Dyson, John Magee, and Jay Forrester have been influential in my thinking. Special thanks are owed to John Little for passing my name on to the rest of the OR community, which has been very encouraging of my work. Saul Gass, in particular, has been extremely generous in lending me his time, influence, wisdom and, yes, documents. Further thanks go to Anna Nagurney, Tina Wakolbinger, and the others at UMass (thanks for the mug—I use it every day), Jim Orlin, Jonathan Rosenhead, Ernest Konigsberg, Peter Horner, Mark Eisner, Gene Visco, Randy Robinson, Frank Trippi, Russell Ackoff, and Graham Rand.

All errors of fact and interpretation are my own.

Archival research for this project was conducted at the MIT Archives and Special Collections, where Nora Murphy has been of great help; the National Archives Northeastern Branch in Waltham; the National Archives in College Park, Maryland; the Library of Congress Manuscript Division; the National Air and Space Museum Graber facility; the National Academy of Sciences archive; the George Washington University special collections; the Hamburger Archives at Johns Hopkins University; the RAND Corporation library and archives, with special thanks to Vivian Arterbery and Ann Horn; the California Institute of Technology archives; the Imperial War Museum Department of Documents; the Royal Society archives; the impeccable National Archives of the UK at Kew; the Churchill Archives Centre; and the Cambridge University Library Manuscripts

Department. I would like to give special thanks to all those institutions that permit digital photography of documents; this is the way of the future. Special thanks go to Greg Kaminski of the CNA Corporation library for the big box of documents, and to Erik Rau for putting in years of work to free the big box of documents.

The bulk of this project has been accomplished under a Graduate Research Fellowship from the National Science Foundation held from 2004 to 2007. This work could never have been accomplished in such a small amount of time without the NSF's generous funding for the history of science. This work also owes a debt to the Harvard History of Science Department for funding travel to conferences, and to an Undergraduate Research Grant from Northwestern University.

As with all projects of this sort, the greatest debts are personal. I want to thank all of my friends for their support, with special thanks to Jeanne Haffner for her tolerance, input, infrastructural assistance, and warm support through a lot of the most stressful work on this project. Thanks to my roommates along the road for keeping me company during some grueling and isolating work, especially Chris DeAngelis. I would also like to thank my friends who have lent a place to stay along the road: Calvin Koo, Terence Li, and Mike Wong. I also believe every dissertation should acknowledge earlier teachers. I would like to single out Bob Normoyle and Red Lyons.

Above all my family has been behind me since forever: my sister Elizabeth Bradford, and my step-dad Scott Bradford; my mom Jennifer Bradford has contributed love, wit, style, and good advice. I owe it all to my grandmother, Jeanne Walter. If this dissertation shows any independence of thought, restless curiosity, empathy, and good sense, it is surely on account of her.

ARCHIVE AND SOURCE ABBREVIATIONS

AVHL	The Papers of A. V. Hill, Churchill Archives Centre, Churchill College, Cambridge, UK
BJHS	*British Journal for the History of Science*
CEL	The Papers of Curtis E. LeMay, Manuscript Division, Library of Congress, Washington, DC
ELB	The Papers of Edward L. Bowles, Manuscript Division, Library of Congress, Washington, DC
GOEV	The Papers of Sir Charles Goodeve, Churchill Archives Centre, Churchill College, Cambridge, UK
HMSO	Her/His Majesty's Stationery Office
HBR	*Harvard Business Review*
HPR	The Papers of H. P. Robertson, Archives of the California Institute of Technology, Pasadena, CA
HSPS	*Historical Studies in the Physical and Biological Sciences*
HTT	The Papers of Sir Henry Tizard, Department of Documents, Imperial War Museum, London, UK
JDB	The Papers of J. D. Bernal, Manuscripts Department, Cambridge University Library, Cambridge, UK
JHU	Administrative Records of the Johns Hopkins University, Ferdinand Hamburger Archives, Milton S. Eisenhower Library, Baltimore, MD
JvN	The Papers of John von Neumann, Manuscript Division, Library of Congress, Washington, DC
MIT	Administrative Records of the Massachusetts Institute of Technology, Institute Archives and Special Collections, MIT Libraries, Cambridge, MA AC 4: Office of the President, Records, 1930-1959 AC 132: Julius A. Stratton, Records, 1949-1957

NACP	National Archives and Records Administration (NARA), National Archives II facility at College Park, MD RG: Record Group (a list of record groups consulted appears in Sources Cited). *Note on Citations*: NARA record groups are broken down into separate file listings, which have a certain name, e.g. "Office of Field Service: Manuscript Histories and Project Summaries, 1943-1946". In my citations, this name is followed by a bracketed code, e.g. [NC-138/177], which denotes this file's unofficial finding aid and entry number, i.e. Finding Aid NC-138, Entry 177. This information can be useful in locating files at the archives, but it is subject to change. I also use the bracketed code to denote the file name on subsequent appearances.
NASM	National Air and Space Museum, Smithsonian Institution, Garber Facility, Suitland, MD
NRC	National Academy of Sciences, National Research Council archives, Washington, DC
PMM	The Papers of Philip M. Morse (MC 75), Institute Archives and Special Collections, MIT Libraries, Cambridge, MA
TNA: PRO	The National Archives of the United Kingdom: Public Record Office, Kew, London, UK ADM: Records of the Admiralty AIR: Records of the Air Ministry and the Royal Air Force AVIA: Records of the Ministry of Aircraft Production CAB: Records of the Cabinet Office COAL: Records of the National Coal Board DEFE: Records of the Ministry of Defense WO: Records of the War Office
PMSB	The Papers of Patrick Maynard Stuart Blackett, Royal Society Archives, London, UK
SHPS	*Studies in the History and Philosophy of Science*

INTRODUCTION

Operations Research, 1940-1960: A Veteran Science

Operations research (OR) is a concept that embodies three distinct professional identities. First and foremost, it connotes a field of inquiry concerned with the development of a certain set of applied mathematical, statistical, and computational methods. It is also a more traditional profession, the practitioners of which use quantitative models to aid private and public organizations in designing and revising policies and technological systems to serve their goals more effectively. It is also an activity performed within military (and sometimes non-military) organizations that makes generalized studies of plans and policies by qualitatively mapping out their rationales and performing more quantitative investigations of plan and policy components to make decision making more robust. These three related but distinct notions of OR were largely set in place by 1960, but only after a long and winding intellectual journey that had begun some two decades earlier.

The term "operations research" has been in existence since its British variant, operational research, was first coined in the late 1930s to describe the auxiliary research performed by scientists and engineers to help military tacticians adapt radar, then an unrefined and unwieldy ground-based technology, into a set of practices for guiding fighter aircraft toward incoming bombers. From the summer of 1941 until the end of World War II, OR came to be identified less with the operational use of specific technologies, and instead became a direct aid to military planners in both Britain and

America by focusing on field operations as a whole. These wartime OR investigations were broadly defined as any concerted act of data gathering or analysis designed to supplement the knowledge informing operational planning, and they rarely made use of mathematical modeling techniques of any real sophistication. Even still, like veteran soldiers, many of the scientists who participated in wartime OR groups were profoundly impacted by the memories of work done in collaboration with military planners—work that saved lives and contributed to a decisive victory over a dire threat. They were convinced that OR represented the crystallization of an idea, long in waiting, that all kinds of executive decision making could be made more successful if they were consistently supported by research. However, if carried to its logical conclusion, the definition of OR as generic research in the service of planning could have involved the direction and undertaking of such activities as environmental impact studies, political polling, and investment research. There were those who felt OR should extend so far.

From the practical standpoint of discipline building, though, attempting to encompass such a wide field would have been impossible. OR's proponents, especially in America, had to cope with the fact that engineers, accountants, lawyers, businessmen, and administrators had already divided and developed business activity so effectively that scientists had no immediately obvious research tools that could contribute to the synthetic problems of the boardroom in ways that existing managers and management consultants could not. Faced with this problem, OR's proponents exploited their ties with mathematical statisticians, applied mathematicians, and economists, which helped them to find new niches in industry where sophisticated formal models could be of use, such as in the management of inventories and distribution networks. At first in America, and soon in Britain, these methods came to be seen as a unique methodology belonging to OR, and as something to which academic OR theoreticians could devote their careers. The adoption of a new set of mathematical methods did not, however, supercede the original OR proponents' war-fostered commitments to practical application. The legitimacy and cogency of their wartime work had hinged on their ability to relate their studies to military planning activities. They firmly believed that the legitimacy of model use was tied to the correspondence between models and business policy, and they urged that OR not be defined by its mathematics. This commitment kept the new profession they were building separate from more academic fields in the decade following the war.

Thus, throughout the 1950s, operations research blossomed alongside related "policy sciences" such as systems analysis and decision theory.[1] It acquired all the usual accoutrements of an independent profession: professional societies, journals, an intellectual canon, pedagogical programs, and textbooks. By 1960 the effort to establish OR outside of its original military context had clearly succeeded. Its professional identity had stabilized and replaced the wartime experience as the chief driver of the field's ongoing development. Yet, when this stabilization took place, it did so around a strange mélange of academic, practitioner, and military subcultures that have never sat particularly comfortably together, but have never been antagonistic or coherent enough to break the field apart. The existence of these subcultures remains, like a scar, as an emblem of the field's origins as a veteran science.

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[1] One may interpret the term "policy sciences" more or less broadly. Daniel Lerner and Harold D. Lasswell, eds., *The Policy Sciences: Recent Developments in Scope and Method* (Stanford: Stanford University Press, 1951), used it to refer to all social scientific methods geared toward policymaking, including but not limited to those considered here; see especially the contribution Kenneth J. Arrow, "Mathematical Models in the Social Sciences," pp. 129-154. To launch the new journal *Policy Sciences* in 1970, the systems analyst Edward Quade used the term to refer to OR and closely associated fields such as "systems analysis, simulation, 'war' gaming, game theory, policy analysis, program budgeting, and linear programming, to name a few…"; see E. S. Quade, "Why Policy Sciences?" *Policy Sciences* **1** (1970): 1-2.

Finding an Intellectual History of OR

The most immediate object of the history I am writing is to explain the remarkable transatlantic intellectual trajectory of operations research. The first thing with which we must contend if we wish to make sense of this trajectory is the enormously rich and diverse institutional and intellectual environment that surrounded and pervaded OR. Unfortunately, historians heretofore have either treated OR as a self-contained and self-evident concept that grew out of a wartime kernel, or they have been content to contextualize its development as a product of World War II and the Cold War.[2] The task that is before us, therefore, is to develop a new history that relates the *practices* of OR to

[2] The following are some of the most notable and recent works on the history of OR. Maurice W. Kirby, *Operational Research in War and Peace: The British Experience from the 1930s to 1970* (London:: Imperial College Press, 2003) and Charles R. Shrader, *History of Operations Research in the United States Army, Volume I: 1942-1962* (Washington, DC: Government Printing Office, 2006) both follow a long tradition in viewing OR as the systematic expression of previously scattered attempts to apply scientific methodology to military activities. Histories in this tradition tend to seek out precursors to the idea of OR. Saul I. Gass and Arjang A. Assad, *Annotated Timeline of Operations Research: An Informal History* (New York: Klewer Academic, 2005) does much the same thing, except from the perspective of probability and statistical theory rather than military planning. Erik Peter Rau, "Combat Scientists: The Emergence of Operations Research in the United States During World War II," Ph.D. Dissertation, University of Pennsylvania, 1999; Erik P. Rau, "The Adoption of Operations Research in the United States during World War II," in *Systems, Experts and Computers: The Systems Approach in Management and Engineering, World War II and After*, edited by Agatha C. Hughes and Thomas P. Hughes (Cambridge, Mass.: The MIT Press, 2000), pp. 57-92; Erik P. Rau, "Technological Systems, Expertise, and Policy Making: The British Origins of Operational Research," in *Technologies of Power: Essays in Honor of Thomas Parke Hughes and Agatha Chipley Hughes*, edited by Michael Thad Allen and Gabrielle Hecht (Cambridge, Mass.: The MIT Press, 2001), pp. 215-252 highlight the significance of OR to larger questions of the relationship between science and the state and the structure of the military-industrial complex. Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, Mass.: The MIT Press, 1996), chapter 4; and Andy Pickering, "Cyborg History and the World War II Regime," *Perspectives on Science* **3**: 1-48, both portray OR as the beginning of an alliance between science and a Cold War political, economic, and social order. Philip Mirowski, "Cyborg Agonistes: Economics Meets Operations Research in Mid-Century," *Social Studies of Science* **29** (1999): 685-718; and Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (New York: Cambridge University Press, 2002) explicitly follow in the tradition of Edwards and Pickering, and bifurcate OR into "British" and "American" intellectual traditions, and point to the mathematical theories of American OR as a key force in the Cold War social sciences, but Mirowski probably goes the farthest in relating OR to other intellectual trends, but there will be occasion to critique his approach here. M. Fortun and S. S. Schweber, "Scientists and the Legacy of World War II: The Case of Operations Research (OR)," *Social Studies of Science* **23** (1993): 595-642; and Robin E. Rider, "Operations Research and Game Theory: Early Connections," in *Toward a History of Game Theory*, edited by E. Roy Weintraub, 225-239 (Durham: Duke University Press, 1992) are useful early overviews of the various facets of OR's history.

related practices in technological research, development and design; mathematical theorization; and management. These are the things to which those most responsible for making decisions affecting OR's future were responding, and they must be understood at least relatively well before a coherent picture of the development of OR can emerge. There is still no complete map of this historical terrain, but the situation has been improving. Official and analytical histories of military operations research continue to be produced, and they have shown an increasing willingness to relate OR to other military practices. A substantial effort has been put into understanding the work of systems analysts, economists, and mathematicians at the RAND Corporation, an influential postwar contract research organization. Increasing effort has gone into understanding the history of other policy-oriented fields of theory, such as neoclassical economics, game theory, and rational choice theory.[3]

Yet, much more work remains to be done. More effort needs to be put into understanding the scientific activities relating to technological development and design, which historical accounts have traditionally subordinated to issues of university-based science.[4] We also need to elucidate the relationships and differences between high-level science advisors and the scientists who worked in policymaking sciences.[5] Finally, I will argue throughout this dissertation that historians interested in policy sciences must pay more attention to non-scientific practices such as military planning and industrial management, which have traditionally been tackled by entirely different sets of historians

[3] I will further elaborate on these areas in the historiographical survey below.
[4] I have found David Edgerton's critiques particularly enlightening. See especially David Edgerton, *Warfare State: Britain 1920-1970* (New York: Cambridge University Press, 2006), chapter 8 on "rethinking the relations of science, technology, industry, and war".
[5] Despite the attention paid to advisors, they have not been regarded as playing a separate role from operations researchers. When they worked in tandem, there was indeed little enough major difference, but it was not a given that advisors would have a research staff.

from those who have studied OR and related areas.[6] We will be able to fill in a few more holes here, but, even after we have finished, the picture will remain far from complete.

Even more important than new empirical work, however, is the need to develop a new analytical framework for the policy sciences. Because there is not yet a strong set of theories surrounding the subject, historians have tended to make due with a few ready ideas surrounding the relationship between science and politics. Some historians, for instance, have followed the line of Ted Porter's influential work on the use of quantification to establish political trust in order to suggest that wartime and postwar developments were part of a growing culture of competing experts who aimed to justify budget requests and legitimize policies with the authority of science.[7] Another popular framework has been established by Paul Edwards linking OR with Cold War politics, the rise of automated command-and-control systems, and the use of computer metaphors in academic fields such as cognitive psychology to suggest the importance of a "closed

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[6] The innovative rationality of industry, business, and management has been well-covered, but largely separately from histories of science and technology. Alfred D. Chandler, *The Visible Hand: The Managerial Revolution in American Business* (Cambridge, Mass.: The Belknap Press of Harvard University Press, 1977) is a cornerstone work. Christopher McKenna, *The World's Newest Profession: Management Consulting in the Twentieth Century* (New York: Cambridge University Press, 2006) is a recent and noteworthy contribution to the business history literature that overcomes the divide somewhat by tracing the history of management consulting to accounting and engineering. One notable case of the integration of the history of science and business practice is Stephen P. Waring, *Taylorism Transformed: Scientific Management Theory Since 1945* (Chapel Hill: University of North Carolina Press, 1991), which includes a chapter on OR. The practices of military planning have been largely disregarded, but see David Lowell Hay, "Bomber Businessmen: The Army Air Forces and the Rise of Statistical Control, 1940-1945," Ph.D. dissertation, University of Notre Dame, 1994; Robert Frank Futrell, *Ideas, Concepts, Doctrine: Basic Thinking in the United States Air Force*, 2 vols. (Maxwell Air Force Base: Air University Press, 1989); and I. B. Holley, Jr., *Technology and Military Doctrine: Essays on a Challenging Relationship* (Maxwell Air Force Base: Air University Press, 2004). Holley has previously written on OR: see I. B. Holley, Jr., "The Evolution of Operations Research and Its Impact on the Military Establishment; The Air Force Experience," in *Science, Technology, and Warfare, Proceedings of the Third Military History Symposium, USAF Academy* (Office of Air Force History, 1969).

[7] Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton: Princeton University Press, 1994). The general argument, of course, predates Porter, but his work is unquestionably instrumental in placing the role of quantification in public life on firmer theoretical and historical ground by focusing on the relationship in multiple locations over long time spans. The critical strategies used in Rau, "Combat Scientists", and S. M. Amadae, *Rationalizing Capitalist Democracy: The Cold War Origins of Rational Choice Liberalism* (Chicago: University of Chicago Press, 2003) follow in this general tradition.

world" discourse aimed at creating a global environment subject to technological and political mastery.[8]

I believe that these ideas, while useful, have their limitations and must be used with great caution. Most importantly, each illustrates how conflicts arise out of the shortcomings of the belief that complicated, innately political problems can be bounded and solved apolitically within a rational framework. There can be no doubt that such conflicts were quite real and were keenly felt, but I believe that the problems of codification to which authors such as Porter and Edwards point are inherent to all acts of policymaking, and I do not view the entanglement of scientists into them as a particularly notable event.[9] In fact, I would argue that studying the difficulties of the relationship between the social and the rational, between fact and value, and between science and non-science through case examples chosen to demonstrate these difficulties only serves to mask the ways these supposedly opposing cultures have actually managed their relationship in day to day practice. Scientists, and certainly, I argue, policy scientists were very much aware of the limitations of their methods, and did not need their critics, much less historians, to point them out.

What is most interesting to me is not the failure of science to resolve political problems, but the strategies employed by scientists and assorted policymakers to complement the weaknesses in each others' work with their own strengths, and to create more robust, more rational policies. Late in this dissertation, we will find Herman Kahn, one of the most famous and controversial policy scientists, suggesting that "no applied

[8] Paul Edwards, *The Closed World.* Mirowski, *Machine Dreams* follows Edwards' tradition.

[9] The historiography makes some issue of the "authority" of science, but I believe that the workings of this purported authority need to be mapped much more convincingly before we can take them seriously. In the conclusion to this dissertation, I offer an alternative view of the history of science and polity that deemphasizes Enlightenment notions of the political authority of science.

professional group is so intensely and continuously concerned with methodological and philosophical questions as Operations Analysts and Systems Analysts."[10] It is the central thesis of this dissertation that the historical trajectories of operations research and the policy sciences surrounding it were governed by just these "methodological and philosophical questions". Although they were never definitively articulated, I believe these questions can be distilled into two central issues. First, what does it mean to make a policy more rational? Second, how can an independent analyst contribute legitimately to the process? Operations researchers answered these questions not so much in words but in deeds: intellectual and institutional innovations that emphasized the complementarity of scientific and mathematical work to traditional policymaking methods.

I am arguing that OR was consciously built with not only the social but the intellectual legitimacy of the work of existing planners and managers firmly in mind. In so doing, I also imply a strong negative thesis that responds to some common notions. I will try and make clear that OR did not replace planners' and managers' work; it did not map a rigid rational framework onto more flexible humanistic practices; and it did not, as a rule, lend OR a scientific aura of authority.[11] Indeed, most policy science was not even conducted to justify budget recommendations or legitimize policies or otherwise seek to depoliticize an irrational policymaking process torn by politicized factions. OR was

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[10] Herman Kahn and Irwin Mann, "Ten Common Pitfalls," RAND document RM-1937, 7/17/1957, p. vii, obtainable free of charge from the RAND Corporation publications website. On Kahn's reputation see Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, Mass.: Harvard University Press, 2005).

[11] Much of the literature and criticism of OR is certainly borrowed from a deeper history of critique against Taylorism. See, obviously, Waring, *Taylorism Transformed.* There are a number of critical works on Taylorism, but see especially Charles S. Maier, "Between Taylorism and Technocracy: European Ideologies and the Vision of Industrial Productivity in the 1920s," *Journal of Contemporary History* 5 (1970): 27-61, and the literature he cites, especially in note 4.

designed to serve and augment the *rational* work of policymakers who were willing to be persuaded of a best course of action. Its legitimacy rested not in some inherent authority in science, but on the rather solid political legitimacy of the policymaking process itself, and its survival depended on its successful integration into it.[12] I will not, of course, claim that OR was never or even rarely used in attempts to rationalize or depoliticize policy, but I do believe that casting OR as a seat of scientific rationality in opposition to irrational acts of policymaking will cripple any historical analysis of its ongoing intellectual development.

Rationality and Arbitrariness

Before proceeding with an outline of OR's intellectual and institutional innovations, we should be clear at this point what policy scientists meant (and what we mean) by rationality in the context of their work. Judging on the basis of my research, rationality does *not* seem to have implied a universally objective perspective that was free of human values and validated as truth by the use of scientific method. Policy scientists considered a rational policy to be a policy that accomplished its value-laden ends in what could be agreed to be the most effective way. Under no circumstances was the seat of a policy's rationality considered to be within any mathematical model. A policy that was based on a model but that did not serve its purpose effectively could not be considered a rational policy. This perspective on rationality, I will argue, was fostered among OR scientists during World War II.[13] Their wartime experience made them acutely aware

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[12] To put this claim another way, I am arguing contrary to the idea that there was a perceived need to bolster the weak authority of policymakers via the authority of science.

[13] Entirely separate motivations involving the clarification of economic concepts drove certain kinds of mathematical economists who later became associated with OR. We will not deal extensively with the

that they were contributing—but only contributing—to policies that could save or cost lives, and that could potentially impact the overall course of the war, and it gave them a keen sense of their responsibility to make only positive contributions to existing policymaking procedures, and not to interfere if they felt they could not play a constructive role in them.[14] Thus, their standard of rational action was not whether a policy was quantitatively rationalized, but whether it made sense in light of existing policy-oriented knowledge.

Policy knowledge (as I will call it) consists of the sum total of factors to be taken into account when formulating a policy as of the moment when the policy must be made, and it can be based on an amalgam of well-established facts, expert opinions, estimates, guesses, hearsay, uncertain forecasts, speculation, superstitions, and stereotypes. Policy knowledge is validated the moment it becomes the basis for action, not out of some sense that it is correct, but out of the agreement that no clearly better basis for action could be said to have been available. Not all policy knowledge is quantifiable, at least not rigorously, but all policy knowledge, by definition, bears in some tangible way on the *rationale* underlying any given policy. This rationale can be implicit or explicit, but it is always present.

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work of economists in this dissertation, although their work certainly provides an interesting comparison. See Arrow, "Mathematical Models," and Kenneth J. Arrow, "Decision Theory and Operations Research," *Operations Research* **5** (1957): 765-774. For a critique of Arrow's and other economists' perspective in the science studies tradition see Philip Mirowski, *Machine Dreams*, especially chapters five and six. For a critique of Mirowski's critical strategy, see Appendix A in this dissertation.

[14] The British physicist Patrick Blackett, probably the most important spokesman for OR during the war, was adamant that operations researchers should stay silent if they could not contribute, and invoked a standard of responsibility to test whether they should speak out: if the decision was theirs to make, would they, themselves, follow their own advice? See P. M. S. Blackett, "Operational Research," *Operational Research Quarterly* **1** (1950): 3-6, esp. p. 6; reprinted in P. M. S. Blackett, *Studies of War: Nuclear and Conventional* (New York: Hill and Wang, 1962), pp. 199-204 as "The Scope of Operational Research".

For our purposes, we can identify three different but related kinds of rationales. First, a *technical rationale* governs a physical action, whether a human or machine action. A technical rationale can be hidden in a trained skill, such as hitting a baseball, or it can be made explicit in a diagram, in a mathematical formulation, or in the construction of a piece of machinery. Although effective technical rationales can always be developed intuitively, and thus implicitly, they can be more effectively replicated, modified, or reified into machinery if rendered verbally, and, especially, mathematically. In World War II, the technical rationales behind aiming and firing weapons became a lively subject of mathematical inquiry as it became increasingly clear that technical design, developing techniques for using technology, and evaluating the requirements for new technologies and techniques had to become increasingly well coordinated processes in order to design effective technologies and to establish effective training regimens for overcoming dangerous and agile opponents.[15] OR played a supporting role in this process of coordination throughout the war.

The second kind of rationale is a *codified rationale*, which is at least partially explicit, often in the form of a printed law or rule. However, because codified rationales can blur into technical rationales, I reserve the term policy to refer to actions built on both. Would, for instance, an order to gunners not to fire once an enemy airplane had escaped to a certain range constitute a codified policy or a technical evaluation of the chances of scoring a hit beyond that range measured against the value of ammunition supplies? Rather than split hairs, I will simply use the term "policy" loosely. Clearly, while codified policies are primarily qualitative in nature, they can entail quantitative elements,

[15] See David Mindell, *Between Human and Machine: Feedback, Control, and Computing Before Cybernetics* (Baltimore: Johns Hopkins University Press, 2002).

such as a range of conditions under which the policy is considered valid. The problem of evaluating the rationality of a codified policy is in whether or not it makes the most sense to follow that policy or an alternative policy given available policy knowledge. Even if the relative values of alternative policy options are not strictly expressible, best choices are frequently obvious, or a choice may be made arbitrarily from a number of good choices. Even still, OR, and particularly military OR, has always operated under the presumption that expanding on policy knowledge, or expressing, exploring, and testing the rationales implicit within codified policies to see if they make sense will result in superior policy decisions.

Finally, there are *mathematical rationales*, which are, by definition, completely explicit. Following in the tradition of classical logic, a mathematical rationale consists of a set of carefully stated axioms from which conclusions can be drawn through calculation. While both technical and codified rationales can be translated into mathematical form, neither typically translates perfectly, and approximations must almost always be used. There is, therefore, a danger that some crucial nuance will be lost—but this danger is, of course, present in the process of training intuitions, building machines, and codifying rules as well. Frequently the translation of a technical or codified policy into a mathematical form can prove useful because of the relative ease in manipulating the logic of a policy within that form. Thus, the act of translation must, itself, be subject to *meta-calculative* policies governing whether or not a translation into mathematical language would prove worthwhile. Machines and policies can always be built without mathematics, but mathematics can in many cases allow them to be built with greater sophistication, and, beginning in the 1950s, this is the principle on which the bulk of OR was based.[16]

In the policy sciences it is best to think of any kind of rationality as in opposition to arbitrariness, and not intuition, or emotion, or even irrationality.[17] While rationality implies the existence of a rationale—some reason, even if left tacit, that governs action—arbitrariness implies the absence of any rationale: action governed by the throwing of a dart or the roll of a die. The opposition is especially useful to us because it invokes similar connotations in discussions of either policy or science.[18] However, every policy is an amalgamation of rational and arbitrary elements. No policy can be said to be completely rational, and few policies, even ones seemingly based completely on intuition, can be said to be completely arbitrary.

The real challenge comes in determining in what ways a policy is rational, and in what ways it is arbitrary. It is not easy to divide a policy up into its constituent components, let alone determine which component might be said to be rational and which component might be said to be arbitrary. Take the case of estimation. An estimated figure might be highly arbitrary within a certain range, say in deciding how many employees to hire for a new store: why hire 25 rather than 30? But, ultimately the figure is at least partially rational: why hire 25 rather than 250, or three? At what point does the

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[16] I will further discuss this issue and formally introduce the concept of meta-calculative concerns in chapter four.

[17] Policy scientists, of course, frequently spoke of rationality in opposition to emotional or intuitive solutions to problems. The implication was not that policies should be devoid of emotion or intuition but that they should be supported by facts whenever possible. They understood that once analysis had contributed all it could, more arbitrary, intuition-based guesses became permissible to fill in the remainder of knowledge necessary to make a policy. See, for example, the many discussions of the problem in E. S. Quade, ed., *Analysis for Military Decisions* (Chicago: Rand McNally, 1964).

[18] In discussions of policy arbitrary authority can imply corruption, i.e. an action taken for personal rather than public reasons, and therefore can be said to have a rationale, but the point is that the policy lacks a legitimate *governing* rationale.

estimate stop being arbitrary and become a rational choice? The answer to these kinds of questions depends on how much and what kind of information one has on hand, how reliable one considers it to be (another meta-calculative contest between the rational and the arbitrary), and how rigorously one has analyzed it.

The connection between correlation and causation is another important site for the intermingling of arbitrariness and rationality. One might institute a policy with the expectation that it will work, at least to a certain degree of effectiveness, without really being sure of *how* it will work. The Federal Reserve Bank, for instance, makes decisions on prime interest rates that are intended to affect inflation using only a working knowledge of the underlying mechanisms that cause fluctuations in price. Similarly, one can be well-trained in a technique, such as target shooting or hitting a baseball, without a thorough understanding the physics of the process. Without such an understanding one may still enjoy more success than someone completely naïve in the matter, but with only limited understanding one will not have a good sense over whether, how, and to what extent one's policies and techniques are improvable, and one might also fall victim to unsuspected changes in conditions that threaten the validity of one's knowledge. To move beyond these restrictions, some theory, however crude, is necessary.

Additionally, one can intertwine arbitrariness and rationality by discounting the unknown and the unknowable. When one makes a plan, one cannot know all of the various things that might go wrong with the plan, whether some element of the plan will unexpectedly break down, and what the effect of such a breakdown would be on the effectiveness of the plan. One cannot evaluate the possibility of some unexpected externality (an "act of God" in insurance parlance), but one *must* rest relatively well assured that over a finite length of time the risk of such externalities is finite. Similarly, one cannot know every unexpected consequence one's actions might have. Above all, one cannot anticipate the distant future. It is useless to make all but the vaguest plans forty years in advance. One always arbitrarily excludes certain possibilities from one's considerations in the expectation that they will not have a major bearing on one's final policies and actions.

All arbitrariness is, at root, a function of ignorance and indifference. Once the moment of decision has arrived and all available policy knowledge has been considered, and inferior choices have been eliminated, one makes a final choice based on some combination of not having any information suggesting a preference for one choice over another, and on a decision that it does not ultimately matter what choice one makes. In choosing between two used cars, one must admit that one does not know which is more likely to be a maintenance hassle; or perhaps one ultimately must decide that there is no correct choice between two different but equally attractive car models. Either way, at some point, randomness ultimately determines final policy choice.[19]

Given the number of ways there are to intertwine rationality and arbitrariness, it should not be surprising that there are a number of ways to push back the boundaries of arbitrary choice. Reducing these to two major categories, the most immediate way of doing so is by making the knowledge and rationale behind a policy as explicit as possible to test whether the rationale follows from the available knowledge: a simple list of pros

[19] Readers attuned to behavioral theory will doubtless recognize elements of Herbert Simon's theory of "bounded rationality" here. Others arguing from a stronger "rational actor" standpoint would consider the decision optimal, because the costs of obtaining further information would be deemed in excess of the expected costs of making the final arbitrary decision. There is a case to be made the some final decision will ultimately be rationalized, but I am not familiar with the literature on preference creation.

and cons might suffice.[20] Alternatively, one could also attempt to expand one's knowledge by identifying areas where a rationale is built on arbitrary foundations and investigating unknown or poorly understood facets of policy knowledge to learn more about them, and to see whether the policy holds up under additional knowledge. A decision to market a product, for instance, is always stronger if preceded by market research to confirm one's suspicion that there will be demand for it. Both ways of improving rationality are kinds of scholarship—attempts to understand the logic of a policy, to see how it is adequate, damaging, optimal, or suboptimal. I am arguing that operations research, in all of its forms, was built on and around lay attempts to build just such scholarship, and that it represented a study and augmentation rather than a replacement of this scholarship.

Overview

From the foundation of operations research as a concept until the early 1950s, when OR began to adopt its own mathematical canon, OR practice almost always revolved around existing policymaking mechanisms. In this period, except in cases where such rationales had already been applied to the technical problems of equipment design by engineers, OR scientists rarely deployed any mathematical rationales of their own. When OR's proponents referred to it as a deployment of scientific method, what they meant was the examination of equipment designs, of the practices governing equipment use, and of more general plans and policies, in order to see whether they made sense in light of available or obtainable knowledge about operations. In World War II,

[20] A note of trivia: the Johns Hopkins University Operations Research Office, a contractor for the United States Army between 1948 and 1961, actually used a balance scale weighing the words "pro" and "con" as its logo. Pro was heavier.

this sort of work was frequently done by the military, but it was also found that scientists could contribute quite fruitfully to it, thus giving rise to the idea that operations research was identifiable as a scientific activity.[21]

In the first two chapters of this dissertation, we will examine these wartime origins of OR. In chapter one, we will look at the most common work of wartime operations researchers: the performance of investigations on behalf of military planners. The object of the chapter will be to show how OR was intellectually related to the codified practices of the military—doctrines, procedures, and plans—and how it largely consisted of gathering and sorting data to test the implicit or explicit assumptions governing these plans. Operations researchers had the enviable luxury of leaving the arbitrary aspects of decisions to the planners while concentrating solely on ways of making plans better informed. In chapter two, we examine OR from the perspective of design engineers. Equipment designers make choices about design based on their understanding of how their own craft abilities can best meet the requirements of the users of their equipment. While the process of design had long been highly mathematical, increasingly mathematical formulations of design *requirements* aided designers in choosing equipment configurations most appropriate for their end users' goals, not just ones that would be expected to perform well on arbitrary tests. OR studies helped designers coordinate their efforts with the efforts of training facilities and military tacticians. However, like military planners—but unlike operations researchers—equipment designers *had* to make choices about equipment design, and therefore had to acknowledge the arbitrariness of their choices in order to minimize its use in them. Thus,

[21] For a discussion of how early leaders in OR related their wartime work to their previous experience in scientific research, see William Thomas, "The Heuristics of War: Scientific Method and the Founders of Operations Research," *BJHS* **40**/2 (2007): 1-24.

during the war, OR established its legitimacy by acting purely as an advisory complement to ordinary policy *and* design activities, eliminating arbitrariness from their plans, all the while deftly avoiding the generation of new arbitrary elements as much as possible.[22]

Chapter three will deal with how the original wartime vision of military OR was adapted into a postwar environment. Throughout World War II, OR had made both military planning and equipment design more rational and less arbitrary by connecting them more closely with the realities of actual operations. After the war (except in cases such as the war in Korea), there was no longer any operational reality to research, and war planning became more conceptual. OR changed in two main ways to meet this shift in working conditions. First, operations researchers became primarily responsible for studying doctrines, practices, and policies to make certain that the concepts and plans being developed concurred with the *tentative* information available from past war experience, intelligence reports, equipment tests, and training exercises, without knowing for certain how well this knowledge would translate to actual combat. Second, because the military also had its own means of policy development, OR began to be consistently identified as an independent, civilian practice that supplemented these means. However, it still focused on *improving* existing doctrines and practices rather than establishing full-scale policy recommendations, and therefore still did not have to take much responsibility for *introducing* arbitrary elements into the policymaking process, even though it now dealt more clearly in them.

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[22] This perspective is in direct contrast to Philip Mirowski's argument that OR's claims to legitimacy were based on their claim to use a generic "scientific method" which guaranteed rationality; see Philip Mirowski, "The Scientific Dimensions of Social Knowledge and Their Distant Echoes in 20th-century American Philosophy of Science," *Studies in the History and Philosophy of Science* **35** (2004): 283-326, p. 300

Meanwhile, *other* civilian analysts, especially at the Air Force's new contractor, the RAND Corporation, took up the problem of coordinating technical design with operational requirements. In the absence of war experience and OR reports from the field, RAND developed systems analysis, a technique initially aimed at mathematically determining which equipment designs made the most sense in light of what was considered to be the best intelligence, tests, and professional expectations of future combat conditions then available. Systems analysis thus began as something that took place in the *absence* of combat OR, even as it became more closely associated with it on account of its handling of tentative forms of policy knowledge as well as its identity as a form of analysis performed by independent civilians. Yet, by producing full designs complete with rational and arbitrary components, RAND systems analyses *replicated* rather than *supplemented* ordinary design practices, and thus had more difficulty than OR in finding a legitimate place in the military's existing policymaking mechanisms.

Chapters four and five deal with the full-scale introduction of mathematical models into the analysis of codified rationales. Military-style OR, I argue, had no clear place in business and industry, given both the high level of commercial, legal, financial, and technical expertise informing business activity and the existence of a well-developed management consulting profession, which was already established as the source of independent quantitative analyses of overall business policies and practices. So, proponents for the expansion of OR had to find some means of making a novel contribution. This task was largely accomplished through the adoption of a mathematical canon. However, I hasten to preempt the possible argument that the development of this canon represented a conscious attempt by operations researchers to render their advice "scientific", and, hence, credible. Such a notion of science was not consonant with OR's proponents' war-fostered concept of scientific method. Instead, I argue that just as the epistemology of wartime OR came largely from planning and technology design, so this new development came primarily from mathematicians and statisticians outside the established wartime OR coterie.

In chapter four I examine how certain groups of mathematical and statistical theorists came to associate their work with OR. Rather than focus on the exploration and investigation of real policies, theorists who began to associate themselves with OR instead focused on rigorous formal examinations of what it meant to act rationally given certain arbitrary assumptions. They asked such questions as, given a production lot of goods and a tolerance for product failure, when could one stop a statistical quality control test and be assured the lot will pass muster? Given a schedule of prices for moving goods from one set of points to another, what distribution policies yielded the lowest cost? Focusing on these kinds of questions represented a radical shift in the practice of OR. Suddenly OR began examining what *constitutes* a rational decision rather than simply trying to make decisions more rational. When using formal models, operations researchers deployed *distinct* mathematical rationales *alongside* codified rationales rather than working largely within the codified framework, much as systems analysis took place *alongside* existing design work.[23]

While this shift tended to separate OR somewhat from the act of policymaking, it also offered operations researchers a powerful new set of tools that they could use to increase the effectiveness of important business operations such as inventory

[23] I point out that there were wartime precedents for this shift, particularly the development of "search theory", but also hasten to add that search theory was more of an exception to wartime practice rather than an indicator of where the field was going.

management and production and distribution scheduling beyond the abilities of previous generations of managers, consultants, and cost accountants. It was through these methods that OR managed to find a stable niche within the industrial community. At the same time, on account of their unique logical structures, these same sorts of problems proved enticing to certain mathematical theorists in the social sciences who sought to address the fundamental nature of rationality. Areas as seemingly mundane as production and inventory control became a substantial research interest for social theorists of no less stature than Kenneth Arrow and Herbert Simon, both of whom would go on to claim Nobel Prizes in Economics.

However, the methodological distance that new modeling methods created between codifying managers and mathematical policy scientists ran against the grain of OR proponents' initial dedication to making *actual* policies more rational. As I explained in the previous section, if a mathematical model were applied to an actual policy arbitrarily, it would have the effect of making the policy less rational, not more. In chapter five, I show how throughout the 1950s operations researchers and systems analysts became increasingly concerned with retaining the wartime conception of OR while embracing OR's useful new methodology. They emphasized how important it was not to limit OR activity to the deployment of a restricted set of techniques, and how important it was when deploying models to do so in ways that lent new insights to existing policies without detracting from their robustness. I will explore their professional strategies by examining four cases in the new profession's stabilization.

The first case shows how consultants at the Arthur D. Little firm used their status as consultants to integrate OR methods into a larger set of consulting practices, emphasizing the management consulting profession's longstanding reputation for successfully integrating their work with the specific needs of different companies. The second case shows how a pedagogy of OR evolved at the Massachusetts Institute of Technology as a direct descendant of military OR, continuously stressing the need to direct efforts at policymakers' real problems, while slowly integrating new techniques into their curriculum. Particular attention will be paid to the uncertainty of the relationship between OR's proponents and the activities of their then-new management school. The third case follows philosophers of science West Churchman and Russell Ackoff as they adopted OR as a practical expression of their "experimentalist" philosophy, which committed them to developing OR as a science *of* management rather than simply a science *in aid* of management.. The fourth case shows how systems analysis at the RAND Corporation evolved from a design-oriented technique into a policy-oriented discipline with close links to OR. Because design decisions were not made at one moment, as initial formulations of systems analysis supposed, but evolved with changes in policy, systems analysis began to focus on analyses of policies that would prove robust under diverse arrays of assumptions, and would thus prove more useful to the evolving work of actual policymakers.

Chapter six, the final chapter of this dissertation, turns back the clock to the origins of "operational research" in Great Britain. The origin story of OR has been told quite competently many times before, and I have not felt it necessary to tell it one more time to introduce the history of OR. We come to it only at the end in order to demonstrate that there are other ways of telling it than those we have inherited from those scientists who originally established its terms. From their perspective, OR fit into what I call an *oppositional view* between scientists and policymakers. This view originated in these scientists' discontent with the way science was used by British state and society, and their belief that the situation could only be allayed if scientists were more effectively incorporated into policymaking structures. Operational research thus came to be seen as one step in bringing scientific expertise into the problems of military planning. However, I argue, OR does not comfortably fit into such a narrative. OR was always more of a means of facilitating understanding between military field personnel, military planners, their science advisors, and the British state research and development establishments. It enhanced the rationality that already existed in planning, and it did not represent a victory for scientists in some sort of struggle to introduce science to policymaking circles. OR's British proponents knew this. Their actions reflected it. But, as I show, their way of speaking about it, informed by their political goals, almost always led to misunderstanding. If we are to understand their work, we must move beyond their most immediate concerns and begin to assert new ones of our own.

Methodology: Beyond the Oppositional View

Indeed, the historian David Edgerton has criticized the early British OR proponents for being "anti-historians". He has described how they have systematically distorted the historical picture of the science-state relationship in Britain, feeding into a much larger "inverted Whig" narrative that continuously seeks reasons why the British state failed to take appropriate advantage of science and technology, thereby ushering in a century of national decline as Britain transformed into a technologically unsophisticated welfare state. Edgerton argues that, contrary to this picture, science and technology played a significant and increasing role in the work of the British state, most especially within the military, and that the relative decline of Britain next to the two postwar superpowers requires no special explanation with respect to that nation's commitment to science and technology.[24]

Edgerton's concept of "inverted Whig" history is important to the argumentative strategy I will deploy here. Inverted Whig arguments recall case examples that question the validity of more optimistic narratives, in spite of strong evidence that such examples do not represent the most typical or the most important historical trends. In the context of Great Britain, according to Edgerton, inverted Whig history

> dredges up any old example of a civil servant being ignorant of technology, a businessman not investing in a modern machine, or a soldier doubting the efficacy of new weapons. The 'inverted Whig' historians of science and technology have been at work for about a hundred years; their prodigious industry has produced an impressive pile of horrors.[25]

Historians of American science and technology have been similarly industrious, but with a striking twist: here the "pile of horrors" deals not with the *separation* of science and technology from government, but the *alliance* between them. There is a vast collection of instances where scientists appeared confident that they had the key to making good policy, where a high profile "scientific" policy failed, or where scientifically backed policies worked in favor of certain interested parties and against others.[26]

The American inverted Whig history is closely associated with the aforementioned historical narrative emphasizing the "limitations" of the abilities of science to offer "rational" or otherwise apolitical or objective solutions to policy

[24] See David Edgerton, *England and the Aeroplane: An Essay on a Militant and Technological Nation* (Basingstoke: Macmillan, 1991); D. E. H. Edgerton, *Science, Technology, and the British Industrial "Decline", 1870-1970* (New York: Cambridge University Press, 1996); and Edgerton, *Warfare State.*
[25] Edgerton, *Aeroplane*, p. xvi.
[26] This strategy is central to the argument in Edwards, *Closed World.*

problems in the postwar period. According to the narrative, in the two decades following World War II, a time when the progressive nature of technology and the correctness of American political values went unquestioned, scientists came to forge an alliance with American policymakers, giving rise to a strong "expert" culture. This culture reached its zenith with the appointment of Robert McNamara as Secretary of Defense in 1961, and his installation of a number of RAND analysts at the Pentagon, who were often referred to as the "Whiz Kids", connoting intelligence and faith in rationality, but also arrogant inexperience. The narrative continues that the limitations in the science-politics relationship became apparent as rationalized and technology-based policies failed to solve the problems of the Vietnam War and urban unrest and decay. The authority of science thus diminished as policy failures mounted and competing sets of experts began advocating for different interests.[27]

The curious duality where in the British case we are presented with a cautionary history focusing on the disregarded *possibilities* of science, and in the American case we are presented with a cautionary history focusing on the *limitations* of science cries out for some explanation. Was it really that America and Britain both got science and technology wrong in precisely the opposite ways? Contrary to this odd asymmetry, I believe it is a result of a failure to achieve historiographical maturity with respect to

[27] There are any number of rehearsals of this narrative, which I will not attempt to list exhaustively here. Yaron Ezrahi, *The Descent of Icarus: Science and the Transformation of Contemporary Democracy* (Cambridge, Mass.: Harvard University Press, 1990) is particularly rigorous theoretical discussion of it. A recent example is Howard P. Segal, "Progress and Its Discontents: Postwar Science and Technology Policy," in *The Social Sciences Go to Washington: The Politics of Knowledge in the Postmodern Age*, edited by Hamilton Cravens, (New Brunswick: Rutgers University Press, 2004), pp. 67-77. On the creation of competing sets of experts, see Brian Balogh, "Reorganizing the Organizational Synthesis: Federal-Professional Relations in Modern America," *Studies in American Political Development* 5 (1991): 119-172; and Brian Balogh, *Chain Reaction: Expert Debate and Public Participation in American Commercial Nuclear Power* (New York: Cambridge University Press, 1991). Rau, "Combat Scientists," is presented as a direct extrapolation of Balogh's theoretical framework.

matters of the relationship between science, technology, and policy. In *both* British and American cases we are pressed to embrace what I am calling the oppositional view and to examine the destructive frictions laying along the divide between science and non-science.[28] Furthermore, the *way* we are supposed to examine these frictions seems to have something to do with historiographical expectations of the kinds of stories we should be telling about the decline of one great power and the rise of another. Exit Britain, workshop of the world, ruler of the ocean-sea. Enter America, overconfident, militaristic Cold Warrior. It becomes apparent that what we are following is more of a morality play than honest history.

The real danger in repeating these inverted Whig arguments is not that they are in error, *per se*. Frictions between scientists and policymakers *did* exist, and they did have important consequences. Indeed, as I am arguing, OR was built around conscious attempts to minimize these frictions. Perhaps we should read it as a sign of the policy sciences' many successes that critical and historiographical attention to their limits arose out of a real need to provide a counterweight to triumphant narratives about them, and a need to recognize those who have been systematically excluded from their benefits or have been harmed by them.[29] But this task was admirably accomplished decades ago. Once historiographical attention turns so overwhelmingly toward these themes that they become clichés, they can easily distract our attention from other, often contradictory, but potentially more revealing histories. We begin to think we know all we need about the

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[28] The oppositional view is closely related to C. P. Snow's notion of the "two cultures"; for a pertinent critique of Snow, see Edgerton, *Warfare State*, chapter five, and especially pp. 196-202.

[29] Of course, the claims of experts were consistently criticized even as they arose. Therefore, the historiographical construction of the dominance of the triumphant narrative may actually require some illumination.

mundane, we allow familiar stereotypes about the mainstream to remain, unquestioned, and we may even begin to confuse the marginal for the typical.[30]

I am, of course, not arguing for a remarginalization of the marginal and a restoration of the triumphant narrative. I am arguing for a transatlantic history that recognizes differences between Britain and America, but does not deal in the stark comparisons implied by the historiography. More generally, I am arguing for deeper understanding. If we follow the inverted Whig historiography, we are inevitably led to ask the wrong questions. As Edgerton has pointed out, British scientific, technological, and industrial output all increased dramatically throughout the twentieth century. Similarly, experts continued to inform American business, government and military strategies to an even greater degree after they were supposedly discredited. These points might seem obvious enough, but within the inverted Whig historiography they suddenly require explanation—how could these scientific cultures survive their seemingly fatal flaws? It is at this point that we begin to make a fetish of things like Harold Wilson's invocation of the "white heat" of science and technology in the 1964 national elections in Britain, or the rise of participatory planning, as we seek out the moment when attitudes began to change, and everyone began to realize that the previous way of doing things was not working. Really, though, it is at this point that we should begin to realize we really know very little about the ways R&D and expert cultures worked. Beyond the frictions, policies were made, and each of these policies had its own history, a set of reasons why it

[30] In many cases the typical *is* the marginal and it is ignored because it is not controversial; on the history and historiography of technology, see David Edgerton, *Shock of the Old: Technology and Global History Since 1900* (New York: Ocford University Press, 2007).

was or was not (apparently) successful, and that these reasons probably almost never had anything to do with the general participation (or lack thereof) of science.[31]

In this dissertation, we are not interested in operations research so as to evaluate its significance as a scientific contribution to the military or to industry. I would venture to say that it was important, but not especially revolutionary. As I have suggested, most of its methods were imported from military planning, from equipment design, and from applied mathematics and statistics. My interest in the intellectual trajectory of OR is that between 1940 and 1960 OR was at the center of a remarkably active marketplace of ideas about policymaking. Following OR's trajectory leads us to *other* histories of which it was a part: histories about military planning, R&D and technology design, management practices, developments in applied mathematics, economics, and the postwar social sciences. Ironically, if we are careful enough, studying OR actually helps us to look beyond the participation of new kinds of policy experts, and to attain a new understanding of how policies came to be developed, and the various strategies used to improve the policymaking process.

The historiographical methodology that I employ is designed specifically toward the goal of achieving just such an understanding, and as a reaction to prior methodologies that, I believe, have tended to detract from it. At this point it will be useful to outline

[31] On Wilson's "white heat" and standard attitudes toward it, see Edgerton, *Warfare State*, chapter six; On public participation, see, for example, Hughes, *Rescuing Prometheus* in which Hughes hopefully discusses the onset of a "postmodern" form of expertise, and uses Boston's Central Artery and Tunnel ("Big Dig") project as a key example. He was writing before it had become clear that the project had been an administrative disaster. Balogh, *Chain Reaction* presents a more nuanced view of the relationship between experts and public participation. Jennifer Light has made a valuable contribution in showing how public participation suffers from the same defects of elitism as other forms of expert planning; see Jennifer Light, *From Warfare To Welfare: Defense Intellectuals and Urban Problems in Cold War America* (Baltimore: The Johns Hopkins University Press, 2003), chapter 7 on the failure of the proposed use of cable television to increase community participation in disadvantaged neighborhoods. Policies, we should realize, are inevitably creations of an elite.

some of the existing historiography to OR, to highlight what we can learn from it, and what new directions we might take. We will begin with some of the most notable recent contributions to the history of OR itself. The recent work of the military historian Charles Shrader shows that the genre of the "official history" of OR continues to attract some attention, and that it still has things to tell us about OR.[32] Meanwhile, the economic historian Maurice Kirby is researching the history of OR in Britain on behalf of that country's Operational Research Society. His work provides especially useful information on the spread of OR in both industry and in Britain, and is essential reading on the size and scope of OR; a topic I do not treat here as extensively as one might.[33] Some of the most interesting analytical work on OR is being done by the historian of technology Erik Rau, who is interested in OR mostly as a bellwether of the relationship between scientists and their patrons, and shows how OR was one instance of the subversion of their separation during the war and in the postwar period. His work on the establishment of OR in the military services is indispensable, and it is another subject treated only briefly here.[34] However, none of the above mentioned histories carefully relates the intellectual content of OR to ordinary planning and management practice. Shrader seems to recognize that military OR practitioners' methods were often based on equipment design and testing principles, although he has an unfortunate predilection for reading the history backwards, suggesting that OR methods were "embedded" in the work of military design and testing facilities before the scientists at these facilities understood they were doing

[32] Shrader, *Operations Research.* Other major installations in the historiography of military OR include Keith R. Tidman, *The Operations Evaluation Group: A History of Naval Operations Analysis* (Annapolis: Naval Institute Press, 1984); Air Ministry, *The Origins and Development of Operational Research in the Royal Air Force* (London: HMSO, 1963); and C. H. Waddington, *OR in World War 2: Operational Research against the U-boat* (London: Elek Science, 1973).
[33] Kirby, *Operational Research*; also see Maurice W. Kirby, "Operations Research Trajectories: The Anglo-American Experience from the 1940s to the 1990s," *Operations Research* **48** (2000): 661-670.
[34] Rau, "Combat Scientists"; Rau, "British Origins"; and Rau, "Adoption".

OR.[35] Both Kirby and Rau seem to address OR as a history of the spread of scientific methodology in policymaking environments, without paying much attention to how OR related intellectually to the act of policymaking.[36]

This dissertation adds to these prior approaches primarily by concentrating almost as strongly on the practices surrounding OR as on OR itself. A sizeable portion of chapter one is dedicated to sketching the way the military builds its policies. In chapter two OR hardly appears at all. It is always something just over the horizon to which the other scientists and mathematicians related their own activities. Substantial portions of chapters three and five are dedicated to the development of systems analysis, which grew out of some of the mathematical design work detailed in chapter two. It was, I argue, the middle of the 1950s before systems analysis became tightly connected to OR, contrary to a general historiographical agreement that it grew out of wartime OR.[37] In chapter four, the emphasis shifts to applied mathematicians and economists whose work eventually came under the OR rubric, but most of the examples I use describe work accomplished with something other than OR in mind. Chapter six deals primarily with wartime scientific advisors and elite scientist spokesman, and discusses how OR did and did not relate to their wartime work and their postwar rhetoric.

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[35] Shrader, *Operations Research*, p. 63 for instance.

[36] Kirby, for example, focuses almost exclusively on the spread of the "techniques" of operational research, even before the idea of an OR technique had meaning. It is important to realize that Kirby does not concern himself with the essential point that from 1945 to about 1960 in Britain, the term OR was treated as an umbrella term for any modernizing approach to business, and so it is misleading to trace the history of OR there without tracing the history of things such as statistical quality control and cost accounting, which had a much broader basis of support. Although Rau's histories of the development of OR are quite thorough, theoretically he is more interested in concerns about the intellectual dependence of OR work on the political concerns of their patrons.

[37] This story was first told by systems analysts themselves, but has little to do with the intellectual trajectory of systems analysis, and seems to rely on the notion that RAND itself was based on wartime OR, which it was not, except in perhaps a rather loose way, as we will see in chapter three, and as Martin J. Collins, *Cold War Laboratory: RAND, the Air Force, and the American State, 1945-1950* (Washington, DC: Smithsonian Institution Press, 2002) should indicate fairly clearly.

The connections I draw between OR, the other policy sciences, and policymakers cannot be easily discerned in the published literature, and so archival records have provided a valuable supplement. Although they are available, not only have original OR reports never been examined in any meaningful way, they can be compared and contrasted with the archived work of others who did not fall under the OR rubric.[38] I have paid particular attention to the work of military tactical and strategic planning committees, tactical "development units" (which have been recognized, but never adequately described), and non-OR civilian mathematical analysis groups, which are beginning to become better understood, though they have often been ignored or mistaken for OR groups. It has only been through my work in these archives that I have felt confident in describing how operations researchers, other scientists, and policymakers understood their epistemology and political legitimacy in terms of their relationships to each other.

However, I should note at this point that I have not attempted to obtain records of industrial OR practices, and that my discussion of actual industrial OR practice is limited to a brief discussion of OR activities at the Arthur D. Little consulting firm, which I obtained through published sources and an interview with John Magee, a central figure in the establishment of OR there. Of course, many case studies of industrial OR were published, especially in the British professional OR journal, but a journal specifically

[38] On military issues, security issues can be a nuisance in obtaining records, but many formerly classified wartime and postwar documents are accessible. American wartime OR reports do remain difficult to come by. Some may be found in the National Archives, and Erik Rau is responsible for negotiating the release of many papers from the private CNA Corporation. Postwar navy reports remain largely inaccessible, but a large number of postwar Air Force and Army reports can be found in the National Archives. British wartime and postwar reports are easily accessible at the National Archives of the UK. Correspondence regarding the *use* of OR is much less common, although records relating to the U. S. Army's Operations Research Office located in the United States National Archives proved enlightening. See Appendix B for an overview of archival sources consulted.

dedicated to case studies did not appear until the late 1960s, following an era of theory dominance in American OR journals, especially.[39] However, it seems naïve to assume that these case studies represented typical industrial OR work. It strikes me as much more likely that they were published precisely because there were deemed somehow methodologically noteworthy. Further, the typical historiographical problem of relating actual practice to its published description no doubt afflicts OR journals as much as it does other scientific journals. Thus, unlike with military work, it is still difficult to say what sorts of practices constituted typical industrial OR work in the period in question. In fact, it is possible that I am mistaken in suggesting that industrial work was largely model-based, and it may be that it was more similar to military work than I suppose. Further study in this direction would be quite valuable.

Concerning model-building, archival work on World War II-era mathematical models from the aforementioned mathematical analysis groups has shed considerable light on the later work of the RAND Corporation, even though RAND's somewhat limited collections on its early years have by now been well-examined.[40] Despite the surfeit of attention given to RAND, new analytical frameworks, including the ones I deploy here, should help historians read RAND documents and archival holdings in new ways, even though substantial amounts of new RAND material is not likely to surface. I

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[39] The journal *Interfaces*, initially published by The Institute of Management Sciences, and later in tandem with the Operations Research Society of America was dedicated to practical applications.

[40] David R. Jardini, "Out of the Blue Yonder: The RAND Corporation's Diversification into Social Welfare Research," Ph.D. dissertation, Carnegie Mellon University, 1996; David A. Hounshell, "The Cold War, RAND, and the Generation of Knowledge, 1946-1962,"*Historical Studies in the Physical and Biological Sciences* **27** (1997): 237-268; Andrew David May, "The RAND Corporation and the Dynamics of American Strategic Thought, 1946-1962," Ph.D. dissertation, Emory University, 1998; Collins, *Cold War Laboratory*; and Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, Mass.: Harvard University Press, 2005) have all made extensive use of RAND holdings. For further reading on RAND, see Bruce L. R. Smith, *The RAND Corporation: Case Study of a Nonprofit Advisory Corporation* (Cambridge, Mass.: Harvard University Press, 1966), and Fred Kaplan, *The Wizards of Armageddon* (Stanford: Stanford University Press, 1983).

would also add that if more detailed evidence could be gathered from design departments at the Air Force or at aircraft companies who were privy to RAND's work, significant strides could be made in understanding the significance of early systems analysis. I have only managed to find hints of these groups' evaluation of its significance.

Along the more academic track of OR, the best evidence relating to OR theory, economic theory, statistical theory, and decision theory is largely published in easily accessible journals and simply awaits historians patient and skilled enough to reconstruct the trajectory of argumentation out of the reams of highly technical material that is available. Fortunately, great strides have recently been made in this direction. The methodological and philosophical import of this work has begun to be realized by non-specialists, and long overdue debates surrounding its meaning have been initiated. For instance, we are beginning to understand why new models in the social sciences were understood to be powerful without having to fall back on vague (and, I suspect, largely incorrect) notions of the perceived political authority of quantitative and formal argument.[41]

Despite these recent advances, the need for better analytical frameworks persists. Policy sciences related to OR have usually been seen as an extension of operations research, with OR defined simply as any attempt to bring scientific methodology (an idea usually translated as mathematics) to problems of warfare. As noted earlier, Paul

[41] Mirowski, *Machine Dreams*; Amadae, *Rationalizing Capitalist Democracy*; and Ghamari-Tabrizi, *Herman Kahn*; Hunter Crowther-Heyck, *Herbert A. Simon: The Bounds of Reason in Modern America* (Baltimore: The Johns Hopkins University Press, 2005); and Paul Erickson, "The Politics of Game Theory: Mathematics and Cold War Culture," Ph.D. dissertation, University of Wisconsin-Madison, 2006, present quite different views of developments in the same intellectual milieu, and all should now be essential reading to anyone with an interest in the area. Also see Mary S. Morgan and Margaret Morrison, eds., *Models as Mediators: Perspectives on Natural and Social Science* (New York: Cambridge University Press, 1999); Frank A.G. den Butter and Mary S. Morgan, eds., *Empirical Models and Policy-Making: Interaction and Institutions* (New York: Routledge, 2000); and Mary S. Morgan, "How Models Help Economists to Know," *Economics and Philosophy* **18** (2002): 5-16.

Edwards has linked OR to whole classes of command-and-control technologies and sciences of cognition through his notion of the "closed world" or "cyborg" discourse.[42] Andy Pickering has deployed similar ideas to identify OR as a crucial agent in the creation of what he calls the "World War II regime".[43] Systems analysis at the RAND Corporation has been treated, repeatedly, as an OR-inspired attempt to generate a "science of warfare" out of pure mathematics.[44] Game theory and decision theories have been treated as extrapolations from OR-type problems of combat.[45] In particular, game theoretic studies of nuclear strategy have been classed as examples of the kinds of studies that can be done when OR ceased to be based on actual operations and was free to develop *a priori* theories of combat.[46] More nuanced studies have repeated the tendency. Philip Mirowski has seen what he calls "American" OR as playing a role in introducing ideal machine actors as economic agents in neoclassical economic theories.[47] In her extremely informative study of the history of rational choice theory, Sonja Amadae treats it as an extension of RAND systems analysis (itself, per custom, portrayed as an extension of wartime OR), and thus as an attempt to turn decision making into a rationalized and depoliticized process.[48]

All of these arguments hinge on the idea that mechanized metaphors designed to create rationalized technological and political machinery were deployed to describe social phenomena objectively, despite the clear difficulty that the mathematical descriptions

[42] Edwards, *The Closed World.*
[43] Pickering, "Cyborg History".
[44] Jardini, "Blue Yonder"; and Collins, *Cold War Laboratory.*
[45] See Kaplan, *Wizards*; and the critique of this view in Erickson, "Politics of Game Theory," chapter 2.
[46] Again, Kaplan, *Wizards*, seems to be a source of this perspective; but see much earlier critiques, even from within the realms of OR sympathizers, especially P. M. S. Blackett, "Critique of Some Contemporary Defense Thinking" [1961], reprinted in Blackett, *Studies of War*, pp. 128-146.
[47] Mirowski, "Cyborg Agonistes"; and Mirowski, *Machine Dreams.*
[48] Amadae, *Rationalizing Capitalist Democracy*, chapter 1.

corresponded to no concrete ontology. Viewed from this perspective, the historiography seems to interpret World War II-era OR as a more-or-less legitimate attempt to employ simple models to highly localized and simple problems, and it seems to argue that the efficacy of such practices fell apart under more ambitious, less constrained, and highly politicized applications. I want to argue against the grain of this historiography and suggest that the policy sciences should be measured on more axes than just ambition of application. Systems analysis, decision theory, and nuclear strategy, I argue, were not *extensions* of the idea that mathematics could be applied to operational problems. These were all trends that took place both alongside each other, and alongside existing policy and planning techniques. "Ambitious" theories, I argue, were not necessarily meant to be *applied*, at least not in any direct or obvious sense of the word application. Instead, it is better to say that theoretical extrapolations from policymaking practice could generate *insights* that could be translated back into practice. Insights from combat could inform the design of a single weapon. Insights from systems analysis could inform weapons systems design. Insights from policymaking could inform a theoretical OR study. Insights from OR theory could inform OR practice. Insights from OR practice could inform policymaking. The development of insights is an essential piece of any intellectual activity, whether in high theory or in policymaking itself, and is crucial to understanding the way OR and the other policy sciences beneficially complemented the strengths and weaknesses of traditional policymaking practices.

I will not further burden this introduction with an epistemological theory of insight transfer. Peter Galison has posited theories of linguistic trading zones and offered the simile of critical opalescence to discuss the transfer of insights across cultural gaps in theory, experiment, instrumentation, and technology-building communities.[49] David Kaiser and Andy Warwick have discussed the dispersion of insights across groups of theorists via pedagogy and the "paper tools" of calculation.[50] I would hope that the contents of this dissertation extend *these* scholars' insights to the divides separating scientific and policymaking communities. Therefore, I simply refer readers to their evocative works for the best available explications of the processes of insight transfer at work here, and repeat once again the need to look beyond the divide between the history of policy and the history of science. However, rather than give further credence to this divide by calling once again for its destruction, I prefer to let the historical actors who sought to overcome it speak through their actions, and thus introduce the possibility that it might never have existed in any historically meaningful way.[51]

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[49] Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997); and Peter Galison, *Einstein's Clocks, Poincaré's Maps: Empires of Time* (New York: W. W. Norton & Company, 2003).

[50] David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005); Andrew Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity, 1905-1911, Part I: The Uses of Theory" *SHPS* **23** (1992): 625-656; Andrew Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity, 1905-1911, Part II: Comparing Traditions in Cambridge Physics" *SHPS* **24** (1993): 1-25; and Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003).

[51] This argument echoes that of David Edgerton against the "linear model" leading from scientific theory to technological application. By constantly setting up the linear model as a straw man to be knocked down, historians have consistently given credence to the notion that at some point in time it was actually influential; David Edgerton, "'The Linear Model' Did Not Exist: Reflections on the History and Historiography of Science and Research in Industry in the Twentieth Century," in *The Science-Industry Nexus: History, Policy, Implications*, edited by Karl Grandin, Nina Wormbs, and Sven Windmalm (Sagamore Beach: Science History Publications/USA, 2004), pp. 31-57.

CHAPTER ONE

The Heuristics of War: Operations Research and Military Planning

Prelude: Curtis E. LeMay, Scholar

United States Air Force General Curtis E. LeMay is a man of a formidable historical reputation, immortalized as an irritable, cigar-chomping, prototypically tough-talking military man. During World War II he was a leader in a relentless bombing campaign against targets in Germany and Nazi-occupied Europe, and, after that, he conducted massive, savagely lethal incendiary raids against Japanese urban centers, killing hundreds of thousands. During the early years of the Cold War, as the head of the Air Force's Strategic Air Command (SAC), he was in charge of the United States' nuclear arsenal. He is credited with transforming SAC into an elite force, and, legendarily, he did not shy away from his nuclear responsibilities. In 1957 Robert Sprague, a leading member of the blue-ribbon Gaither Committee investigating the nation's nuclear strategy, was shocked to learn that LeMay had his own plans to attack the Soviet Union independent of Presidential authority if he saw the need arise.[1] LeMay rose to become Chief of Staff of the Air Force.[2] In this office, he remained an aggressive presence, recommending an attack on Cuba during the 1962 Missile Crisis at the risk of nuclear war, and tirelessly advocating the bombardment North Vietnam. LeMay retired in 1965, but to cap off his extraordinary career, he became the Vice-Presidential candidate on Alabama Governor George Wallace's segregationist ticket in the 1968

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[1] Fred Kaplan, *The Wizards of Armageddon* (Stanford: Stanford University Press, 1983), p. 134.
[2] The Chief of Staff is the highest military position in the United States Air Force.

election, where he maintained his aggressive approach to nuclear policy. Curtis LeMay was not a man of self-questioning or otherwise liberal-thinking inclinations.[3]

I am beginning our story with LeMay to show that, despite his reputation, it is possible to understand him as someone who could demonstrate subtlety of thought and participate in a critical dialogue about technical issues—that he had a heuristic attitude toward combat. Beyond his bluster and ruthlessness, there was more to his treatment of facts than the images like the ones the journalist Fred Kaplan has given us: LeMay, unable to bomb precisely by daylight because of the weather over Japan, threw away his orders, took out a *World Almanac*, found the largest cities in Japan, and put them on his target list. When will the war be over? LeMay was asked. He got his staff to total up the number of square miles of target in Japan left to be bombed and the time necessary to bomb them.[4] The point is well-taken: he had an inordinate faith in strategic bombing. But I want to discuss his credentials as a scholar. It has been recognized that LeMay was a great tactician, and what concerns us here is his deep commitment to understanding the intellectual principles of bombardment.[5]

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[3] On LeMay, see his autobiography, Curtis E. LeMay with MacKinlay Kantor, *Mission with LeMay: My Story* (Garden City: Doubleday, 1965); Thomas M. Coffey, *Iron Eagle: The Turbulent Life of General Curtis LeMay* (New York: Crown Publishers, 1986); and the recent overview, Barrett Tillman, *LeMay* (New York: Palgrave Macmillan, 2007).

[4] Kaplan, *Wizards*, pp. 42-43.

[5] Michael S. Sherry, *The Rise of American Air Power: The Creation of Armageddon* (New Haven: Yale University Press, 1987) is quite critical of LeMay and General Henry Arnold, the head of the Army Air Forces during the World War II, but acknowledges their expertise in their work (rather oddly suggesting that the commanding styles of the delegating Arnold and the meticulous LeMay were similar). Intriguingly, however, he subjects them to the same critique as is usually made of scientific experts: "the limitations of these men were generally the product of the political, cultural, and intellectual environment in which they worked. At times military men did better than civilians in overcoming that environment, and rarely did they do much worse" (p. xi). Further, Sherry describes LeMay's and Arnold's commanding styles in the same chapter on the "sociology" of bombing as he discusses operations researchers. Effectively, they are all, scientists and non-scientists, portrayed as technocrats, and Sherry's work is the more informative for it, regardless of whether or not one accepts his "limitations" of technocracy critical strategy.

One of the best places to see this side of LeMay is in his own environment, among his staff, and especially in the meetings he held regularly after missions had been completed. LeMay and the various officers and air crewmembers who might have attended the meetings shared a finely detailed knowledge about how missions work. Missions were based on intricate plans and procedures, and if one went as planned, there was no reason for further comment: everyone in the room already knew roughly what had happened, and its vital statistics would have been recorded. If, on the other hand, a mission did not go as planned, it was deemed important to find out why.[6] Most often what had gone wrong was fairly obvious, and in most of these cases, crews were found at fault for not following the mission plan.

The plan was not some arbitrary declaration that crews were expected to follow blindly. Individual crews were encouraged to think critically about plans and contribute to them, but, when they were carrying out missions, they were expected to respect the collective wisdom that stood behind them. Most of the time crews' on-the-spot initiatives would not yield superior results. In a critique of one mission over Japan in November 1944, LeMay groused:

> I am going to get rough with people who violate our tactical doctrine. I want to give Airplane Commanders freedom of action in case of emergency, as no plan will cover all situations, so they can digress from tactical doctrine in an emergency, but they had better

[6] A large number of LeMay's mission critiques from 1944 and 1945 are to be found in *CEL*, Box B6. These will be cited over the next several pages only by their title and date. In the first mission critique in LeMay's files, Minutes of the Combat Wing and Group Commanders' Meeting, 2/27/1944, he started the meeting by saying, "I want to bring out again what we expect to accomplish at these division critiques. We don't want to go into any of the detailed problems you are having unless you feel we can help you with them. You can take care of the details in your combat wing critiques. We do want to bring out any objections you may have to our methods of planning these missions, or the problem of other combat wings interfering with or bothering you in your assembly. We will run quickly over the planning of the missions."

have a good reason for doing it. Everybody should follow the plan even though it is wrong. *A poor plan well executed is better than poor execution of a good one.*[7]

What LeMay meant by a "wrong" plan was that it certainly contained elements that would later prove unfounded. However, crew experiences, cross-checked against received wisdom, could improve plans for future operations. LeMay elaborated on this point in the foreword to a manual on tactical doctrine issued in March 1945:

> In its present form, the Tactical Doctrine reflects the overall combat experiences of very heavy bombardment units.... As more experience is obtained and as the tactics employed by the enemy are modified, new precepts will be involved and must be incorporated. Organization commanders are encouraged to suggest revisions and new techniques which will improve the doctrine. Recommendations will be embodied immediately, following the establishment of the value and need for change.

He closed the foreword with a rewording of his comment from the November raid: "It must be kept in mind that an excellent plan half heartedly followed will never put as many bombs on the target as a less perfect plan *which is thoroughly understood* and is forcefully executed."[8] His rephrasing is revealing: it clearly shows that the formation of doctrine was as much about giving missions an explicit intellectual framework as it was about maintaining discipline.[9] The idea of the intellectual framework will prove crucial as we move forward. What was structured, and thus comprehensible, even if not perfectly so, could be compared with experience and amended. If crews acted on their own initiative, it would not be possible to derive any sort of common wisdom from experience. It invited intellectual chaos and, eventually, defeat for all. However, mission

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[7] Critique, 11/26/1944, p. 13. The last line is underlined in pencil. It is likely LeMay underlined it, given it was his personal copy of the critique and it is paraphrased in his foreword to the tactical doctrine manual quoted below.

[8] XXI Bomber Command Tactical Doctrine, 3/12/1945, *CEL*, Box B13. Emphasis added.

[9] Consider the quote from Critique, 11/8/1944, p. 2: "I am working toward a standard procedure to use every time, so that everyone knows what is going to happen. Knowing what is going to happen, the mission will be a success from every standpoint. So let's do these things we decided on down here. Let's have no special occasions. What we want is a standard procedure for bombing those targets—one that will work under all conditions. If we get that type of procedure we will have simpler problems." LeMay's rhetoric harkens to a longstanding discourse of standardization, homogenization and governance; but, we can also see how the ability to identify and adapt to changes was also central to his thinking.

critiques made certain that doctrines did not become doctrinaire and thus incapable of adapting to circumstances.[10]

Some of the problems afflicting missions could be straightforwardly technical. When Japanese aircraft attacked by surprise in bright sunlight, it reinforced the need for better sunglasses. A gunner lost when his blister got blown out suggested the need for better seatbelts. Dust generated on take-off could be an enormous problem in launching large missions, and frost forming on aircraft windows could impair formation flying and navigation. The frost problem, in particular, prompted a flurry of questions as to whether turning on a heater hindered rather than helped, whether installing double panes of glass would solve the problem, whether the problem was worse on old planes than on new planes, and whether it was advisable to open the plane's windows to clear the frost. LeMay could temporarily accept messy *ad hoc* solutions, such as opening the windows, but ultimately he had a "special wire to Washington" sent to get technical personnel "back home" to work on it. Special heaters were later installed to cope with the problem.[11]

Other problems dealt with ascertaining mission performance. LeMay depended on statistical evidence to make a proper evaluation. Although strategic bombing may seem to require nothing more than brute force, they were actually relatively complicated affairs. Accuracy and economy mattered. LeMay made this point clear in a mission critique from

[10] On the importance of doctrine building to military thought, see Robert Frank Futrell, *Ideas, Concepts, Doctrine: Basic Thinking in the United States Air Force*, 2 vols. (Maxwell Air Force Base: Air University Press, 1989); and I. B. Holley, Jr., *Technology and Military Doctrine: Essays on a Challenging Relationship* (Maxwell Air Force Base: Air University Press, 2004). Operations research is featured in a supporting role in both works, which accords well with the argument presented here.
[11] Critique, 11/26/1944, on p.4. The use of the new heaters is discussed in Critique, 2/12/1945, p. 3.

October 22nd, 1944:

> Our bombing job at first glance appears to be good, but a study of the average shows we did a poor job. We only destroyed the target because of the volume of bombs we put on it. Only 11% of our bombs hit within 1,000 feet of the aiming point, and since only 74% of the total number of bombs dropped were actually found and plotted, this means that only about 8% of all bombs struck within 1,000 feet of our aiming point. This is a very poor showing.

LeMay demanded more information out of Group Bombardier's reports detailing how they made their bombing run, how they felt about their run, and what they would recommend for the future. "We should find out why we miss the target when we do miss," he declared.[12]

Metrics also occasionally came into question, and identifying how close bombs landed to a target was clearly one of the most important metrics in determining whether a mission was flown successfully. In one critique, given that the starting points of a bomb's trajectory were known, LeMay questioned the practice of identifying hits based on strike photographs:

> You are not giving enough credit to the Groups in locating the percentage of bombs dropped. If the bombs are dropped in formation, they've got to be somewhere in the pattern. When a good formation is flown, and assuming all ships bomb together from the formation, we will have to have 100% of the bombs found. It may not be possible to see each burst, but all the bombs will have landed somewhere in the pattern.... Definitely outline the pattern you can see and draw a dotted line to identify where smoke makes it impossible to see.[13]

Seeing was not always believing, and it was important to get details like these right.

It was quite possible that in-depth study could actually overturn how planners such as LeMay understood the dynamics of missions. In another critique LeMay responded with disdain to complaints that bombs seemed to fall consistently short of the target. "This is the same story I've heard repeatedly many times before," he said. He

[12] Critique, 10/22/1944, p. 3.
[13] Critique 11/8/1944, p. 5.

Figure 1.1. An illustration of LeMay from a wartime Army Air Forces document. The magnifying glass and the officer with the clipboard connote study (as of a strike photograph), as much as the evasive expression on the bomber's "face" connotes interrogation. Source: Headquarters, XXI Bomber Command, "Incendiary Bombing Phase Analysis," 1945, *CEL*, Box B37, Folder 3.

pointed to a study of British bombardiers who insisted something was wrong with the ballistics of their weapon:

> Long studies were conducted resulting in bombardiers being given 500 and 1,000 pound bombs to drop in practice. Nothing was wrong with the bombs or the ballistics. The bombardiers merely needed practice in dropping these bombs. In the future, all bombardiers will be given the opportunity to drop all the same type of bombs that are used on the missions.[14]

In this case neither discipline nor equipment was the issue, but training. It was not a straightforward matter to parse these issues, though. In this case, LeMay's judgment was aided by "long studies". Problems were not always identifiable on the spot, even when individuals with diverse expertise were gathered together in meetings.

One problem that continually plagued missions was the perennial difficulty in establishing and maintaining airplane formations. Formations were central to LeMay's strategy of bombing straight and fast. They helped ward off fighter interference, and if lead planes bombed accurately, it would improve the accuracy of the entire formation. To achieve a formation, planes took off individually and only came together at a specific time at a specified rendezvous point where they only had a limited amount of time to

[14] Critique 10/22/1944, p. 4.

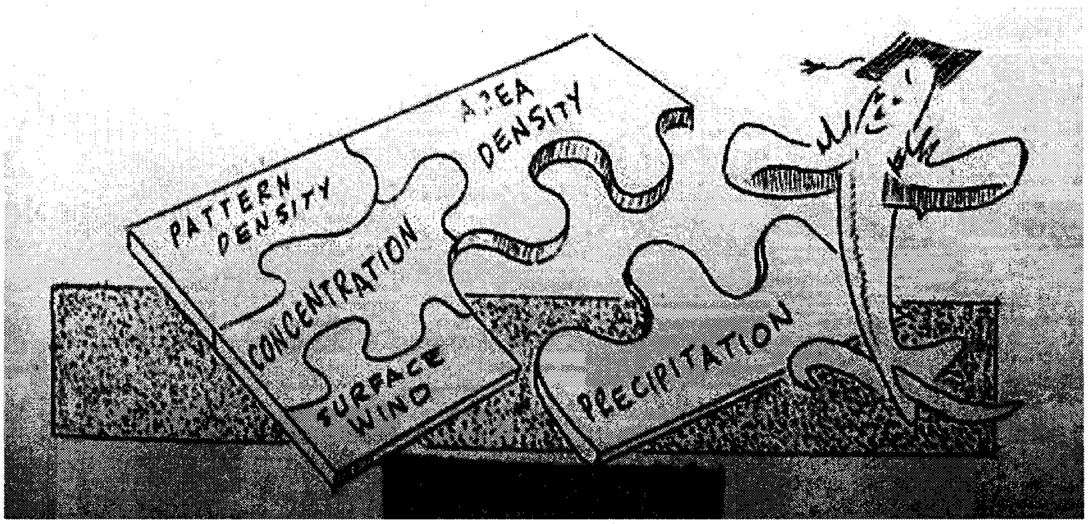

Figure 1.2. Bombing portrayed as a scholarly activity for crewmembers; "precipitation" is being taken away from the puzzle because it had been discovered it did not have a major impact on the success of incendiary raids. Source: Headquarters, XXI Bomber Command, "Incendiary Bombing Phase Analysis," 1944, *CEL*, Box B37, Folder 3.

identify and get behind the lead plane they were to follow to the target. A second rendezvous was also usually specified to pick up stragglers. This procedure was easier said than done, and it sparked serious discussions as to whether separate groups should have separate rendezvous points to avoid confusion, whether different groups should circle in opposite directions or at different altitudes. One of the greatest problems was simply identifying the leader at the rendezvous point. In November 1944 the accepted method was for the lead plane to lower its nose wheel, although there was some question as to whether that was advisable in terms of depressurizing the plane, and whether it should be done only at the first rendezvous. LeMay wondered whether it would be better simply to paint a large spot on the leader.[15]

At this point in the minutes of the critique we hear from a new voice: "Mr. Dyer, Operational Analyst [sic] suggested using a smoke bomb as another means of identifying the lead plane. We can get four different colored smokes in order to have a different

[15] Formation assembly problems were discussed in Critique, 10/22/1944; Critique, 11/1/1944; Critique, 11/8/1944; Critique, 11/26/1944.

color smoke for each Group." LeMay agreed that it was a good idea, but advised that the smoke would have to be in large volumes. "We tried it in Europe with a case of grenades," he recalled, "and it wasn't very successful or satisfactory. It doesn't put out enough smoke. However, it is all right with me if you want to experiment with smoke, but it will have to be used in a big volume."[16]

Operations analysis was the U.S. Army Air Forces' term for operations research. The object of this chapter is to understand the relationship between the activities of World War II-era OR figures such as Dan Dyer (then head the Operations Analysis Section at LeMay's command) and how military figures such as LeMay ordinarily designed, critiqued and altered their plans every time they got together to reevaluate their own practices. Every military doctrine, every training regime, and every plan was based on what we are calling policy knowledge. Talented commanders such as LeMay fostered a careful understanding of policy knowledge, as embodied explicitly in doctrine, and understood how to direct their organizations to craft more effective policies. LeMay understood, though, that it was not always immediately obvious how to do so. Operations research thrived in the niches where the most arbitrary elements of policy knowledge prevailed, such as where the effectiveness of certain policies could not be easily judged, or where various kinds of kinds of localized knowledge could not be properly evaluated with respect to their impact on planning through meetings and liaison—where it was necessary to go off and "experiment" or to perform "long studies".

We will begin our discussion with an overview of who operations researchers were, how they were organized, and what they did, before moving on to a more important question: why OR was seen as a peculiarly scientific activity and a significant concept.

[16] Critique, 11/8/1944, p. 3. *Operations* analysis was the official term.

Although we have no reason to doubt that it was scientific, it will be useful to put the military back into a story in which they are usually only seen as patrons of scientific work. Only once we have understood how "military heuristics" relates to scientific heuristics will we really be able to understand how OR established itself with respect to military planning. The chapter ends with a tour through the different ways OR was used during the war to add robustness to existing planning methods. We will find when we are done that understanding the significance of operations research as a scientific activity does not have to entail perpetuating the myth of a non-scientific military. OR was as much a product of military heuristic culture as it was a product of any outside scientific culture brought into the military by the exigencies of the war.

Introduction to Operations Research

By the end of World War II operations research had become an activity practiced in each of the five military services that then existed in the United States in Britain. Groups of British scientists, largely concerned with the deployment of technology (to be discussed in Chapter Two), were created at the Royal Air Force (RAF) Fighter Command, and in the Army's Anti-Aircraft Command, in 1939 and 1940 respectively. Both these groups grew to include several dozen members, and were soon incorporated into a more coherent "operational research" organization.[17] In the RAF, this organization developed

[17] The history of these groups has been repeatedly rehearsed. See especially Air Ministry, *The Origins and Development of Operational Research in the Royal Air Force* (London: HMSO, 1963); Maurice W. Kirby, *Operational Research in War and Peace: The British Experience from the 1930s to 1970* (London:: Imperial College Press, 2003), which contains an extensive bibliography of prior histories; and Erik Peter Rau, "Combat Scientists: The Emergence of Operations Research in the United States During World War II," Ph.D. Dissertation, University of Pennsylvania, 1999; and Erik P. Rau, "Technological Systems, Expertise, and Policy Making: The British Origins of Operational Research," in *Technologies of Power: Essays in Honor of Thomas Parke Hughes and Agatha Chipley Hughes*, edited by Michael Thad Allen and Gabrielle Hecht (Cambridge, Mass.: The MIT Press, 2001), pp. 215-252.

in the summer of 1941 with the establishment of OR groups in the headquarters of the RAF's Bomber and Coastal Commands in Britain. The former employed 48 researchers and 34 assistants by 1944, while the latter, probably the most written-about group of the war, never had more than about twenty-five researchers.[18] In December 1941 the Royal Navy established an OR group that was smaller still and never grew very large. In 1942 OR grew rapidly. The RAF deployed groups in its smaller home commands and in theatres of operation. The Army also began to expand its OR infrastructure by establishing a variety of new teams both in Britain and in the field.[19]

In March 1942, a key group dedicated to anti-submarine operations, called the Anti-submarine Warfare Operations Research Group (ASWORG), was established in the U. S. Navy. It soon grew to include dozens of members, and later branched out into other areas, creating new OR groups, such as PhibORG, dedicated to amphibious operations, SORG, dedicated to submarine warfare, and AirORG, dedicated to studies of Naval air power. In 1944 the overarching group name was shortened simply to Operations Research Group.[20] The Army Air Forces established Operations Analysis Sections in its

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[18] Air Ministry, *Origins*, pp. 46, 75.

[19] The most complete published accounts of OR in the British Army and Royal Navy can be found in Kirby, *Operational Research*, pp. 110-126. All accounts of OR in the Royal Navy focus exclusively on the activities of Patrick Blackett. On British Army OR, see also Brian Austin, *Schonland: Scientist and Soldier* (Philadelphia: Institute of Physics, 2001), which focuses only on those aspects of Army OR in which South African physicist Basil Schonland participated. Nevertheless, it is the most detailed published information available.

[20] See Keith R. Tidman, *The Operations Evaluation Group: A History of Naval Operations Analysis* (Annapolis: Naval Institute Press, 1984). For additional information on the formation of ASWORG, see Rau, "Combat Scientists," and Erik P. Rau, "The Adoption of Operations Research in the United States during World War II," in *Systems, Experts and Computers: The Systems Approach in Management and Engineering, World War II and After*, edited by Agatha C. Hughes and Thomas P. Hughes (Cambridge, Mass.: The MIT Press, 2000), pp. 57-92. Philip M. Morse, *In at the Beginnings: A Physicist's Life* (Cambridge, Mass.: The MIT Press, 1977), chapter 7 is also useful. In addition to the OR sub-groups mentioned here, there was also AAORG, dedicated to anti-aircraft activity and was renamed SpecORG (Special Defense) when it began to study kamikaze attacks; and an Operations Research Center dedicated to supplying mathematical services to the groups. The Navy also hosted a Mine Warfare ORG, which was separate from its other ORGs.

various arms, beginning in 1942 with a section at the Eighth Air Force that was closely tied to the RAF Bomber Command OR Section, but would eventually expand to 30 sections varying in size between one and 48 analysts (totaled over the course of the war).[21] When America's Office of Scientific Research and Development (OSRD) established an Office of Field Services in 1943, it took over the Navy group and established its own OR sections in the army, although these, like the earliest British groups, were specifically oriented toward the extension of laboratory expertise to technical problems of the field, and not to research into military operations in general. The land forces in the American army never established a widespread OR program while the war lasted.[22] All three branches of the Canadian military also employed OR groups during and after the war, but, following a lamentable trend, we will neglect them here.[23]

Operations research was largely done by civilians. Unlike some other kinds of specialist personnel, such as meteorologists or statisticians who were brought into the service ranks to perform a skill for which they were specifically trained,[24] it was usually seen as advantageous for OR personnel to retain their civilian status largely because of

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[21] See Charles R. Shrader, *History of Operations Research in the United States Army, Volume I: 1942-1962* (Washington, DC: Government Printing Office, 2006), Appendix B; based on material in LeRoy Brothers, et al, *Operations Analysis in World War II: The United States Army Air Forces* (Philadelphia: Stephenson-Brothers, 1949). Also see Rau, "Combat Scientists"; and Rau, "Adoption". See also Charles McArthur, *Operations Analysis in the U.S. Army Eighth Air Force in World War II* (Providence: American Mathematical Society, 1990), which is more of a personal reminiscence than a thorough history.

[22] See Lincoln R. Thiesmeyer and John E. Burchard, *Combat Scientists* (Boston: Little, Brown, and Company, 1947). See also Rau, "Combat Scientists"; and Rau, "Adoption".

[23] However, see N. W. Norton, "A Brief History of the Development of Canadian Military Operational Research," *Operations Research* **4** (1956): 187-192; and R. H. Lowe, "Operational Research in the Canadian Department of National Defence," *Operations Research* **8** (1960): 847-856.

[24] On military meteorology see Roger Turner, "Teaching the Weather Cadet Generation: Aviation, Pedagogy and Aspirations to a Universal Meteorology in America, 1920–1950," in *Intimate Universality: Local and Global Themes in the History of Weather and Climate*, edited by James Rodger Fleming, Vladimir Jankovic, and Deborah R. Coen (Sagamore Beach: Science History Publications/USA), pp. 141-173; on the Army Air Forces Statistical Control Section, see David Lowell Hay, "Bomber Businessmen: The Army Air Forces and the Rise of Statistical Control, 1940-1945," Ph.D. dissertation, University of Notre Dame, 1994.

their disinterested outsider status, because their analyses sometimes required them to ask probing questions of high-level officers, and because it was administratively simpler for civilians to gain access to personnel and records outside their regular contacts, and because a number of suitably-talented individual were, for various reasons, not commissionable. However, some OR teams, and especially those working in combat zones, were largely populated by commissioned officers, mostly because international agreements protecting prisoners did not apply to civilians, but also probably because it was often easier to obtain military personnel for field work.[25] Basil Schonland, a commissioned South African physicist who was for a time the head of the Army Operational Research Group (AORG) in Britain, managed to maintain a nonchalance on the matter. Visiting England in late 1942, the civilian administrator of ASWORG, MIT physicist Philip Morse, observed,

> Colonel Schonland has about 70 men under him, part of them officers and part civilians. He shifts his officers and civilians back and forth from one status to the other, at will, with a very pleasing celerity. He feels it doesn't matter what suit a man wears so long as he does his job, and he expects his officers to obey their superior whether the superior is a civilian or another officer. This seems an ideal situation.[26]

Although the advantages of OR personnel retaining civilian status were doubtless quite real, historians should note Schonland's example and not overstate its importance. Civilians could sometimes be contemptuous of the military bureaucracy's restrictions on

[25] Various rationales have been reported for the use of civilians in various places, and for the use of commissioned officers in some cases. Good examples are to be found in Shrader, pp. 27-28. See also "Report on Operations Analysis", Joint Committee on New Weapons and Equipment," 7/7/1942, *NACP*, RG 218, Joint New Weapons Committee, Subject File, May 1942-1945 [UD/92], Box 57, "Operational Research Reports" folder.

[26] See "Diary of Dr. Philip M. Morse and Dr. William Shockley, Visit to London Commencing November 19, 1942," written by Morse, entry for 12/5/1942; *ELB*, Box 39, Folder 5. On Schonland, see Austen, *Schonland.*

who could talk to whom with whose permission, but if good liaison was practiced, solutions could usually be worked out for military personnel and civilians alike.[27]

The bulk of OR personnel were, of course, technically trained in one way or another. The final report of the American Navy's ORG provides excellent data on group members. The report lists 86 individuals who had served in ORG at some point or another since the group's inception in 1942. Thirty-two members had degrees in mathematics and 28 in physics, including William Shockley, a future Nobel Prize-winner and the first research director of the group. Chronologically, the group's first fifteen members came entirely from these two backgrounds. There were six members with degrees in chemistry—including George Kimball, who became the group's sixteenth member and later its second director of research, and Jacinto Steinhardt, who took over the group's administration after the war. The remainder of the members was comprised of five with degrees in engineering, four in biology, two in economics, two in geology, two in astronomy, two in library science, one in zoology, one in architecture, and one member was listed as having an Abitur. In terms of depth of training, 39 of the 86 had PhDs. Roughly half of the degrees in physics and mathematics were PhDs. Among the biologists, geologists, astronomers and the zoologist, only one (a biologist) *did not* have his PhD. Thirteen, including the architect, had master's degrees, and thirty-one are listed as having only bachelor's degrees. Another source from late 1945 lists the actual peacetime professions of the 59 members then still with the group. While most were instructors or research assistants at universities, a number of others were still graduate

[27] Morse, *In at the Beginnings*, chapter 7 discusses liaison problems in some detail. On similar issues of bureaucracy in the context of the Manhattan Project, see Martin J. Sherwin, *A World Destroyed: The Atomic Bomb and the Grand Alliance* (New York: Knopf, 1975); and Charles Thorpe, *Oppenheimer: The Tragic Intellect* (Chicago: University of Chicago Press, 2006).

students, a few were researchers or engineers in private industry, and, intriguingly, eleven of the non-PhDs worked in the insurance industry, primarily as actuaries.[28]

Although precise figures are not available, we do know that operations analysis sections in the U.S. Army Air Forces boasted a fairly large contingent of lawyers. The first head of the Operations Analysis Section at the Eighth Air Force was the lawyer John Harlan, who was later appointed to the U. S. Supreme Court (a classic OR trivia question).[29] LeMay's teams in Asia and the Pacific seem to have been largely recruited from the European teams, emphasizing members' previous experience in operations analysis, and especially in specialized military fields such as bomb plotting and flight engineering, rather than their civilian expertise. Some of the staff had PhDs, while the rest were split between civilians and officers, but, again, we should probably not read too much into it. For instance, Major Forrest Sandborn, recruited to the Operations Analysis Section at the Twentieth Air Force in the waning days of the war, was a specialist in target analysis, a skill that might have been adapted from his peacetime work as a fire engineer with the Improved Risk Mutual Insurance Company.[30]

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[28] "Summary Report to the Office of Field Service, O.S.R.D. from the Operations Research Group" 12/1/1945, *NACP*, RG 227, Office of Field Service: Manuscript Histories and Project Summaries, 1943-1946 [NC-138/177], Box 284, "ORG – Operations Research Group P. M. Morse's Final Summary" folder; and "Members of the Operations Research Group (as of 16 Oct. 1945)," *NACP*, RG 38, New Development Section – Subject File, 1942-46 [A1/329], Box 9, "Operations Research Group – Correspondence" folder. At its 1945 peak, the ORG had some 70 members, having retained most of the personnel who had joined. Before the war George Kimball was an Associate Professor of Physical Chemistry at Columbia University and Steinhardt had been a physical chemist at the National Bureau of Standards. The actuaries primarily had bachelor's degrees in mathematics.

[29] On the fact's trivia value, see the back cover of Saul I. Gass and Arjang A. Assad, *An Annotated Timeline of Operations Research*; and Saul Gass, "Not a Trivial Matter," *OR/MS Today* (October 2002), available online at <http://www.lionhrtpub.com/orms/orms-10-02/frtrivia.html>. The fact arose independently in my correspondence with former members of the U. S. Army's Operations Research Office.

[30] "Report for the Period 1 August 1945 to 4 September 1945", 9/4/1945, Operations Analysis Section, Headquarters Twentieth Air Force, *CEL*, Box 38. Sandborn had been requested by the Operations Analysis Section in the Twentieth Air Force on May 4th, 1945, but did not arrive until after the end of the war because he had been needed with the U.S. Strategic Bombing Survey in Europe.

On the British side, we have a list of the "qualifications" of the Bomber Command ORS in September 1942, about a year into the section's existence. ORS personnel were quite similar in background to the U.S. Navy's ORG: of the 29 members listed, there were eight mathematicians, seven physicists, two physiologists, two botanists, two engineers, a statistician, a metallurgist, a zoologist, a chemist-geologist, a physicist-geologist, an archaeologist, an historian, and a "General B.Sc.".[31] This list did not include the section's leader, Dr. Basil Dickins, a member of the Ministry of Aircraft Production's research corps. The official history of OR in the RAF lists the professions of Fighter Command's OR Section, which had grown directly out of the work of the research and development establishments. The section's "nucleus" was formed by permanent civil servants, only one of whom had done academic research. Other members boasted the usual motley array of degrees. Apparently, many of these members moved on to newly created sections and were "replaced by schoolmasters with degrees in physics or mathematics."[32] Breaking from the usual trend of employing women only as number crunchers and information managers, the RAF employed several women as analysts in its OR Sections. The Coastal Command ORS' expert on the methodology of

[31] RAF Operations Research Centre, sub-committee on distribution of personnel, Bomber Command personnel roster, 9/28/1942, TNA: PRO AIR 14/763. Freeman Dyson, who would later gain fame in theoretical physics, would join the group in 1943 at the age of 19. He was disillusioned with his experience in the section. See Freeman Dyson, *Disturbing the Universe* (New York: Harper and Rowe, 1979); and Freeman Dyson, "A Failure of Intelligence: Operational Research in RAF Bomber Command, 1943-1945," *Technology Review* **109**/6 (2006): 62-71. On Dyson's physics career see Silvan S. Schweber, *Q.E.D. and the Men Who Made It: Dyson, Feyman, Schwinger, Tomanaga* (Princeton: Princeton University Press, 1994).

[32] Air Ministry, *Origins and Development*, p. 11.

aerial attacks on U-boats was one of them.[33] We know comparatively little about the general makeup of members of the Royal Navy's and the British Army's OR groups.[34]

Although OR groups advised senior military officers in person, corresponded with them via letters and memoranda, and participated in high-level meetings, their principal output was official reports, which were sometimes requested by military officers but often produced at the OR group's own volition. It would be difficult to offer an accurate figure of how many reports were actually produced during the war. Not all reports were accounted for even at the time, and, when the war ended, studies in progress were completed, and retrospective studies of the war experience continued to be produced for years. Certainly well over one thousand OR reports were produced in Britain alone.[35] These reports differed dramatically in size, scope, content and purpose. Some were nothing more than one or two page diary accounts of visits to a laboratory or a base. Others could be many dozens of pages, offering mathematical theories of military operations or statistical breakdowns of thousands of mission reports. They received wide distribution among high level officers, particularly those concerned with tactics and

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[33] "Miss Goodman"; listed in "Summary of Work of Coastal Command O.R.S." c.10/1942, TNA: PRO AIR 14/763.

[34] The most senior members of the Navy's OR group were all elite physicists. The head of the group was Patrick Blackett, a physicist who would win the 1948 Nobel Prize, and was the most recognized OR personality on either side of the Atlantic. He was assisted by Ralph Fowler, Edward Bullard, and E. J. Williams. H. R. Hulme, who had been an assistant at the Royal Observatory, later became Blackett's deputy and successor. Other members were drawn from the Admiralty's research corps, but we know little of their backgrounds. Aside from figures such as the physicists Leonard Bayliss and Basil Schonland, who had been at the Cavendish Laboratory at Cambridge, and the Canadian physiologist Omond Solandt, we know very little about Army OR personnel..

[35] "Complete List of Reports Issued by the Research Branch, Fighter Command, or its Equivalent, from 1940 to October 1947," TNA: PRO AIR 16/1042 indicates that reports from the Fighter Command ORS ran from number 101 to number 678 through 1945; Coastal Command reports ran from 125 to roughly 365; Bomber Command seems to have produced some 150 reports, plus an unknown number of memoranda and "S" reports, which did not reflect official results. There were a large number of Army groups; the ones serving under the Ministry of Supply seem to have produced somewhere near 300 reports, if numbering is consistent. Field groups were scattered, and seem to have produced varying numbers of reports making counts tedious and unreliable. Blackett's office at the Royal Navy did not number their reports during the war and I have found no list compiling them; they are largely located in TNA: PRO ADM 219.

equipment, and among research and development personnel, and they were regularly passed between commands, military services and countries. It is important to understand, though, that these reports were one small segment of a vast knowledge economy of communiqués, memoranda, circulating files of correspondence, intelligence reports, technical reports, exercise reports, mission reviews, meeting minutes, statistical digests, bulletins, instructions, orders, updates on doctrine, training syllabi, maps, charts, and nearly any other means of conveying and analyzing information one cares to consider. There is really no consistent set of criteria that could effectively be used to distinguish OR reports from these other kinds information shuttling their way through busy offices and across the globe. From this perspective it is surprising that OR managed to grow as rapidly as it did, given its newcomer status. Still, while it is important not to take OR's importance for granted, it is just as important to understand what it was that allowed it to hold its own.

The Question of Methodology

The methodology of wartime OR can either be extremely simple or extremely complicated to describe. It would be possible, for instance, to offer a straightforward rundown of the limited set of statistical and other analytical techniques actually employed by OR personnel. Given the growing interest in wartime OR, it has been surprising how little detailed historical work has been done in this area, and we will discuss some of these methods presently. However, simply classifying a set of methods without regard to what use they were put would not be an especially illuminating exercise. More compellingly, one can discuss OR as a sort of an organizational catch-all for a variety of activities that entailed the observation of field operations and depended on close liaison with operational military personnel. Thus studies requiring a background in a specialized area of knowledge, such as bombing accuracy analysis, target prioritization, evaluation of the relative effectiveness of weapons or radar technologies, and scheduling personnel assignments, would all in day-to-day practice bear a far closer resemblance to their related military specialties than to each other. Yet, because they all also entailed the introduction of scientifically or mathematically trained personnel into a field environment, they would be grouped under the rubric of OR.

The fact that OR teams usually divided their work according to subject matter bears this view out.[36] In RAF Coastal Command, a young student named William Merton was in charge of studies relating to bomb sights and camouflage. (His father, the noted physicist Thomas Merton, had done camouflage studies for the Air Ministry and had produced a special black paint for night bombers.)[37] The biologist Cecil Gordon worked on personnel assignments and aircraft serviceability.[38] Other team members worked on the aforementioned aerial attacks on U-boats, photographic reconnaissance, navigation, and Anti-Surface Vehicle (ASV) radar, to mention only a few examples.[39] The larger RAF Bomber Command OR Section was divided into teams associated with the efficacy of night bombing, the "cost" of night bombing (meaning losses), and

[36] Kirby, *Operational Research*, discusses the work divisions within certain British groups.
[37] Harold Hartley and D. Gabor, "Thomas Ralph Merton" *Biographical Memoirs of Fellows of the Royal Society* **16** (1970): 421-440. William Merton later became Lord Cherwell's assistant (see Adrian Fort, *Prof: The Life of Frederick Lindemann* (London: Jonathan Cape: 2003)), and, after the war, he became a merchant banker.
[38] Jonathan Rosenhead, "Operational Research at the Crossroads," *Journal of the Operational Research Society* **40** (1989): 3-28. According to Rosenhead, Gordon drew analogies between animal reproductive behavior and aircraft servicing. See Gordon's original report, "The Efficient Utilisation of Manpower Resources, With Special Reference to Maintenance Manpower," ORS Coastal Command Report No. 206, 11/15/1942, TNA: PRO AIR 15/732; and the co-authored "Final Report on the Experiment on Planned Flying and Maintenance with No. 502 Squadron: August-December 1942," ORS Coastal Command Report No. 223, 3/16/1943, TNA: PRO AIR 15/732.
[39] "Summary of work of Coastal Command O.R.S.," n.d. [c.1942], TNA: PRO AIR 14/763.

problems related to day bombing. These teams, in turn, divvied up their work according to problems within each category. One set of problems in night bombing efficacy, for example, included "research on the problem of visual identification of the target", "study of general navigational problems (including the use of non-radio aids)", "study of the use of decoys and camouflage by the enemy", "determination of the effect of experience on target location", and "study of the effect of routeing on target location".[40]

It has, unfortunately, never been made clear what all this "study", "research" and "determination" actually entailed. To give some idea, let us look for a moment at work on bombing accuracy analysis. In the late phases of the war at the Twentieth Air Force, incendiary bombing was a relatively coarse art, the accuracy of which was measured in thousands of feet from pre-identified targets. The accuracy of any given raid could be judged by plotting the locations of bomb explosions using photographs taken during strikes and then determining the radius within which 50% of bombs fell. This task fell to intelligence personnel specially trained for the job. As we have seen even LeMay took a personal interest in finding methods to make sure that these counts reflected actual results. As incendiary bombing ramped up through the first part of 1945 to sometimes include hundreds of planes, the amount of data retrieved from missions as a proportion of the amount of destructive force rained down on Japan diminished. At this time, the operations analysis section took up the task of learning what they could on the subject of accuracy given the data restrictions. Given that only 15% of incendiaries dropped could be located, the section first recommended that cameras be installed on every third or fourth plane. Using the available evidence, though, the section also attempted to

[40] "Operational Research Section Bomber Command, Present Allocation of Staff to Items under Investigation," 9/26/1942, TNA: PRO AIR 14/763.

ascertain factors bearing on accuracy, which could in turn have repercussions for the number of planes assigned to a particular target. This task, though, really only involved parsing existing statistics, such as they were, according to certain criteria. So, for instance, the section measured differences in day and night bombing, how much more accurate bombing was in coastal cities where navigation was easier, and how much accuracy diminished after the first formations made their bombing runs on account of smoke obscuring the target.[41]

In a more generalized sense, operations research involved taking an existing method, here bomb plotting, and, while relaying the standard results from such an analysis, also sought ways to improve the method, or to adapt it to specific operational contingencies. Although, in some cases of OR work such an analysis meant introducing new statistical methods to a problem to which they had not previously been applied, it should not be taken as a defining characteristic of OR. In the case of bombing accuracy determination, far more sophisticated analytical methods than those used by the operations analysts were regularly employed by career military personnel at training and testing facilities to parse causes of systematic and random errors in bombing to do such things as test bomb sights. These methods, though, were clearly inapplicable to measurements of operational data, which was not precise, abundant or varied enough to do detailed error analyses. Operations analysis personnel had to combine a knowledge of existing concepts in bombing accuracy analysis techniques, existing anecdotal knowledge of operational factors impacting bombing, and existing means of mission reporting with

[41] This description relates to Headquarters Twentieth Air Force, Operations Analysis Section, "Report on Bombing Accuracy of Night Incendiary Missions," Bombing Accuracy Report No. BA-2, 8/6/1945, *CEL*, Box B37, "Official Document Summaries, Weapons Analysis 1945" folder.

an eye to improving these methodologies in view of the limitations imposed by fighting conditions. Few of these methods, though, were uniquely theirs.[42]

Many of the scientists, mathematicians, actuaries and military officers recruited into wartime OR would likely recognize this picture of it as a collection of disjointed specialties, as would many of today's OR practitioners. Inventory and production control specialists or transportation system analysts would understand their fields primarily as situated around the specialized knowledge inherent to them, and only secondarily through the shared methodology that binds today's professional field of operations research together. Such a perspective would be neither surprising nor by any means peculiar to OR: physicists who work on problems in aerodynamics and physicists who work on problems in orbital mechanics, although theoretically bound together by the methods of classical mechanics, in reality live in largely separate worlds defined by specialized sets of applications. What makes wartime OR rather unusual is that there was not yet any standardized methodology to foster any sort of disciplinary coherency. At the time, OR simply needed to be defined well enough to tell military commanders that they were receiving trained scientific minds to work on problems that might help them plan their operations more effectively. Accordingly, OR was usually defined simply as an application of "scientific method".[43] Definitions of this kind have proven exceptionally

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[42] However, some OR sections did revise bombing accuracy measurements. See LeRoy A. Brothers, "Operations Analysis in the United States Air Force," *Journal of the Operations Research Society of America*, **2** (1954): 1-16; and also, for instance "Bombing Accuracy with the Mk. XIV Bombsight" ORS Bomber Command Report No. S.220, 4/20/1945, TNA: PRO AIR 14/4537, which pertained to measurements of accuracy in practice runs.

[43] In the introduction to his widely circulated 1941 memorandum "Scientists at the Operational Level," Patrick Blackett wrote, "Operational staffs provide scientists with the operational outlook and data. The scientists apply scientific methods of analysis to this data, and are thus able to give useful advice"; reproduced in P. M. S. Blackett, *Studies of War, Nuclear and Conventional* (New York: Hill and Wang, 1963), pp. 171-176. In the postwar period, ASWORG member Charles Kittel, "The Nature and Development of Operations Research," *Science* **105** (1947): 150-153, put forward the definition officially,

enduring despite, or likely because, of their vagueness. Yet, in terms of understanding OR as a coherent concept in and of itself, they are wholly unsatisfying.

Methodology, Epistemology and the Concept of OR

And OR was a relatively coherent concept. We will find that the postwar developments to be examined in the last four chapters of this dissertation were based on the idea that OR was something that could be identified and applied to postwar situations. So, we need to understand what OR was beyond a loose collection of techniques, which will mean delving into what scientific method meant to those who saw significance in wartime OR. Formal definitions and the philosophy of science are not the issue. The term scientific method was used colloquially to mean drawing on mission data to conduct detailed investigations into unanswered questions pervading military activities. It connoted a questioning, anti-doctrinaire approach to warfare that sought to check assertions routinely made about it, either implicitly or explicitly, against available evidence. However, since we know that military doctrines were not exactly doctrinaire, this working definition does little to indicate why such activity was worthy of sustained interest. To answer this more pressing question, we need to come back to our question of what made OR stand out against other military activities. How did "scientific" thinking relate to military thought?

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and it was given its most-quoted form in the introduction to Philip M. Morse and George E. Kimball, *Methods of Operations Research* (Boston: The Technology Press of MIT; and New York: Wiley, 1951), p. 1: "Operations research is a scientific method of providing executive departments with a quantitative basis for decisions regarding the operations under their control."

C. H. Waddington, a geneticist, embryologist, and one of the wartime heads of the RAF Coastal Command OR Section, was one of the more introspective thinkers about the meaning of OR. He wrote:

> Non-scientific methods of thought, if they are to be of value, rely on a judgment based partly on the mysterious quality of 'flair' or insight, and partly on assimilated experience. Discussions on this level are largely concerned with testing out the 'feel' of the problem and its ramifications. This involves rather little drudgery; and in many cases, there is little of it which can be delegated to those of less experience. In contrast with this, the characteristic contribution of science is twofold, and both parts of it call for sustained and often laborious work. The scientist [...] will try to express problems in quantitative terms, and he will attempt to apply an empirical check to any provisional conclusion.[44]

Waddington was drawing a fairly clear boundary between an intuition-based military and a reason-based science, but as even our brief discussion of General LeMay should indicate, quantitative and empirical reasoning were not exactly foreign to even very high levels of the command hierarchy, to say nothing of technical specialists. In order to understand OR, we must run against the grain of a long rhetorical trend that ascribes importance to the introduction of a foreign scientific epistemology into military thought.[45] Histories that examine forerunners of the wartime discipline, for instance, almost invariably point to instances of scientific advisors entering a military domain, rather than discussing methodological innovations from *within* the military. There are some limited exceptions. Maurice Kirby's history of OR in Britain does describe a few

[44] C. H. Waddington, *OR in World War 2: Operational Research against the U-boat* (London: Elek Science, 1973), p. 248

[45] Consider Fred Kaplan, *Wizards*, p. 52: "thinking about new problems was not an integral feature of the military profession." On p. 53, referring to OR: "for the first time in history, [standard scientific methods of investigation] were being applied to military tactics in wartime." Or Stephen P. Waring, *Taylorism Transformed: Scientific Management Theory since 1945* (Chapel Hill, University of North Carolina Press, 1991), p. 22: "[OR scientists'] special skill lay in scientific training, which had taught them how to define research projects, gather information, analyze data using mathematics, and arrive at objective conclusions. Relying on such scientific methods and on steady, cumulative, quantitative research was more effective than relying on the sporadic insights of the rare military genius."

military predecessors, but even in his work the implication remains that, whatever else OR was, it was exceptional to military activity.[46]

Historical work concentrating on broader issues has capitalized on this trend. Andy Pickering, for instance, has portrayed the activities of ASWORG scientists as a scientific polyp injecting itself into a military body thereby creating a hybrid science-military "cyborg", an early relic of the postmodern "World War II regime" in which we apparently now live. According to him, the formulation of search theory in ASWORG, and the reconfiguration of the military's data "archive" through the recommendation of changes in data-production procedures, are sure signs of the mixing of scientific and military activities. However, he leaves it unclear if studies unrelated to search theory were a part of this pattern, or if it would constitute a scientific infiltration if the military reconfigured its own archive, which it did far more often than OR did it for them. Certainly he is correct that the military became more technically sophisticated than it was before, but the overarching social significance is unclear.[47]

I believe it will not prove worthwhile to push too far with the question of scientific versus non-scientific modes of thought. Naturally, the fact that OR was in fact done largely by civilian outsiders makes the perspective seem sensible, but, looking at typical military activities, it is too easy to become embroiled in unanswerable questions of what constituted a scientific development, and who constituted a scientist. Given the number of longstanding but understudied technical military activities such as weapons testing or intelligence analysis, to understand the significance of OR, it is doubtless better simply to look to what OR's proponents felt was so scientific about it. Following this

[46] Kirby, *Operational Research*, pp. 30-45.
[47] Andy Pickering, "Cyborg History and the World War II Regime," *Perspectives on Science* **3** (1995): 1-48.

logic, Erik Rau sensibly views the question from a political perspective. For him, OR was a product of the interface between the interests of local policymakers and scientists who sought influence in deliberations over the use of technologies that came from their labs. The localized methodology and knowledge emerging from their negotiations were less crucial than the simple fact of their interaction in forging an identity for OR. This view serves Rau's purpose of conceptually analyzing the evolving *political* relationship between scientists and the state, but it does not address the question of what scientists so clearly saw in the *intellectual* content of OR.[48]

Scientists doing OR were, of course, aware that the exact methodology of OR was not in itself novel, even in military circles.[49] While they, and historians following them, have often sought to illustrate the importance of OR by offering clear examples of the scientific approach's ability to reform military conduct, these examples have actually distracted historians from a more important point. As Waddington pointed out,

> ...operational research is concerned, above all, with getting down to brass tacks, and its essential nature cannot be conveyed except through discussion of many particulars. The 'mythology' of operational research has tended to pick on certain items [...] as paradigms of O.R. But I should argue that the real paradigm is one which includes also all sometimes dull and unglamorous other studies—accuracy of navigation, effects of weather, radar countermeasures and counter-countermeasures, and so on.[50]

Here is the crucial point. What would have actually struck scientists as significant was that by treating military doctrine and practice as though they constituted a body of

[48] Rau, "British Origins," p. 217: "In the absence of any professional standards or techniques, [OR] practitioners from a wide variety of technical fields fell back on the techniques of their own disciplines, adjusting as necessary to local circumstances and work cultures." This point is central to Rau's argument in this paper, but I disagree with it, and argue that OR had no major relationship with academic scientific practice, and that its techniques were not foreign to military practice.

[49] For an early instance: memorandum from Ward Davidson to Vannevar Bush, 10/7/1942, *NACP*, RG 218, [UD/92], Box 57, "Operations Analysis" folder: "Operations analysis is not new except to the extent that it contemplates the use of personnel entirely free from the responsibilities of command and military administration and free to devote their entire time and energy to the solution of the commander's problems."

[50] Waddington, *OR in World War 2*, p. xiv.

scientific knowledge, they were able to explore them in a systematic way. It was how OR scientists continually studied, clarified and updated military knowledge that gave them a sense that they were bringing their scientific *epistemology* into a new realm, which is really what they meant by scientific "method". What has habitually been missing from accounts of OR, including Waddington's, is that a reliable, evolving and generally well-quantified body of military knowledge actually existed independent of OR and was growing more sophisticated alongside its rise.

The question becomes just how much of their epistemology OR scientists actually took from the military rather than from their laboratories and from the university. For Rau, the relationship was ultimately about influence and data. To obtain each, scientists had to agree to abide by secrecy requirements and to bound their research according to the desires of the command. As he puts it, "Without a basic agreement about the mission of a command, no trust could develop between a command and its OR section; without trust, no access to data; without data, no meaningful operational research."[51] Philip Morse was more circumspect. In his instructions to ASWORG members, he stressed:

> Our sole value is due to our specialized scientific ability, which makes us more useful in helping solve certain special problems, some important, some not.... We begin to be useful when we can combine with our scientific training a practical *background* gained from contact with operating personnel. This practical background can only be obtained when the operating personnel trust us and like us. [52]

Waddington was drawing closer to the crucial point when he observed in retrospect that

> the entire development of the complex and interrelated body of *scientific doctrine* was guided at every step, not solely by the scientists who did the actual thinking and calculating, but to at least as large an extent by the senior Staff Officers whose needs the scientists were trying to serve. The relation between the scientists and Staff was one of almost unblemished cooperation and trust. If this had failed on either side, Operational Research as Coastal Command knew it would have been impossible. If the scientists had not been taken completely into the Commander-in-Chief's confidence, if they had not sat

[51] Rau, "British Origins", p. 236.

[52] "Instructions for ASWORG Members," 12/1/1942, *NACP*, RG 38, Records of the Anti-Submarine Warfare Analysis and Statistical Section, Tenth Fleet [A1/350], Box 47.

> in at his most professional and confidential conferences but had been fobbed off at lower level discussions, they would have learnt only too late of the importance of many of the subjects to which they made contributions of some value.[53]

My only point of disagreement would be that the doctrine did not belong to science, but to the military—who, I argue, also did "actual thinking and calculating". In the act of planning, the military built the intellectual framework within which investigation could take place. Revising Rau's view, I would claim that OR scientists were not compelled to remain within the bounds of military interests by politics: military plans were simply the object of their study.[54]

Unlike science, which might be (coarsely) said to revolve around natural phenomena, military epistemology was defined solely by a plan, the effects expected of it, and whether a plan produced those effects *by virtue of the plan's implicit or explicit rationale*. It was precisely OR scientists' acts of navigating the framework of military knowledge, discerning weaknesses in light of plans and doctrines, designing investigations to repair those weaknesses, and ultimately making military knowledge stronger, not to mention the relative intellectual freedom they enjoyed doing it, that gave OR its qualities of systematic inquiry that they perceived as scientific method.[55] Why, then, has it not been more recognized that the military was setting the epistemological

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[53] Waddington, *OR in World War 2*, p. 246; emphasis added.

[54] Both Erik Rau and Maurice Kirby claim that such strategic concerns dominated the later, offensive stages of the war, thereby leading to conflict between scientists and the military. This claim is overstated, and seems based on the attention later given to an early controversy over strategic bombing by Patrick Blackett and C. P. Snow. Surveys of archived reports reveals that OR studies demonstrated no major shift in emphasis over the course of the war. See Rau, "British Origins," p. 219; and Kirby, *Operational Research*, p. 180.

[55] Again Waddington is introspective; *OR in World War 2*, p. viii: "[OR] is simply the general method of science employed to study any problem which may be of importance to an executive. No one would claim, of course, that such applications had never been made before the British Military Services set up their operational research sections. But it is probably true that never before had science been used by responsible executive authorities for such a thorough and such an unrestricted analysis of practical affairs as it was by the Royal Air Force from 1941 onwards. Looking back to that time from one's regained civilian world, one cannot but be amazed at the immense body of fact and theory which was built up in a few years around the war themes."

pace? We may surmise that scientists who later commented on wartime OR—who were largely academic scientists, at that time still primarily engaged in individualistic "sustained and laborious work" as Waddington put it—would not have recognized the way the military, as a collective, learned about and adapted to operations. "Assimilated experience", I argue, was not a stand-in for non-scientific intuition—it was a product of a rational heuristic process that depended on the coordination of a vast array of technical, often quantitative knowledge.[56]

Military Heuristics: Rethinking Military Thinking

We quite naturally understand warfare to be a highly technical enterprise, but we do not easily understand military thinking to be correspondingly technical or scientific. It is usually portrayed—by its proponents and detractors alike—as founded on tradition, instinct and rigid discipline, while inspired by individualistic acts of courage and heroism: all traits seemingly opposed to self-critical scientific thinking. Certainly these qualities are cornerstones of military life. Too often, though, their dominance of our perceptions leads us to portray the way military personnel think, if not as outright anti-scientific, then certainly as uncritically technological: they are either romantically attached to fighting with swords and horses, or blowing everything to hell with the biggest bomb available. Contrary to perception, though, the military had many mechanisms that allowed it to adapt intelligently to the rapidly-changing conditions of modern warfare. They are

[56] See Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997), chapter 4, for a discussion of how military practices informed postwar production of knowledge in large, cooperative physics laboratories. On the study of heuristic processes within organizations, see Herbert Simon, *Administrative Behavior: A Study of Decision-Making Processes in Administrative Organization* (New York: The Macmillan Company, 1950); see chapter four of this dissertation for a too-brief discussion of the relationship between Simon's work on administrative theory and his work in mathematical OR subjects.

simply hidden: no one ever writes about assistant chiefs of staff in charge of tactics. To lend some focus, let us call the military's means of adaptation *military heuristics*.

I would like to develop our understanding of military heuristics a little in order to make explicit certain concepts that intellectually link OR to military planning.[57] When military officers planned missions—when they chose combinations of personnel, materiel and tactics—they did so with specific *expectations* of how their choices would affect the outcome. These expectations were based on a knowledge of one's own *capabilities* as well as the capabilities of the enemy. One's own capabilities were measured using technical tests of equipment, training exercises and reports of how crew and equipment performed in tandem under combat conditions, all of which could produce measures with some quantitative precision. Knowledge of the enemy was, of course, less certain, but it was still possible to gather quality information from military intelligence: reconnaissance, espionage, signal interception, interrogation of captured enemy personnel, examination of captured equipment, analysis of enemy tactics and so forth.

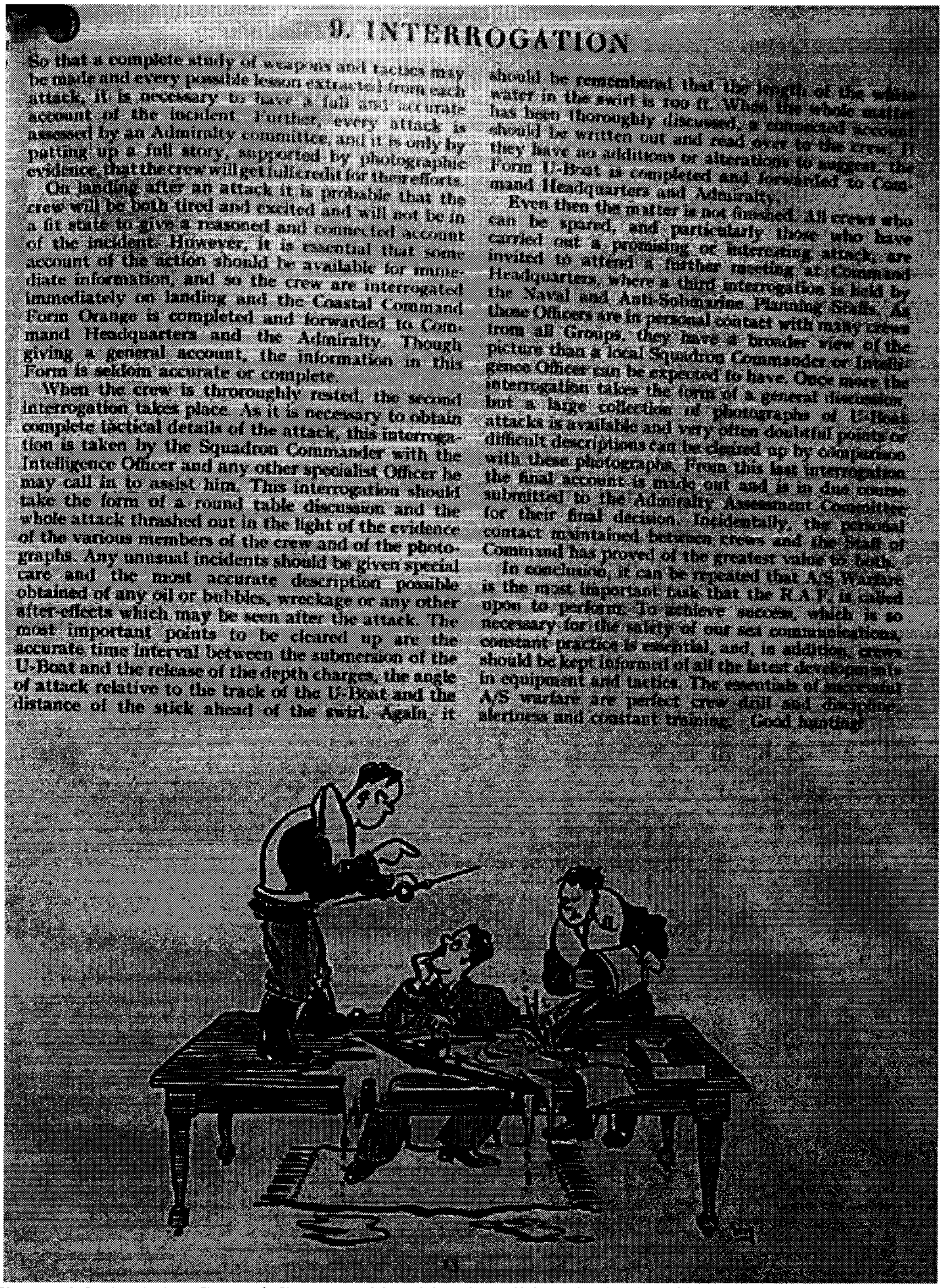
The significance of disparate forms of data had to be evaluated within the context of what was happening on operations, in a campaign, and in the war as a whole. Bodies of military knowledge so compiled were rarely expressed in any sort of collected format, partly because they were prone to frequent changes, and partly because, unlike scientific knowledge, the goal was to conduct successful operations, not to produce scholarly works. The most complete collections were usually to be found in training materials. In 1942, for instance, the British RAF Coastal Command produced a booklet for its air crews called "Submarine and Anti-submarine" summarizing the Command's combined

[57] What follows should not be taken as a generalized theory of military thought, but as an exposition of certain recurrent themes in military decision making. However, see Holley, *Technology and Military Doctrine*, for more generalized discussions of doctrine-building processes.

knowledge about German U-boat technology, tactics and methods of hunting them. Following the two elements of the title, the booklet was divided into two sections analyzing operations from the perspective of the hunted before moving on to the perspective of the hunter. The first part contained a rundown of the physical attributes and capabilities of U-boats, much of which had been obtained from tests performed on U-570, a captured U-boat the British rechristened the *H.M.S. Graph*. In light of the physical limitations of U-boat technology and known enemy behavior, the booklet spelt out U-boat strategy and tactics for traveling long distances, running away from destroyers, re-charging batteries on the surface and attacking ships. A discussion of how U-boats dived to avoid aerial attacks even included a discussion of the psychology of a U-boat crew under aerial attack. The second part contained detailed instructions of submarine hunting: how to conduct a monotonous search effectively, how to disguise the approach of the aircraft, how to target a U-boat, how to harass a U-boat after an unsuccessful attack, and so forth. The booklet was not a comprehensive treatment of the subject, boasting only 13 pages of text. It focused only on the "broad principles" of attack, which did not tend to change, leaving out details such as preferred weaponry. Some of these details would be updated through regular memoranda and instructions. Of course, some initiative was left to the initiative of squadrons and crews to deal with peculiar situations.[58]

Building this sort of body of knowledge around an operation entailed assembling a coherent picture from a diverse array of information sources. The page from "Submarine and Anti-submarine" depicted in Figure 1.3 offers a good example of how

[58] "Submarine and Anti-submarine: Notes on the characteristics and limitations of U-boats and a general guide to the air aspect of Anti-Submarine Warfare," October 1942, TNA: PRO AIR 15/155.



9. INTERROGATION

So that a complete study of weapons and tactics may be made and every possible lesson extracted from each attack, it is necessary to have a full and accurate account of the incident. Further, every attack is assessed by an Admiralty committee, and it is only by putting up a full story, supported by photographic evidence, that the crew will get full credit for their efforts.

On landing after an attack it is probable that the crew will be both tired and excited and will not be in a fit state to give a reasoned and connected account of the incident. However, it is essential that some account of the action should be available for immediate information, and so the crew are interrogated immediately on landing and the Coastal Command Form Orange is completed and forwarded to Command Headquarters and the Admiralty. Though giving a general account, the information in this Form is seldom accurate or complete.

When the crew is throroughly rested, the second interrogation takes place. As it is necessary to obtain complete tactical details of the attack, this interrogation is taken by the Squadron Commander with the Intelligence Officer and any other specialist Officer he may call in to assist him. This interrogation should take the form of a round table discussion and the whole attack thrashed out in the light of the evidence of the various members of the crew and of the photographs. Any unusual incidents should be given special care and the most accurate description possible obtained of any oil or bubbles, wreckage or any other after-effects which may be seen after the attack. The most important points to be cleared up are the accurate time interval between the submersion of the U-Boat and the release of the depth charges, the angle of attack relative to the track of the U-Boat and the distance of the stick ahead of the swirl. Again, it should be remembered that the length of the white water in the swirl is 100 ft. When the whole matter has been thoroughly discussed, a connected account should be written out and read over to the crew. If they have no additions or alterations to suggest, the Form U-Boat is completed and forwarded to Command Headquarters and Admiralty.

Even then the matter is not finished. All crews who can be spared, and particularly those who have carried out a promising or interesting attack, are invited to attend a further meeting at Command Headquarters, where a third interrogation is held by the Naval and Anti-Submarine Planning Staffs. As those Officers are in personal contact with many crews from all Groups, they have a broader view of the picture than a local Squadron Commander or Intelligence Officer can be expected to have. Once more the interrogation takes the form of a general discussion, but a large collection of photographs of U-Boat attacks is available and very often doubtful points or difficult descriptions can be cleared up by comparison with these photographs. From this last interrogation the final account is made out and is in due course submitted to the Admiralty Assessment Committee for their final decision. Incidentally, the personal contact maintained between crews and the Staff of Command has proved of the greatest value to both.

In conclusion, it can be repeated that A/S Warfare is the most important task that the R.A.F. is called upon to perform. To achieve success, which is so necessary for the safety of our sea communications, constant practice is essential, and, in addition, crews should be kept informed of all the latest developments in equipment and tactics. The essentials of successful A/S warfare are perfect crew drill and discipline, alertness and constant training. Good hunting!

Figure 1.3. An RAF Coastal Command booklet describing the body of knowledge surrounding anti-submarine ("A/S") warfare. This page offers a rich description of how various kinds of specialty knowledge are assembled *both* to establish a coherent understanding of individual incidents and to revise the body of knowledge surrounding anti-submarine warfare. In the illustration, Coastal Command crewmembers lecture Adolf Hitler on the finer points. Source: "Submarine and Anti-submarine: Notes on the characteristics and limitations of U-boats and a general guide to the air aspect of Anti-Submarine Warfare," October 1942, TNA: PRO AIR 15/155, image reproduced by permission.

crew and command worked together to develop an understanding of anti-submarine operations by combining overarching and localized perspectives. Another good example is how technical, intelligence and operational data were brought together to develop a coherent picture of enemy antiaircraft (AA) activity. Basic information about the distribution of enemy AA guns could be obtained both from photoreconnaissance and reports of AA fire (flak) experienced during the course of missions, but this information was relatively valueless without a firm understanding of what enemy capabilities entailed for operational planning. One could only say by making more detailed observations of how good the guns were, how well the enemy used them, and how much it mattered. Special expertise was required.

Following a December 1944 raid over China, an intelligence digest reported the observations of the Antiaircraft Officer of the 40th Group:

> Heavy black bursts, in greatest quantity, were seen to be continually below our formation.... The air volume covered by the bursts and the number caused two crews to think barrage fire was being used. It is possible the enemy may have realized his altitude limitation with one model of weapon and used that weapon in barrage fire. The observations of small numbers of bursts occurring at our altitude and following our course indicates at least part of the enemy's gun defenses were used in Continuous Pointed type of fire.[59]

The antiaircraft officer knew from experience what to expect from various methods of AA gunnery, and, consequently, what to look for in evaluating enemy AA tactics. He arrived at no definitive conclusions about overall enemy capabilities, but rather suggested a possible explanation that accorded with his observations.

Larger numbers of observations contained in mission reports could be amalgamated statistically and cross referenced with expert opinions, such as the antiaircraft officer's, to offer some broader perspective on emerging trends about enemy

[59] Air Intelligence Digest Weekly, Vol. 1, No. 31, 1/6/1945, p. 33; *CEL*, Box B37, Folder 2.

technology and tactics. The intelligence digest noted, following a statistical breakdown of reported encounters with flak:

> The quality and quantity [of Heavy Anti-Aircraft fire] is still below that offered by the Germans over comparable areas. But the Jap has indicated that he is quick to learn, the accuracy of his opposition increasing in proportion to the number of missions over the same area. He also shifts guns into areas subject to frequent attack to supplement his basic defense.[60]

Combining an overall picture of defenses with other factors, such as known or suspected technological capabilities of the enemy, other methods of defense, and even the overall conduct of the war, experts could make predictions about future enemy activities. Another antiaircraft intelligence bulletin noted:

> In conjunction with further employment of [smokescreen defenses], the Jap will be forced to shift existing heavy AA emplacements to areas outside and beyond the regions normally concealed by screening to maintain his present standards of heavy AA opposition without the use of gun-laying radar. This will necessitate an increase in the size and number of the gun defenses as the Germans discovered at PLOESTI.[61]

Smokescreen defenses, while useful in preventing bombers from hitting their targets, also prevented AA guns (unequipped with radar tracking devices) from aiming accurately, thereby forcing a choice. If they did move their guns from the smokescreen and obtained more of them to cover the perimeter, it would increase the duration of exposure to their fire and, consequently, the probability of damage and loss, but if they did not or could not, their use of smokescreen would limit their ability to fire on American bombers, barring the use of gun-laying radars or non-irritant smoke. In any of these cases, intelligence officers were developing expectations of what individual phenomena they might observe in the future and, just as importantly, what it meant for operational planning. Their job was to distill detailed qualitative and quantitative

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[60] *Ibid.*, p. 40.

[61] "Enemy Antiaircraft Defense Bulletin Number 8," p. 17, 1/1/1945, Headquarters XX Bomber Command, Intelligence Section, *CEL*, Box B37, Folder 2.

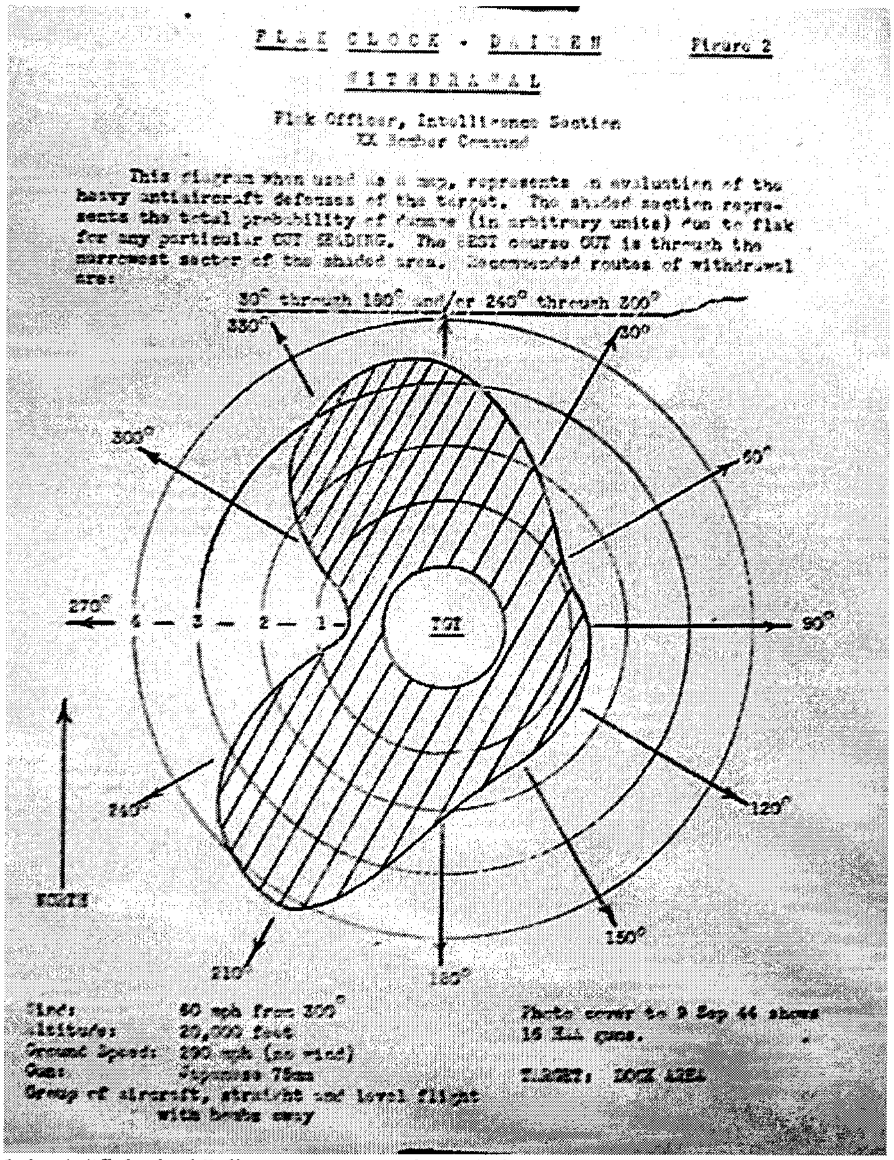

Figure 1.4. A "flak clock" diagram representing relative probabilities of various approaches to the dock area of Dairen (Dalian) China, given the tactic of flying straight and fast over the target. A separate but similar clock was prepared for outbound headings. The overall AA capability of Japanese forces over this target was considered to be ineffective. Source: Flak Intelligence Bulletin, No. C-6, 12/16/1944, Headquarters XX Bomber Command, Intelligence Section, *CEL*, Box B37, Folder 2.

assessments into simple measurements of capability that were relevant to mission planning. Military intelligence drew maps showing the distribution of AA defenses, but this data was also regularly translated into the language of the operational planner in order to elucidate its significance vis-à-vis bombing missions. The "flak clock" pictured in Figure 1.4 is a good visual example of such a translation, showing expectations for how known AA defenses around the Chinese city of Dairen (now called Dalian) would impact bombing runs of a given speed, altitude and heading.[62]

Note how expectations and observations must work on each other simultaneously. In order to make a coherent report of mission experiences as well as other kinds of data, it was important to develop a set of expectations of what the technologies and tactics employed by the enemy might look like. Of course, these expectations were, themselves, informed by Allied technical experience with AA gunnery, which made experts able to put themselves in the shoes of the enemy, to know what effect the limitations of a model of weapon might have on how one used it, and also what sort of qualitative and quantitative effects those combinations of tactics and technologies would have on the distribution of flak explosions. At the same time these expectations, if they were not being met, allowed the military to detect shifts in enemy technologies and tactics. Expectations defined what experts thought they might see, but what experts actually saw had the ability to refine their expectations as well.

These different kinds of information could not all be mastered by individual experts. Bodies of policy knowledge had to be built through the superposition of a variety of expert perspectives. Special liaison personnel, often at a high level in the military hierarchy, maintained consistent links between its parts, such as between operational branches and technical establishments. Distribution lists served a similar purpose for printed reports. Standing committees, *ad hoc* committees, and various other kinds of formal and informal meetings also served to keep policies, plans and estimates in

[62] Flak Intelligence Bulletin, No. C-6, 12/16/1944, Headquarters XX Bomber Command, Intelligence Section; *CEL*, Box B37, Folder 2.

line with available information from a variety of sources. The most basic exchange of expertise was between field personnel, who experienced a mission first hand, and commanding officers, who had an overarching view of a campaign. The interrogation meetings described in Figure 1.3 and LeMay's mission critiques later in the war were clearly one such example. Others were more wide-ranging. Meetings of the U.S. Navy's Anti-Submarine Warfare (ASW) Committee were attended by representatives from the Navy's various operational frontiers, members of the Navy's technical Bureaus of Ships, Aeronautics and Ordnance as well as members of the independent National Defense Research Committee (NDRC) and members of ASWORG. The meetings were largely administrative, but auxiliary discussions and papers presented by specialist staff members (some by ASWORG members) also played a key role in the development of ASW methods.[63]

There are no hard and fast rules to describe the heuristic process through which bodies of military knowledge changed over time. Some changes were anticipated, some knowledge was subject to routine update, in some cases the need for change was quickly ascertained, while some knowledge was only found to be faulty through bitter loss. However, we can make a few general observations. Given the fast pace of change and the administrative burdens on the military decision making apparatus, alterations of bodies of military knowledge usually took place in reaction to a deviation of results from expectations.[64] Expectations of how one's own plans would *actually* play out against the enemy could not be directly calculated *a priori* as a function of one's own capabilities

[63] Copies of the minutes of the meetings can be found in *NACP*, RG 38, [A1/350], Box 47, "ASM Conferences (2)" folder.
[64] Much of what follows in the rest of this section is adapted from William Thomas, "The Heuristics of War: Scientific Method and the Founders of Operations Research," *BJHS* **40**/2 (2007) 1-24.

and enemy capabilities.[65] Instead, deviations were often expressed in terms relating to what we might identify as two heuristic constructs: the *ideal mission* and the *typical mission*. The ideal mission was, by definition, a mission that unfolded in close accord with plans informed by knowledge of the capabilities of crews and equipment. If one knew that an average crew *theoretically could* bomb a target with a certain accuracy from a certain altitude, traveling at a certain speed, under certain flying conditions using certain equipment and thereby cause a certain amount of damage, it thereby followed that if a mission unfolded as it was planned and with no mitigating circumstances, ideal results would be achieved. How often a mission unfolded ideally depended on the difficulty of the mission and how much was known about the operation being undertaken, but it was desirable that some missions unfold ideally to prove the theoretical validity of mission plans.

In typical missions, however, things did not always go according to plan and success was often attenuated or indeterminate. As we have found from our discussion of LeMay, it was useful to obtain a thorough understanding of individual elements of such missions' narratives because they suggested both ways in which combat experience tended to deviate from ideal plans and metrics by which success could be measured. Typical deviations and measures of success so obtained defined a *reasonable* outcome for a mission given a set of circumstances. For instance, when planning a bombing operation, one could (usually tacitly) connect knowledge of capabilities with typical results to form

[65] The Lanchester Equations, developed at the end of World War I, do make crude calculations of outcome based upon the strengths of the opposing forces. OR scientists sometimes pointed to the Lanchester equations as a predecessor of their own work. See F. Lanchester, *Aircraft in Warfare: The Dawn of the Fourth Arm*, London, 1916; see P. M. S. Blackett, "A Note on Certain Aspects of the Methodology of Operational Research," a 1943 memorandum, reprinted in *Studies of War*, pp. 176-198, Appendix D, p. 198; and Morse and Kimball, *Methods of Operations Research*, pp. 63-77.

reasonable expectations by saying, "we are sending so many planes in so many groups flying at such an altitude, equipped with a certain kind of bomb with a radar of certain known capabilities as well as a well-tested countermeasure, and, given that we know the enemy has fortified this location heavily, based upon typical outcomes to such missions we can reasonably assume that we will lose a certain number of planes depending on how things play out and expect to accomplish our mission within a given degree of effectiveness."[66]

To make an analogy to science, the ideal of a mission and the ways a mission might be reasonably expected to *deviate* from that ideal formed a military theory. Its experimental counterpart was the actual performance and analysis of a mission. LeMay's critiques tested the efficacy of doctrine, equipment, training and discipline against mission performance. Whether or not a mission was successful, the goal of post-mission analysis was to understand how various factors played out during its course in order to make adjustments for future missions. If a mission came up short of expectations in some way, it was particularly important to determine why. Such a determination could be made by comparing actual mission results with an ideal mission using sets of expectations derived from typical missions as a guide. If a plane got lost from a formation, for example, it could be on account of bad flying conditions, the failure of its crew to follow procedures, the failure of the procedures to lay out an effective means for keeping planes together, a failure of the plane's navigational or communications equipment, or some combination thereof. Typical experience with these sorts of problems could suggest the appropriate choice from a number of different responses to

[66] An illustrative example of just such tacit calculations will be related in chapter three when RAND systems analyst Edwin Paxson consults with military experts to develop sets of expectations for various scenarios involving a land engagement with Soviet forces along the Ruhr River.

such a scenario: training crews harder in navigation techniques, increasing the frequency of equipment maintenance, revision of tactical doctrine or training methods, calls for improved technology, or dismissal of the incident as attributable to a unique set of circumstances or as an expected cost of flying under certain conditions.

Sometimes analysis would prompt further attention and study on a subject. If missions consistently failed to meet expectations, if a debate ensued over appropriate tactics, or if operations started inexplicably demonstrating peculiar behavior, mission experts would begin to seek out *additional* intelligence and data that might offer some illumination. Problems with technological hardware, for instance, might cause inquiries to go back to the technical personnel who were the most familiar with its functioning, or even the original manufacturer, to call for a solution. If enemy behavior did not match expectations, intelligence personnel might be consulted to see if there was any indication of a change in enemy technology or tactics. If there was some question as to how crew performed under certain conditions drills could be established to test them. In a war with quickly shifting technologies and tactics, the need for such research arose frequently. Operations research was only one facet of the rapid growth of military heuristic techniques brought on by the coming of the war.

Special Investigation in RAF Bomber Command

As we have seen, Curtis LeMay had a keen regard for carefully scrutinized mission plans and tactical doctrine. In their construction, he was supported by a large staffs of technical specialists, photographic interpreters, other kinds of intelligence analysts, a Statistical Control Unit and an Operations Analysis Section. The mission critiques at which we have already looked took place late in the war, after statistical techniques and punch card business machines had become well-integrated into many facets of military management. However, it would be a mistake to assume that the military heuristic practices embraced by LeMay were in some sense a *product* of advanced methods. Just the opposite, I am arguing that statistical control and operations analysis were specialized aids to the military heuristic process. Before returning to OR, I would like to go back several years to Britain's RAF Bomber Command and pick out a few brief examples of how they refined their knowledge about operational matters prior to the August 1941 establishment of their Operational Research Section.[67]

Although it would not be fruitful to try to identify the origins of heuristic mechanisms in the military, or to attempt a broad survey, we can identify individuals who were particularly committed to their development. Air Chief Marshal Sir Edgar Ludlow-Hewitt, who was the Commander-in-Chief of Bomber Command between 1937 and 1940, surely qualifies. Ludlow-Hewitt had a reputation for being technically astute and having a keen interest in improving the operational readiness of the command as the war approached. Sir Arthur Harris, who later led the Command's fiercest attacks on Germany, characterized Ludlow-Hewitt as "far and away the most brilliant officer" he had ever met, making note of his "immense technical ability and practical knowledge," which later served him well as the RAF's Chief Inspector until the end of the war.[68] Ludlow-Hewitt was committed to fostering the flow of information through all levels of the military

[67] We will see in chapter six that Bomber Command was assigned an "Operational Research Officer" in January 1941, but that a full-fledged section was not instituted until that August.
[68] Arthur Harris, *Bomber Offensive* (London: Collins, 1947), pp. 35-36. According to Harris, Ludlow-Hewitt was dismissed for insisting on diverting planes and personnel into Operational Training Units. Incidentally, Harris' description of the role of his Operational Research Section, although brief, is actually quite demonstrative of the day-to-day role of OR in planning.

hierarchy. In 1940, for instance, dissatisfied that decisions made at the highest level were not being received by air crews, and desiring that ideas "ventilate" up from them, Ludlow-Hewitt ordered that units conduct their own meetings in coordination with the agendas of higher-level group meetings in order to make sure that crews could make a contribution at the command level and that the decisions that were taken in light of those contributions made their way back down the hierarchy to them.[69]

Accident prevention was a consistent priority for Bomber Command. Beginning in 1931 all accidents were recorded in a "flying accident book," which also contained regular reports analyzing that quarter's accidents.[70] Pilots were required to read this book and sign off that they had done so. According to a squadron leader in 1936 minute,

> The intention in the issue of these analyses and in instituting the flying accident book is that, by drawing the attention of pilots to accidents and their causes, general knowledge is increased, pilots know what to avoid and more care in flying should result with, it is hoped, a consequent reduction in the number of accidents.

These accidents were grouped statistically by cause: those due to inexperience, carelessness, disregard for orders, and bad maintenance. The squadron leader noted a marked increase in accidents that quarter due to inexperience, which could mean pilots losing themselves in bad weather, practicing "bad judgment", and mishandling controls.[71] After he arrived at the Command, Ludlow-Hewitt praised the accident books and pressed for their perpetuation, but was dissatisfied with established accident classifications such as those due to "bad judgment" and "inexperience," preferring to make preventable causes more explicit. For instance, if a pilot was unable to handle controls in the dark, it

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[69] Ludlow-Hewitt to Group Leaders, 1/5/1940; and Senior Air Staff Officer to Group Headquarters, "Ventilation of Ideas", 1/8/1940; both TNA: PRO AIR 14/101.

[70] These reports were compiled statistically, but there was not an especially large number of incidents in any given year requiring any sort of advanced analysis.

[71] Minute from Squadron Leader, Tr. 2. to Senior Air Staff Officer, Bomber Command, 11/24/1936, TNA: PRO AIR 14/1.

would be better if the pilot learned his controls on the ground blindfolded rather than relying on further experience in the air to ingrain the necessary skill. Ludlow-Hewitt also wanted to establish a "flying accidents inquisitor whose job will be really to get down to the elucidation of the fundamental cause in all these accidents and not rest until the true cause has been uncovered."[72] Detailed accident investigation and analysis of broader trends allowed Bomber Command to connect individual incidents not only with their immediate causes, but with broader Command policies and practices and to argue for appropriate responses to accidents as well as broader changes in official policy and training.

The need to connect investigation with policy also resulted in the establishment of special tactical experimental units. Bomber Command began considering establishing a "Bomber Development Unit" in 1938 in order to perform experiments to assist the integration of new technologies into effective methods and tactics, to liaise with the Air Ministry's technical and scientific staffs and with its experimental establishments, and, finally, to elucidate "problems connected with the operation of bomber aircraft, to which adequate attention cannot be given in the operational units."[73] Investigative programs suggested for the group included as many as twenty-five different questions requiring clarification, although this tentative list was whittled down to about fourteen, and the availability of only one each of four kinds of aircraft made the unit's experimental schedule an *ad hoc* affair.[74]

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[72] Minute from Commander-in-Chief to Senior Air Staff Officer, Bomber Command, 3/10/1938, and subsequent minutes, TNA: PRO AIR 14/1.
[73] "Bomber Development Unit" from Senior Air Staff Officer, Bomber Command, to The Officer Commanding Bomber Development Unit, 11/14/1940; and Minute from Ops.2 to G/C Air, 7/21/1939, both from TNA: PRO AIR 14/190.
[74] Minute from Ops.2 to G/C Air, 7/21/1939; and "Programme of Trials for the Bomber Development Unit," 8/19/1939, both from TNA: PRO AIR 14/190.

The BDU did not begin work until November 1940 (after Ludlow-Hewitt's departure), and it was shut down early in 1941, on account of disappointing results, certain factors including winter weather handicapping the unit's activities, and the difficulty in justifying the diversion of experienced personnel and equipment from operations and training.[75] However, by 1942 Bomber Command had committed itself to a policy of bombing targets by night, and its ability to even locate targets in the dark had come under criticism.[76] The Command consulted with the noted science advisor Sir Henry Tizard, who suggested attaching a small group of scientists and technical officers to an operational station to allow them to develop the most practical methods of a large number of possible means of improving navigation. This suggestion led to the re-establishment of the re-named Bombing Development Unit.[77] While the BDU had been disbanded, though, Bomber, Fighter and Coastal Commands had all already established Operational Research Sections, which were manned by scientists and charged with undertaking studies on behalf of the Commands' Commanders-in-Chief.[78] To avoid replication of functions, Bomber Command decided to drop the idea of having the new BDU harbor its own scientific personnel, but it was agreed that the Bomber Command ORS would dispatch members of its staff to the new BDU as appropriate in order to

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[75] Commander-in-Chief, Bomber Command to the Under Secretary of State, Air Ministry, 3/26/1941, TNA: PRO AIR 14/682.

[76] This criticism was largely the result of Winston Churchill's personal scientific advisor, Lord Cherwell, who had ordered his assistant, David Butt, to investigate accuracy of Bomber Command's raids. The results were disappointing, and Butt's report put considerable pressure on the Command to improve navigational practices. We will consider Lord Cherwell and the Butt Report in chapter six.

[77] Deputy Chief of Air Staff to Commander-in-Chief, Bomber Command, 2/15/1942, TNA: PRO AIR 14/682. BDU came to stand for "Bombing Development Unit" after Bombing Tactics Development Unit and Bomber Command Development Unit fell by the wayside. The difficulty was not to represent the unit as concerned with the technology of the bomber itself, or merely the act of bombing, but rather the whole mission.

[78] Also by Tizard's suggestion, see chapter six.

monitor its trials and to integrate the results into its own studies.[79] Such overlapping of OR responsibilities with other investigative mechanisms, it should now come as no surprise, was the rule rather than the exception.

Building OR on the Framework of Military Knowledge

1. Analysis of Past Operations
1.1 Obtain the plans for Husky, Torch, Avalanche, Dieppe.
" " account of what happened.
Compare these, using some few suitable variables as functions of time.
No. of men ashore
" " vehicles shore. [sic]
Area of bridgehead occupied.
Compare actual enemy reaction with that allowed for in the plans.
Attempt to deduce degree of rigidity of a plan which is useful; also how much deliberate flexibility should be planned for.

from "Notes for an Operational Research Study of the Planning of Landings"
written by Patrick Blackett, Chief Adviser on Operational Research, Admiralty, November 11th, 1943.[80]

What, then, did OR add to the military heuristic process? Probably the most notable thing distinguishing OR from other military specialties was the synthetic and theory-laden nature of its studies. OR scientists did not simply produce information for the military; they asked and answered questions based on the rationales of existing military practices, just as high level military planners would. To do this job, they had to become *individual* scholars of how the military *hierarchy* built and revised plans based on available policy knowledge, which meant they had to understand how different kinds of information could be brought together into a knowledge framework defined by the military plan.

[79] Commander-in-Chief, Bomber Command, to the Under Secretary of State, Air Ministry, 5/15/1942; and Minutes of the 30th Meeting of the Bombing Committee, 6/23/1942; both from TNA: PRO AIR 14/683.
[80] "Notes for an Operational Research Study of the Planning of Landings," 11/11/1943, TNA: PRO ADM 219/630.

In Figure 1.5 we can see the central location of the RAF Coastal Command ORS in the coordination of the Command's (ORS-designed) Planned Flying and Planned Maintenance scheme, which coordinated equipment and maintenance personnel assignments with operational requirements. In order to keep assignments in-line with overall military goals, it was important to reevaluate the effort required to achieve those goals continuously, which meant maintaining careful liaison between specialists in administrative, maintenance, supply, and operational practices. This liaison was accomplished primarily through regular meetings of the separate staffs, but meetings only provided a limited and ephemeral encounter in which disparate kinds of information could be synthesized within the framework of the plan. However, the ORS played an advisory role in the process, and, as we can see from the color-coding in the figure

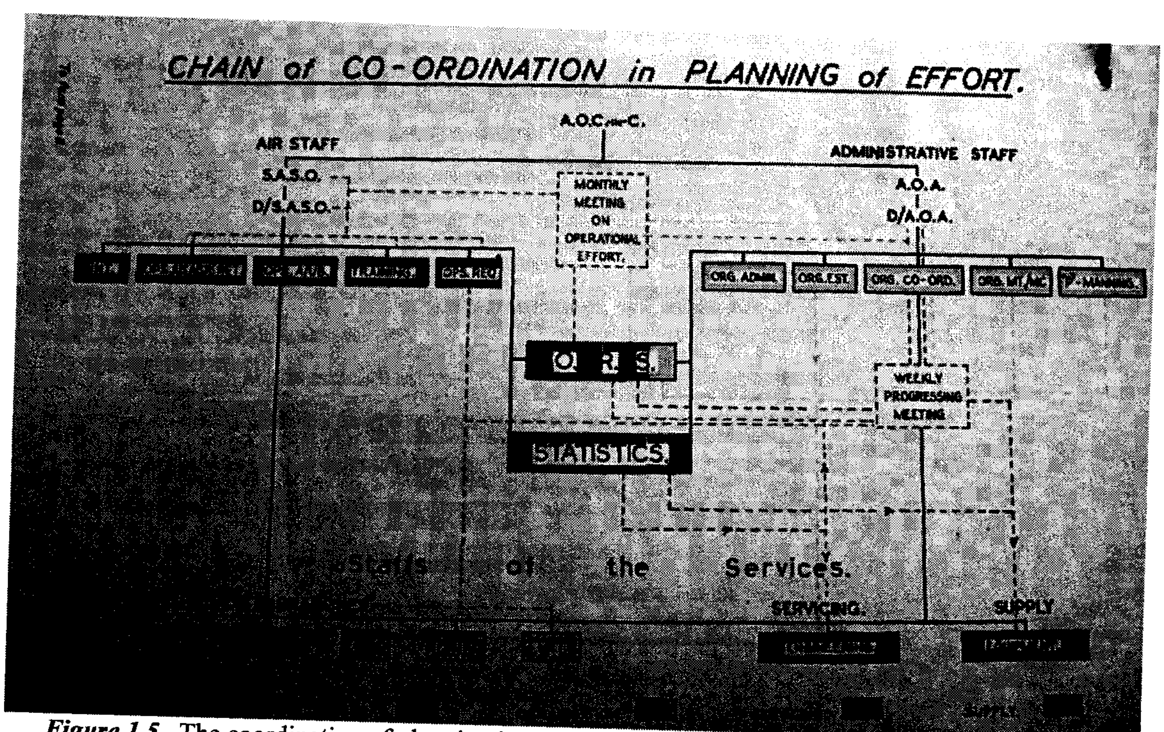


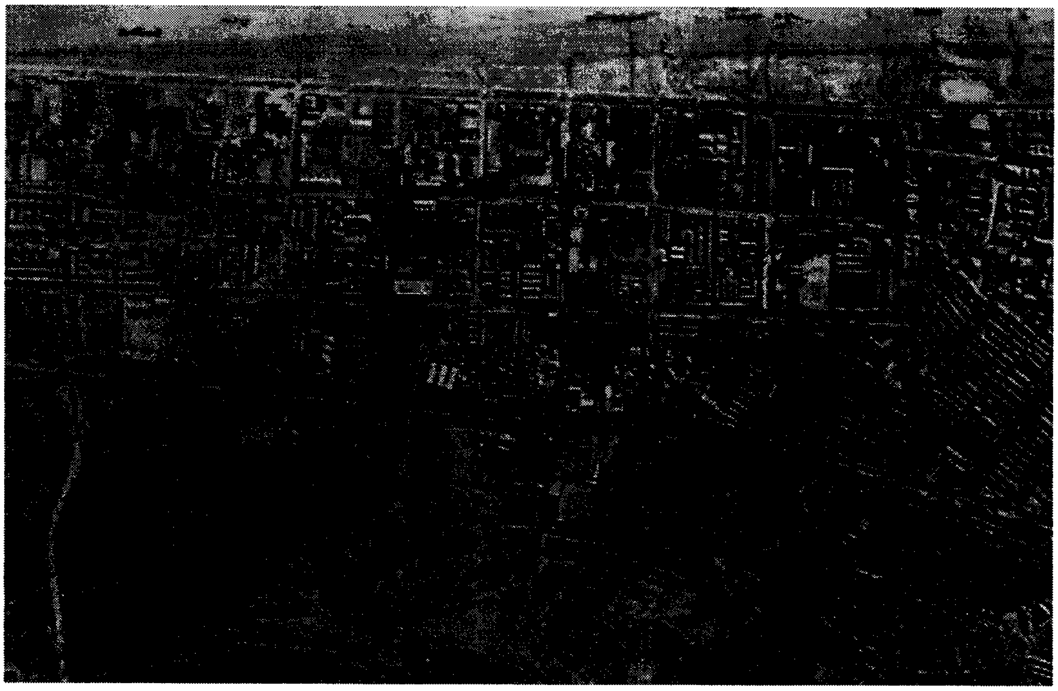
Figure 1.5. The coordination of planning in the British Royal Air Force Coastal Command's Planned Flying and Planned Maintenance scheme. Note the position of the Operational Research Section ("O.R.S.") straddling all of the various aspects of planning. Source: "Planned Flying and Planned Maintenance in Coastal Command, Part V: The Approach to a System," 8/22/1945, TNA: PRO AIR 15/154, facing p. 8; image reproduced with permission.

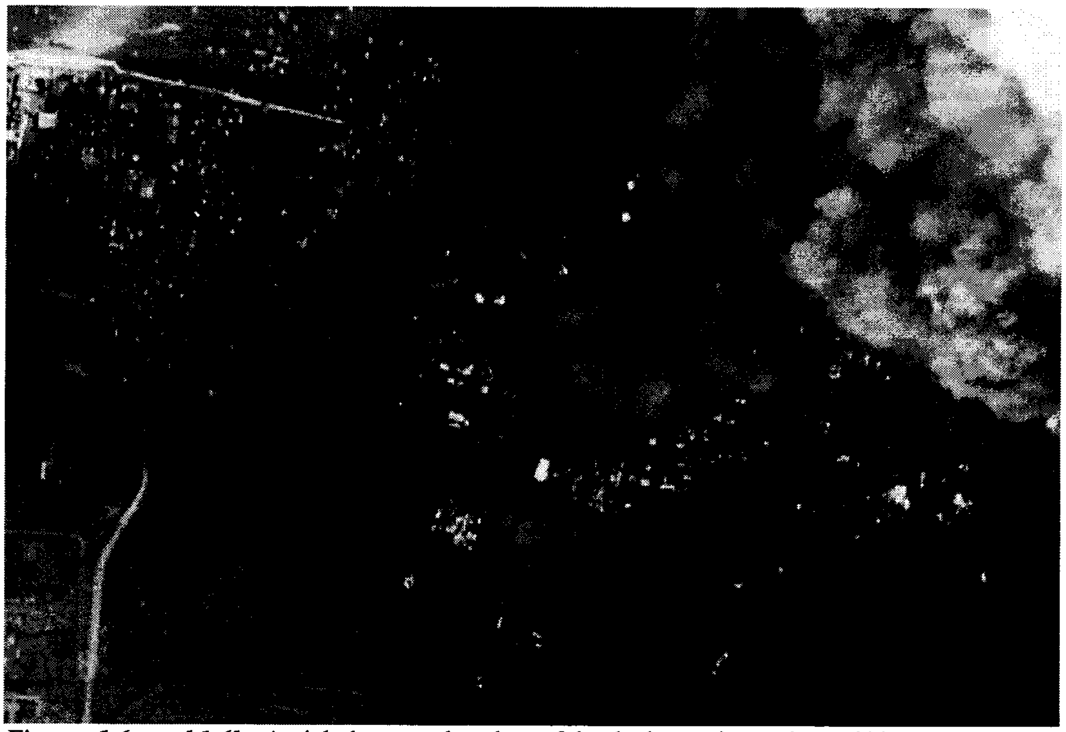
(rendered here in grayscale), it was the only organization that performed duties in all four areas of specialist expertise identified.[81] Working beyond the limits inherent to the military heuristic process, OR scientists could sustain the synthetic purpose of a meeting after it had ended, and, to inform the discussions of future meetings, they could perform what Waddington called "sustained and laborious work" or what LeMay called "long studies", and publish the results ahead of time.

The exact nature of OR's sustained and laborious work was predictably *ad hoc*, varying depending on the heuristic infrastructure already in place. In some cases, OR reports were only evaluations of how well plans with an explicit, well-considered rationale actually worked. For instance, Report No. 19 of the Operations Analysis Section at LeMay's XX Bomber Command was a study of how the planning for an incendiary raid on the dock and storage area of Japanese-occupied Hankow, China on December 18th, 1944 played out. Planning for this mission had been intricate, selecting a specific order in which "components" of the dock area should be bombed with a certain amount of force and with a certain kind of weaponry. The economic use of the target, flammability, the location of fire fighting facilities, the size and density of the buildings, combined with estimates for how much total effort could be mustered against the target, how much would actually reach the target, and how well planes could be expected to bomb once they got there, all had determined the deadly architecture of the attack.

This plan went wrong from the start. Due to difficulties of weather, it was agreed to begin 45 minutes early, but only the group due to bomb third received the message. The smoke resulting from their attack, which would have blown over areas that had

[81] "Planned Flying and Planned Maintenance in Coastal Command, Part V: The Approach to a System," 8/22/1945, TNA: PRO AIR 15/154.





Figures 1.6a and 1.6b. Aerial photographs taken of the dock area in Hankow, China at 0308Z, just prior to the first bombs hitting, and at 0338Z. In the lower photograph, the operations analysis report notes, "As in the case of Formation I and III, Formation IV's pattern is exceedingly well planned. The southern [right] portion of the pattern slightly overlaps the portion of Component D already burning fiercely and still covers the northern end of the remainder of the component with slight overlap. In the course of thirty minutes day had been turned into night at Hankow." Source: "Study of Incendiary Attack on Dock and Storage Area, Hankow, China," XX Bomber Command, Operations Analysis Report No. 19, *CEL*, Box B37, Folder 3.

already been bombed had the aiming points been hit in order, hampered the targeting of the later formations resulting in overshooting. According to strike photographs only 30% of the total bombs dropped hit any of the components of the target, let alone the assigned one, and all of these were dropped by the first four formations over the target. One of the later formations even ended up hitting the "Chinese Quarter", which was supposed to have been avoided. The report dutifully analyzed patterns of devastation and the spread of fire through the areas that were bombed, sorting out the various factors, such as density of bomb pattern, the likely presence of firewalls in buildings in targeted areas, and the distribution of firefighting facilities, that had contributed to the observed effects (see Figures 1.6a and 1.6b). Areas hit properly were almost entirely devastated. In the Chinese Quarter, on the other hand, even though the area was built-up, the fires were contained, a fact "best explained by the tremendous number of individual firefighters living in the area and by the fact that the type and tonnage of bomb superimposed on this area was neither the type nor the quantity of the bomb designed to destroy such an area." The report made a number of "pertinent observations" about the conduct of the mission, such as how important knowing wind speed and velocity were and how aiming using an offset point of reference across the river could have improved accuracy, and, of course, it admonished how important it was that plans be executed properly, and how disruptive even simple last minute changes could be.[82]

[82] "Study of Incendiary Attack on Dock and Storage Area, Hankow, China," XX Bomber Command, Operations Analysis Report No. 19, *CEL*, Box B37, Folder 3. Planning had a strongly quantitative side to it, determining expectations of damage for a certain weapon. For example: $F=\alpha(1-e^{M_I D})$ where F is the fraction of damage, α is vulnerable portion of a target (assumed 1), M_I is index of effectiveness against a kind of target in acres/ton, and D is density of bombs in tons/acre. Also, $M_I = N \times A \times P_f$, where N is number of bombs per ton, A is average area of fire [division], and P_f is probability of one bomb starting a sustaining fire. P_f was calculated differently in different situations. So for an M47 bomb, $P_f = 0.8$

In other cases, military practices were based more on acquired wisdom than explicit rationales, and OR reports simply created an analytical framework in which to discuss plans and doctrines. A good example of this process is a report issued by the AORG in January 1945 at the request of the British Army's Director Infantry entitled "Street Fighting". It had few pretensions. Its second paragraph began: "To enter a house with minimum casualties inflicted on the attackers, it is frequently necessary to blow a hole in the wall" (the enemy often had the usual entrances to a building covered by gunfire). The report combines material testing and field knowledge of tactics, listing various methods of blowing holes in walls, and evaluating the effectiveness of each in light of the fact that a hole in the wall had to be at least two feet in diameter to permit entrance to troops wearing equipment. To aid in this task, the AORG made use of specially built 9" brick walls, a house used at the London District School of Tactics, a derelict barn with 20" stone walls, and a derelict farm house with 20" brick walls.

The "standard method" of "holing" a wall, setting a pole charge against it, was effective but dangerous in that the soldier setting the charge was at risk of being shot, and if he was killed or injured the battle would be held up considerably. The report considered this method against other means of holing walls, both from up close and at a distance. One method, firing a similar anti-tank PIAT bomb against the wall, had been recommended "from various theatres of the war," although the study found that the hole produced by a single shot in its test walls was rather small—though it did weaken the surrounding brickwork as well. OR personnel found it required between two and twelve such shots to blow a sufficiently large hole in the wall, and speculated that perhaps walls

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(combustibility) x 0.95 (dud factor) x 0.90 (height factor) x 0.478 (built-up-ness) x 1.00 (conservatively assumed for all other factors) = 0.327. It is not clear who devised this "weapons analysis" scheme; see Operations Analysis Report No. 19, Appendix I.

encountered in the field were weaker, or that the effect on the morale of those in the house was "considerable". Other weapons and various tactics for using them against walls were also tested.

In addition to the problem of holing walls, the report also considered approaches to killing defenders from outside a building, killing defenders after entry had been forced, various means of setting fire to a house that all proved ineffective (even if the "house" was really a barn filled with hay), the uses of smoke, a couple of problems of weapons accuracy, and the use of wire netting over windows as a defensive measure. None of this work was especially novel, and much of it would have been known by, or at least not surprising to, field and training experts. At fourteen pages including appendices detailing trial results, the study was certainly not an exhaustive or even authoritative guide to the art and science of street fighting. The object was simply to map out certain aspects of the rationale behind certain street fighting tactics, to place what would have been a fairly reliable but largely anecdotal body of knowledge within an explicit experimental framework, and possibly to set the stage for future discussion and elucidation of problems involved in street fighting, specifically discussing the roles various kinds of available weapons could best play. For instance, the study suggested that, given a choice, it might be more useful to fire a PIAT bomb through an open window of a house to kill those within than it was to use it for blowing holes in the walls.[83]

In some cases, OR attempted to distill highly disorganized information about missions into a discussion about how desirable things that were happening given extant training and planning actually were compared to more ideal scenarios. In April 1945, the

[83] "Street Fighting," Army Operational Research Group, Report No. 167, 1/2/1945, TNA: PRO WO 291/156.

U.S. Navy's AirORG[84] released a sprawling "analytical report" on carrier aircraft activities during the Battle for Leyte Gulf in the Philippines, which had taken place from the 24th to the 26th of October, 1944. At over fifty pages, many of them taken up by statistical tables, the best way to describe the report is not so much as the analysis of a single incident, like the Hankow report, but a dissection of the airborne aspects of many different missions taking place simultaneously. The first part of the report compiles accuracy rates against various classes of naval targets, but notes that claims of hits were based only on the reports of pilots who doubtless had other things on their minds than accurately recording their actions. The report notes that accuracy data was probably exaggerated because the same hits were likely claimed by different pilots, and it later points out on the basis of limited photographic evidence available that accuracy was certainly overstated and even that "there was gross misidentification of the vessels attacked."

Nevertheless, even on the basis of available data, it was possible to reconstruct some pertinent facts (or possible facts) relating what had happened in the chaos of the battle. In particular, the study combined reports of types of tactics and weapons used against various classes of target, observations of damage caused, and known or suspected technical specifications and vulnerabilities of Japanese ships (supplied by the Navy's Bureau of Ships) to ascertain how likely it was that the kinds of hits claimed would produce the damage or destruction that was actually observed. This analysis was used to present qualification and caveat-laden conclusions about how effectively the aerial effort

[84] AirORG was the first group to break of from the U.S. Navy's ASWORG organization and was, initially, an offshoot of the Air Intelligence Division of the Office of Naval Intelligence, although it maintained close contact with its parent organization. In October 1944, it formally became a part of ORG. See "Summary Report to the Office of Field Service, O.S.R.D.," p. 10.

was coordinated versus how effectively it was said to be coordinated, and to offer suggestions for how weapons selection, target selection and strike tactics could be designed to produce a more effective attack in the future.[85]

The Functions of Statistics

By reputation wartime OR has a deep association with statistical argument. However, the role OR played in relation to statistics varied depending on the military's extant practices. In certain cases, especially early in the war, OR sections produced routine series of reports updating vital figures. The OR Group working for the British army's Anti-Aircraft Command took over the compilation of results from "ZZ" forms (detailing anti-aircraft activity) from an Air Ministry group called the Air Warfare Analysis Section (which we will discuss in the next chapter).[86] In some cases, data from reports were not being regularly tabulated at all. RAF Coastal Command's ORS, the Admiralty's OR staff, and the U. S. Navy's ASWORG, all produced regular reports detailing patrol activity by region, convoy activity, estimated U-boat activity (which was usually determined by intercepting their transmissions), and encounters with U-boats. RAF Fighter Command kept track of statistics related to detection, interception and destruction of enemy bombers over England. Such statistics helped to lend some rigor to the idea of the typical mission—just how typical was it for an aircraft flying over the North Atlantic to encounter a U-boat? to destroy it? Plotted over time, one could observe

[85] "Carrier Aircraft in Battle for Leyte Gulf, 24-26 October 1944," Air Operations Research Group, Air Research Report #18, 4/10/1945, quote on p. 50; provided by courtesy of the CNA Corporation library.
[86] See "Report on the Analysis of ZZ forms for the period August 1st – November 30th, 1941," ADRDE (ORG) Report No. 43, December 1941, TNA: PRO WO 291/41; the first non-AWAS analysis of ZZ forms.

deviations from the typical due to weather trends from deviations caused by shifts in technology and tactics.[87]

Most OR groups made a point of not being bogged down with routine work. When facilities for keeping statistics were not available, OR personnel pressed for their establishment, but they were not the only ones. The physicist Lord Cherwell, Winston Churchill's personal confidant and science advisor, established and administered statistics groups at the Admiralty (where Churchill was First Lord[88] before he became Prime Minister). When Churchill moved to Downing Street, Cherwell created an economic statistics group for the Cabinet and a special statistics group for the Prime Minister himself.[89] The most pressing demands for statistics, however, came from within the military. Most notably, the U. S. Army Air Forces, just before the debut of operations analysis, established their own behemoth Statistical Control Section with remote Statistical Control Units to handle the burden of tracking logistical planning as well as processing the mountain of reports produced by operations.[90] The Statistical Control Unit at XX Bomber Command even had a sub-section called Analysis Section. Among its duties:

1. Translates the plans and policies established by the Commanding General and his staff into quantitative terms.
2. Compares the parts of the overall plan to assure that when expressed quantitatively they are all in balance.

[87] Many such files are to be found in collections of reports from the ORS at RAF Coastal Command (TNA: PRO AIR 15/731-734) , and the Admiralty's OR reports (TNA: PRO ADM 219); and among files provided by courtesy of the CNA Corporation library.
[88] The First Lord of the Admiralty is its civilian head, and the British equivalent of the American Secretary of the Navy. The position should not be confused with First *Sea* Lord, which is the military head of the Royal Navy, and is the equivalent to the American Chief of Naval Operations.
[89] On Cherwell's sections, see Thomas Wilson, *Churchill and the Prof* (London: Cassell, 1995); and Adrian Fort, *Prof.* See also G. D. A. MacDougall, "The Prime Minister's Statistical Section," in *Lessons of the War Economy*, edited by D. N. Chester (Cambridge, UK: Cambridge University Press, 1951), pp. 58-68; and G. D. A. MacDougall, "Machinery of Government: Some Personal Reflections," in *Policy and Politics*, edited by David Butler and A. H. Hasley (London: Macmillan, 1978), pp. 169-181.
[90] See Hay, "Bomber Businessmen".

3. Develops statistical controls by which the actual performance may be checked continuously against the plan in order that when deviations occur either the plan may be revised or performance corrected.[91]

Many of these were duties that would have been performed or at least aided by OR personnel elsewhere, even after routine work had been displaced onto more appropriate personnel.

The oft-celebrated incidents when OR groups overturned a gross statistical fallacy underlying military practice were actually quite rare.[92] While the military's prior estimates of battle expectations were often only implicit in a plan or very roughly expressed, they were not wholly arbitrary, and tended to be founded on good sense. Statistics were more useful as a means of guiding thinking about an operation. When one thought one understood an aspect of a mission, one could gather statistics to see if one's assumptions were borne out. Alternatively, if one discovered an unsought statistical correlation, one attempted to find a suitable qualitative explanation. This process made planning more rational, but did not necessarily entail more detailed observation and control. In compiling statistics on the efficacy of attacks against U-boats, for instance,

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[91] "Functional Organization Chart, Statistical Section, XX Bomber Command," 9/3/1944, *CEL*, Box B14, Folder 1. Statistical control was being used increasingly to set quality standard for industrial products. The analysis section's activities essentially takes the mission as the product. On quality control, see Judy Klein, "Economics for a Client: The Case of Statistical Quality Control and Sequential Analysis," in *Toward a History of Applied Economics*, edited by Roger Backhouse and Jeff Biddle (Durham: Duke University Press, 2000), pp. 27-69.

[92] The most frequently cited and abused reference in the history of wartime OR—and intentionally avoided here—is ORS Coastal Command Report No. 142, "Analysis of Attacks on U-boats by Aircraft," 9/11/1941 (a copy is available in TNA: PRO AVIA 7/1004), which, among other considerations, observed that depth charges were being set to explode too deep, given that almost all hits would be expected to be made on surfaced U-boats, or U-boats submerged for 15 seconds or less. Most citations note that the result was counterintuitive because such situations constituted only a "few" U-boat encounters. The original report reveals such encounters actually constituted a full 40% of U-boat sightings, and that the recommendation to use shallower depth charges had already been made as early as December 1939. Blackett himself pointed out in 1953 that the argument was a restatement of military wisdom to shoot at good targets and ignore poor ones, P. M. S. Blackett, "Recollections of Problems Studies, 1940-1945," reprinted in *idem*, *Studies of War*, pp. 205-234, on pp. 215-216; Blackett's essay is a major source of the perception that OR personnel were destroyers of statistical fallacies. See also Charles Goodeve, "Operational Research," *Nature* **161** (1948): 377-384. Waring, *Taylorism Transformed*, p. 21, presents the study as a direct calculation of optimal depth charge settings rather than a result of a survey of the anatomy of the attack.

OR groups scored fractions of kills for attacks where the result was judged to be "probably sunk", "probably seriously damaged, may have sunk", and "seriously damaged and had to return to port". So, if, after evaluation by experts, a U-boat was judged to have been probably sunk, the Coastal Command ORS would have tabulated it as 4/5 of a sunken U-boat—an absurdity perhaps, but a useful fiction producing expectations for how often an attack was likely to result in a sinking without actually being able to determine definitively whether any individual attack had resulted in a sinking.[93]

OR scientists also had to be aware of the possibility that phenomena were a figment of data production methods, and did not actually represent any operational reality pertinent to planning. Questions on forms, for example, if not stated carefully, could produce inconsistent or misleading data. In one case, researchers attempting to understand how often disappearing radar contacts represented submerging U-boats suspected a dearth of data on contacts tracked for less than a mile was due to radar operators not bothering to record brief contacts.[94] Similarly the physicist Patrick Blackett related after the war how anti-aircraft kill rate discrepancies along the coast and inland had been resolved by discovering that inland results were confirmed through reports of wreckage whereas kills over water were obviously not. He portrayed the issue as an instance of examining operations scientifically, but as usual we must be careful. In September 1940 military personnel were busy attempting to estimate for themselves how kills made over water skewed results, whereas a year later the RAF Fighter Command

[93] See "Air Offensive against U-boats in Transit," ORS Coastal Command Report No. 204, 10/12/1942, Appendix I, TNA: PRO AIR 15/732. See also "Trip Diary" of Morse and Shockley, entry for 11/24/1942: "They believe that these percentage ratings on the assessments have a certain amount of statistical significance. For instance, they mention that the net number sunk, which they compute using these percentages, turns out to be roughly equal to the net number actually sunk, which they obtain by espionage."

[94] "Disappearing Contacts with Mk. II. A.S.V.", ORS Coastal Command Report No. 226, 4/10/1943, TNA: PRO AIR 15/732.

ORS was still puzzling over the issue, suspecting the answer to the statistical discrepancy might be in the effects of geography.[95]

Like evolving doctrines, OR studies were not used as one-off statements of operational facts. Rather, they represented a distillation of policy knowledge that grew more nuanced and competitive with officers' more tacit policy knowledge. We can see this sort of process in the differences between two studies the RAF Fighter Command ORS did on nighttime "intrusion" tactics against enemy bombers. The most direct form of acting against German air raids was to attack defensively with fighter interception and anti-aircraft fire while the enemy was over Britain. Intrusion tactics, on the other hand, called for sending small groups of fighters on patrol over France to attack bombers on their return to base, and to bomb enemy aerodromes at the same time. As Fighter Command ORS Report No. 267, issued in November 1941, noted, the relative value of the two tactics had "become the subject of some controversy." The British home defensive effort was sizeable, entailing over 5,000 sorties over a six month period, whereas intrusion entailed only a couple of hundred sorties. Comparisons of effectiveness were not obvious, but parsing the statistics already collected detailing effort made and results achieved produced a revealing comparison: it took eleven intruder sorties versus thirty-two defensive sorties to down a German plane in the first ten months of 1941.[96]

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[95] See Blackett, "Recollections," p. 211; unlabeled, unsigned note, 9/16/1940, TNA: PRO AIR 16/166; and "The Trend of Air Defence at Night", ORS Fighter Command Report No. 234, 8/24/1941, TNA: PRO AIR 16/1043.

[96] "Intruders: A Statistical Report comparing the Relative Success of British and German Intrusive Efforts, with reference to British Defensive Success", ORS Fighter Command, Report No. 267, 11/8/1941, TNA: PRO AIR 16/1043.

A subsequent report, No. 326, issued in April 1942, approached the issue with more sophistication, observing, "A simple examination of success achieved, measured in enemy aircraft casualties, is not enough to determine the overall worth of an operation. There are sure to be less obvious or even intangible factors to be taken into account." In this case, it was found that while defense and intrusion tactics for twin-engine fighters at night had been equal, since German bombing raids had tapered off over the latter half of 1941 twin-engine fighters using defense tactics were, thanks to radar information from ground bases, three times more effective against German planes than intruders, and four times if intrusion losses were taken into account. Intrusion patrols from single-engine craft, though, were still extremely effective, in part thanks to the fact that German planes still flew with lights on as they approached their aerodromes. However, the reduced size of German effort had also made an impact on these planes' success. Parsing the statistics over time periods, as German raids were broken off in the latter half of 1941, intrusion success rates dropped dramatically, particularly (the report speculated) because German bombers did not have to employ bases within fighter range. Intrusion was also becoming much more dangerous, as sorties per loss of single-engine craft decreased from 58 to 9.25, while the defensive figure increased from 104 to 130. Still, the report remained optimistic about adapting intrusion tactics to new uses, such as catching bombers on take-off, and staging interceptions using German navigational beacons, and it offered several characteristics of a successful intrusion sortie, privileging aircraft endurance, maneuverability and armament over high speed.[97] Most of this material would not have

[97] "Intrusion. The Value of Intruders compared with Defensive Fighters for Night Defence", ORS Fighter Command, Report No. 326, 4/9/1942, TNA: PRO AIR 16/1043.

been especially surprising, but it did provide a firmer intellectual foundation on which to base future policy debates.

More advanced methods should be seen in a similar light. For instance, the body of "search theory" formulated by mathematicians at ASWORG combined expectations of search efficiency with geometrical arguments to produce the likelihood of encounter between objects making stated movements. The expectations thereby produced, though, would not represent an actual expectation value that one could necessarily replicate by averaging operational results. Rather, deviations from these values would indicate ways in which actual operations deviated from the assumptions that had produced the numbers in the first place. It was more the gaps in the study than the study itself that made it possible to compare it to less explicit methods, and to provide a framework for further refinement of both study and practice. We will further discuss search theory in light of its perceived relationship to equipment and tactic design techniques in the next chapter, and as a heuristic tool in Chapter Four.[98]

Component Studies

The examples of OR work offered in the previous two sections were overall studies of a particular kind of operation. Another common function of an OR study was to take a single, poorly understood component of an operation, and clarify it, thereby permitting more informed overall planning. In the course of ordinary military heuristic practice, details of mission performance often remained hidden because if a plan seemed reasonably robust with respect to them, there was little incentive to understand that

[98] The most complete discussion of wartime search theory is Bernard Osgood Koopman, *Search and Screening: General Principles with Historical Applications* (Elmsford: Pergamon, 1980).

mission component more explicitly. Even still, it could be useful to gain detailed information about how these mission components affected overall mission performance, and while OR was far from unique in making such studies, the selection and investigation of critical mission components was a major feature of OR's wartime—and postwar—identity.

The No. 10 ORS, a field unit of the British Army Operational Research Group, traveled on a rotating circuit through its Asian jurisdiction. Before their return to one of their stops in 1944, they sent out a request for suggestions for research into "facts that will be of use when conducting further operations" with a preference for long-term problems, and offered a list of "useful facts" from the previous season's research, including what Japanese weapons caused the most casualties, what percentage of casualties were likely to be saved by wearing helmets, and the inadvisability of leaving behind any first line small arms ammunition to reduce loads. The response received was enthusiastic, and resulted in the compilation of a list of thirty-four suggestions for investigation. These suggestions offer intriguing glimpses into the problems inhabiting the rationales of broader planning practices, ranging from "the value of issues of dehydrated meat to Indian troops" paying attention to such pertinent points as "what types and classes refuse to eat it" and whether the refusal to eat was "religious or prejudice", to a study of the efficiency of the "present system for evacuation of repairable and unserviceable stores".[99]

[99] See G.Research, "Problems for Operational Research Section," September 1944, TNA: PRO WO 203/716; and "Problems for No. 10 O.R.S.", October 1944, TNA: PRO WO 203/716, Appendix A. A list of problems actually studied in the Southeast Asia Command (SEAC) between 1944 and 1947 reveals just as much variety. A few selections: From February 1947, "The times and space problem of a mechanised column operating in hilly country against an enemy using guerilla tactics – A study of the theoretical problems involved with movement of this kind. A modified form of movement table is evolved from which written orders can easily be compiled." From May 1944, "Investigations regarding the rates of

Some problems involving specific mission components were far more developed. All three RAF Home Commands released a large number of OR reports relating to the evaluation of various equipment used for navigation, radar detection, and so forth. At RAF Coastal Command gauging the efficiency of search tactics and radar equipment provided regular research fodder. Anti-surface vessel (ASV) radar equipment and training changed steadily throughout the war, so knowing just what kind of results could be expected from radar, and of course why, was important for gauging how to assemble an effective doctrine for searching for U-boats.

Figure 1.7 is a chart showing the differences between theoretically ideal Long Range Anti-Surface Vessel (LRASV) radar efficiency plotted against altitude of the search aircraft and typical operational results. The report offered some tentative reasons for the discrepancies. For instance, the higher than theoretical search efficiency at lower altitudes was not a technical issue. U-boats that would have been seen by radar at short ranges actually tended to be detected visually instead, and so would not have counted in the average, leaving only U-boats spotted at long range to weight the curve in that direction. The severe discrepancies at higher altitudes presented more problems, and ultimately there was not enough information to make more than educated guesses. Even the translation of operational data into a curve representing how much of the ocean could be considered "swept" presented a host of caveats. The curve did not have a simple ontological interpretation; it was an expectation value that did not represent the expected

wastage of war materials (Jungle Warfare) – A detailed inquiry into the expenditure of all kinds of equipment, ammunition etc: under the varying operational conditions found in SEAC." From February 1945, "The utilisation of hospitals – A detailed study of the hospital layout in India and Burma with special reference to the evacuation of casualties and their length of stay in hospital." See "Operational Research Reports from SEAC 1944-47," Army Operational Research Group Memorandum No. B5, January 1951, TNA: PRO WO 291/1199.

results of any given mission. When making actual plans it would be necessary to consider the

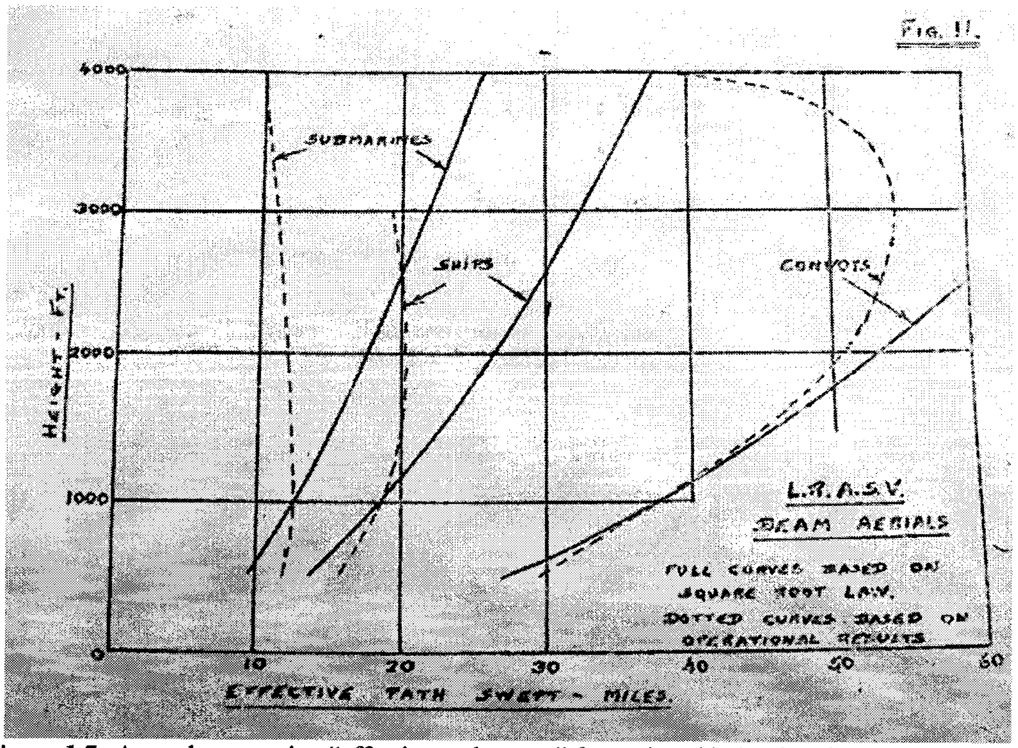

Figure 1.7. A graph comparing "effective path swept" for various kinds of surface targets from an aircraft at various heights using Long Range Anti-Surface Vessel Radar beam aerials (a separate graph was compiled for forward aerials). Effective path swept was a measure developed to indicate on average how much surface area of an ocean a plane could consider covered after passing over it. Source: "Disappearing Contacts with Mk. II. A.S.V.", ORS Coastal Command Report No. 226, 4/10/1943, TNA: PRO AIR 15/732. Image reproduced with permission.

variance in the efficiency of individual radar sets, the effect of the state of the sea in producing interfering radar return signals, and other factors such as the experience of the radar operator. However, like the aforementioned method of counting the number of U-boats killed, the curve did not have to represent a real quantity to be of use. Representing an expectation value for a typical mission, it could be used as a better *benchmark* than the theoretical values, thereby permitting better planning and evaluation for particular missions.[100]

Such expectation values could prove useful in analyzing other components of an operation. A separate Coastal Command study analyzing instances of disappearing radar contacts noted that the range distribution on first pickup of those contacts that later disappeared (and were known not to have other clear causes) appeared to adhere to a cube root law, which was consistent with observed radar efficiency, and that the range at which they disappeared adhered to the more ideal square root law, which would have been consistent with expectations of a U-boat equipped with a radar detector that submerged once the signal achieved a definite strength. On this quantitative basis, the report was able to suggest that if this was indeed what was happening aircraft could home in on the U-boat by diving at a rate that would decrease the signal strength faster than the decrease in range would increase its strength, thus fooling the U-boat into thinking that the plane had not detected it and was heading away. Of course, the use of this tactic at night would depend on having a reliable radio altimeter to prevent crashing into the ocean. In any case, the data appeared to confirm that U-boats were detecting radar signals, and that since radar only gave a 20% increase in efficiency over visual detection, radar should not be used in daylight. The report was not necessary for arriving at a useful plan, but, following the goal of all OR, it made the planning process much better informed and, thus, more robust.[101]

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[100] "A Review of A.S.V. Performance", ORS Coastal Command Report No. 201, 10/14/1942, TNA: PRO AVIA 7/1005.

[101] "Disappearing Contacts with Mk. II. A.S.V.", ORS Coastal Command Report No. 226, 4/10/1943, TNA: PRO AIR 15/732.

Conclusions

Throughout World War II all branches of the British and American militaries grew rapidly in size and sophistication, and they took many steps to expand their ability to assess the adequacy of their plans and doctrines, and to revise them accordingly. Operations research was one such step. What made it a unique approach was not the fact that it was scientific, *per se*, but rather the fact that OR scientists were able to address entire bodies of military knowledge in much the same way as they would a scientific body of knowledge: finding its weak points, developing theories about them, and then testing these theories based on *all* available forms of evidence. Of course, their theories were bound by the need for relevance to the ordinary planning process. Further, observation was the responsibility of personnel undertaking operations, and the power of experimentation was held by military planners alone. Therefore, OR scientists became scholars of how field personnel established narratives of operations, how technical experts deployed equipment and interpreted data brought back from the field, and, above all, how military executives consolidated this information into an acceptably coherent body of knowledge. Those who wrote about the requirements for OR personnel argued that the most important qualifications were scientific training of any kind, and that they have a personality allowing them to communicate easily with military officers and field personnel.[102]

Thus, I have argued, the epistemology and legitimacy of OR were not built on science, but on existing military heuristics. The great majority of operations research

[102] This requirement has been stated many times in many contexts. For instance, see letter from Solly Zuckerman to Henry Tizard, 5/22/1943, *HTT* 311; Thiesmeyer and Burchard, *Combat Scientists*; and the last paragraph of Morse and Kimball, *Methods of Operations Research.*

work did not require knowledge of any advanced methods. All that was required was a methodical mind that was comfortable working with numbers. Scientists, obviously, qualified, but so did others within and outside the military. Of course, there were also other reasons why scientists proved particularly useful. They were familiar (or could easily familiarize themselves) with the workings of sophisticated equipment—and as we will see in the next chapter, much of the mathematics that they did use involved adapting equipment design principles to field requirements. However, if we want to understand the significance of OR as the scientists who participated in it understood it, we should subordinate a study of its techniques to the more general idea that the arbitrary aspects of planning could be reduced, and that, as a consequence, the planning process could be made more robust. To OR scientists, battling arbitrary assumptions was the essence of scientific method, and it was their successes in contributing to this battle that remained with them after the last bombs had been dropped; not any particular techniques they deployed, or direct influence over policy that they had achieved.

CHAPTER TWO

Technology and Technique: OR and the Scientific War Effort

During most of World War II operations researchers primarily saw their work as a corollary to operational planning, but OR's origins and its identification with scientific activity both owe a strong debt to its relationship with technological research, development, design, testing, and training activities. In addition to making military planning more robust, throughout the war OR served as a mediator in the dialogue between technology designers and those who developed techniques and tactics for technology use. Of course, OR had by no means created this dialogue. Over the previous century, the growth of large technological systems—especially the iconic railroad, electrical, telegraph, and telephone networks—made the production of system components into an increasingly planned process of innovation and implementation that depended on liaison between inventors and designers on one hand, and users on the other.[1] This trend was certainly accelerated by the pressing demands of industrialized warfare, and World War II brought these demands to unprecedented levels.[2] The massive scale of the war, restrictions on material resources and trained personnel, the pressures of

[1] On some of these technologies, see Thomas P. Hughes, *Networks of Power: Electrification in Western Society, 1880-1930* (Baltimore: The Johns Hopkins University Press, 1983); David E. Nye, *Electrifying America: Social Meanings of a New Technology, 1880-1940* (Cambridge, Mass.: The MIT Press, 1990); and Wolfgang Schivelbusch, *The Railway Journey: The Industrialization of Time and Space in the 19th Century* (New York: Berg, 1986 [1977]); Daniel R. Headrick, *The Tentacles of Progress: Technology Transfer in the Age of Imperialism, 1850-1940* (New York: Oxford University Press, 1988).

[2] The world wars were surely crucial events in the evolution of the relationship, but warfare had made its impact felt on technological development and design much earlier; see Ken Alder, *Engineering the Revolution: Arms and Enlightenment in France, 1763-1815* (Princeton: Princeton University Press, 1997).

evolving measures and countermeasures, and the introduction of novel technologies, most notably radar, stretched the extant planning infrastructure beyond its limits.

The point where this infrastructure tends to be able to stretch the farthest and the quickest is in technique. Where technology fails to transcend its status as a physical object and serve its role as a means to an end, technique must bridge the gap. Traditionally in the military, the production of technology and the development of technique remained fairly separate. Some technologies were developed and produced by private manufacturers with their own facilities, while others were designed by workshops affiliated with the military. Training and tactics, on the other hand, were clearly military concerns that were developed in certain locations where experience could converse with the experimental: testing ranges and military schools. However, as technological development diverged from a relatively self-evident state-of-the-art and became more clearly tailored to specific operational requirements, the intertwining of the development of technologies and techniques necessitated increasingly frequent and effective liaison between equipment designers and operational personnel. OR was one of many responses to this necessity.

In this chapter we will turn our attention slightly away from OR to see how other scientists involved in the war effort used it to facilitate their dialogue with the users in the field. In Britain OR was virtually inseparable from the proliferation of scientific advisers who were charged with helping the military adapt technology to field use, and helping officers formulate their requirements for new equipment. It provided them with the clear knowledge of operational realities they needed to offer effective counsel to military executives. In America, scientific advice was never systematized to the extent it was in

Britain. There OR had a real but much more idiosyncratic relationship with advice and field observation of equipment use. Liaison was only half the story, though. Mathematics became an increasingly powerful aid in the design of tactics as well as of equipment, but in order for mathematics to add much to design, detailed knowledge of typical field scenarios had to be obtained. Much of this chapter will be devoted to examining how OR helped designers break down the barrier between technology and technique by abetting the proliferation of what we might call *intermediary technologies*.

Intermediary technologies can inhabit any point on the spectrum between a physical machine and the action of the human mind and body, and they may be very simple in nature. To operate or even ride on a railroad, it is necessary to have coordinated schedules. Telephone networks require a combination of switchboards, identifying numbers, and human (or, later, automatic) operators to route calls properly. It might at first seem heavy handed to claim that training regimens represent an intermediary technology, but the increasing encapsulation of training principles in mechanical "trainers" or simulators suggests otherwise. In fact, the functions of all kinds of simple technologies such as schedules, calculating tables, nomograms, slide rules, situation plotting boards, and even instruction booklets have been regularly incorporated into gears, cams and circuitry embedded within machines as soon as, or even before, the physical mechanisms of the technology became sophisticated enough to handle the burden. Located so close to the military's operations, wartime OR groups were frequently employed as field technical specialists who could give information to developers of intermediary technologies before full automation could be made practicable.

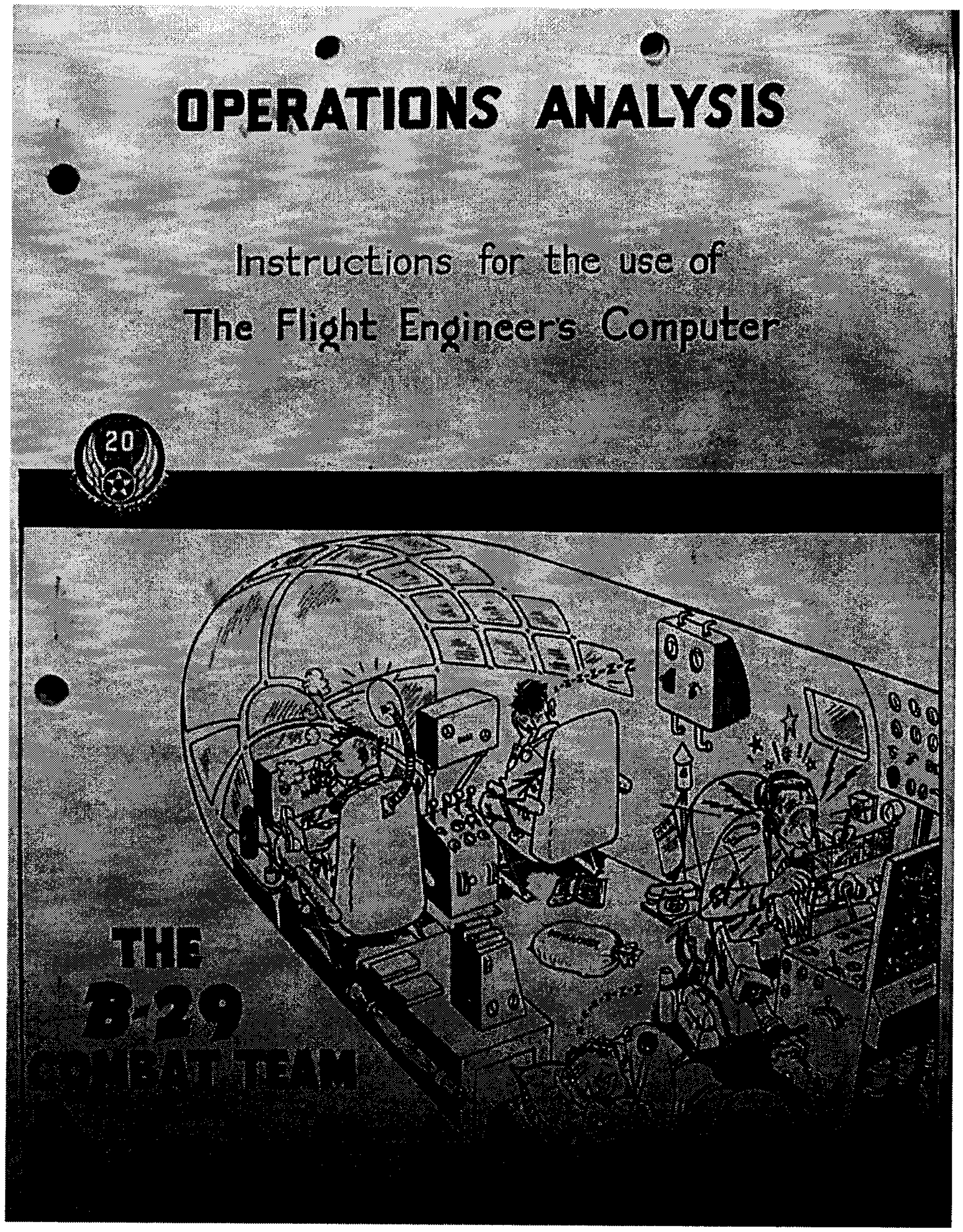

Figure 2.1. An instruction booklet prepared by the United States Twentieth Air Force's Operations Analysis Section for use of the flight engineer's "computer", which they had designed. The computer is the slide rule being held by the overburdened flight engineer, and is an intermediary technology between him and his console. The instruction booklet itself is an intermediary technology between the flight engineer and his computer. The acknowledgements make clear that the knowledge going into the design of the computer was assembled from many sources. Source: "Instructions for the Use of the Flight Engineer's Computer," Operations Analysis Section, Twentieth Air Force, Report No. 27, *CEL*, Box B38.

Sometimes they even designed makeshift intermediary technologies themselves (see Figure 2.1).[3]

Ultimately, we will find that the ever-shifting divide between technology and technique bore certain resemblances to the line between science and planning examined in chapter one. Just as every doctrine or plan could be said to harbor its own codified rationale, whether that rationale was explicit or implicit, so every technique could be said to harbor its own technical rationale, whether the principles behind it were understood or not. If those principles could be expressed explicitly, and preferably mathematically, they could be manipulated and improved in abstract form, and then retranslated back into application, which might mean making alterations in training programs designed to hone instincts and skills, or it might mean designing technology in a new way to accommodate current instincts and skills. Although mathematical design was prevalent throughout private and military R&D facilities, the last half of the chapter will be dedicated to two unique mathematical groups at the vanguard of technological and tactical design: the British Air Warfare Analysis Section, and the prewar work of its head, L. B. C. Cunningham; and the American Applied Mathematics Panel, which was headed by Warren Weaver, a doyen of the American scientific and mathematical communities.

The Organization of Science and Science Advisors

In most circumstances, technological research, development and design met operational requirements through established liaison channels. Military officers in charge

[3] Erik Rau has also emphasized the importance of the *lack* of automation in the development of OR; see Erik P. Rau, "Technological Systems, Expertise, and Policy Making: The British Origins of Operational Research," in *Technologies of Power*, edited by Michael Thad Allen and Gabrielle Hecht, (Cambridge, Mass.: The MIT Press, 2001), 215-252, esp. p. 223.

of procurement formulated requirements by consulting with technical and tactical specialists and officers in charge of establishing overall military strategy. Procurement officers, in turn, consulted with budget controllers and equipment designers to establish development and production contracts. A great deal of research and development was done in private laboratories of large military contractors, but the military had its own laboratory facilities as well. The Royal Aircraft Establishment at Farnborough, the Admiralty Research Laboratory and the Royal Arsenal at Woolwich in Britain; and the laboratories run by the Naval Bureaus of Ordnance, Ships and Aeronautics, and the Army Air Forces' experimental establishments at Wright and Eglin Fields in America were important names to anyone with an interest in the development of military equipment and tactics. The design of equipment for production was a tightly regulated process, sometimes frustratingly so, but the regulations were intended to ensure that all equipment could be harmoniously incorporated into military practices. The most effective means of coordinating laboratory work with the intended use of research products was through permanent and *ad hoc* committees that brought developers of tactics and research supervisors together. Notable examples include the Ordnance Board in Britain, and the National Advisory Committee on Aeronautics (NACA) in the United States.[4]

When the war came to Britain these channels remained the basic means through which research and development were coordinated. When they needed to be supplemented, new liaison officers, boards and committees were usually established.

[4] On the need for further study of the everyday infrastructure of research and development, see David Edgerton, *Warfare State, Britain 1920-1970* (New York: Cambridge University Press, 2006). On British military research and development, see M. M. Postan, D. Hay and J. D. Scott, *Design and Development of Weapons: Studies in Government and Industrial Organisation* (London: HMSO, 1964). I do not know of comparably detailed histories for the American case, but see Nick A. Komons, *Science and the Air Force: A History of the Air Force Office of Scientific Research* (Arlington: Office of Aerospace Research, 1966); and Harvey M. Sapolsky, *Science and the Navy: The History of the Office of Naval Research* (Princeton: Princeton University Press, 1990).

However, in June 1940 the American government took the relatively radical step of supplementing its extant research facilities, committees and offices through the establishment of an entirely new organization called the National Defense Research Committee (NDRC), which was presided over by the electrical engineer Vannever Bush, who was the chairman of NACA and the new president of the Carnegie Institution of Washington. The NDRC was supposed to mobilize civilian scientists and engineers who were not already working for military suppliers and to coordinate new research projects across a wide spectrum of fields. It reported directly to the White House. In June 1941 this organization was expanded into an even more powerful and better-funded Office of Scientific Research and Development (OSRD). As Bush took over leadership of the OSRD, the NDRC, which became a subsidiary organ within it, was taken over by Harvard University president James Conant. High level liaison officers were charged with coordinating an overall research strategy. Individual OSRD research projects also had project liaison officers, who were not always suited to interacting with scientific researchers, and most contact between the various research divisions and service personnel took place through less formal channels.[5]

Meanwhile, in addition to augmenting their own technical staffs, the British armed forces began to employ non-military scientific advisers directly—a need that became more acute once the Ministry of Supply was separated from the War Office in 1939, and the Ministry of Aircraft Production (MAP) was split off from the Air Ministry

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[5] On coordination of work in the NDRC and OSRD, see Irvin Stewart, *Organizing Scientific Research for War: The Administrative History of the Office of Scientific Research and Development* (Boston: Little, Brown and Company, 1948).

in 1940.[6] Sir Henry Tizard, the chair of the Aeronautical Research Committee and the newer Committee for the Scientific Survey of Air Warfare, was a scientific adviser to the Chief of Air Staff between late 1939 and early 1940. From late 1940 until 1943 he was scientific adviser to MAP. In addition to other posts he held with respect to radar, Sir Robert Watson-Watt was Scientific Adviser for Telecommunications between the end of 1939 and 1950, although by the end of the war, this post had already become quite redundant.[7]

At a lower level, special units were set up within both British and American hierarchies that were staffed by experienced personnel and charged with the development of new kinds of tactics and the incorporation new kinds of equipment into missions. These units had frequent contact with laboratory personnel, and often supplemented the work of committees. Some long-standing facilities were closely connected to R&D activities. The *H.M.S. Vernon*, for example, was an Admiralty facility dedicated to developing equipment and tactics for use in mine warfare.[8] Others were creations of the war, and liaised with laboratories and coordinating committees. The RAF's Air Fighting Development Unit (AFDU), for example, supplemented the work of the Air Fighting Committee, which brought together high officers in charge of tactical development, officers from the operational commands (including OR personnel), and research and development staff. The AFDU was accompanied by the Bombing Development Unit,

[6] The War Office was responsible for the Army, while the Air Ministry was responsible for the Royal Air Force. The Admiralty retained control of its supply branches.
[7] Watson-Watt's most important work, by far, was done prior to his obtaining high-level positions; see David Zimmerman, *Britain's Shield: Radar and the Defeat of the Luftwaffe* (Stroud: Sutton Publishing Limited, 2001).
[8] There are no histories of *H.M.S. Vernon* itself, but see the Gerald Pawle, *The Secret War, 1939-1945* (London: Harrap, 1956).

discussed in the last chapter, and a Coastal Command Development Unit, that was later called the Air-Sea Warfare Development Unit.[9]

It was also found that there were virtues in bringing experienced scientists in to study equipment performance in actual combat environments. When radar was first developed in the years leading up to the war, in order to route fighter aircraft to intercept invading bombers it was necessary to set up a series of radar stations, to staff them with skilled radar operators and plotters, and to establish communication relays to skilled fighter pilots. The development of this system in Britain was expedited by involving personnel from the Air Ministry's radar laboratory, (eventually called the Telecommunications Research Establishment, TRE),[10] in all stages of the process, and the British term "operational research" was first used to distinguish this field work of scientific personnel from ordinary laboratory work. The Stanmore Research Section was established in 1939 at the RAF's Fighter Command Headquarters in the northern outskirts of London as an outgrowth of this work. In 1941 it became known as the Operational Research Section, Fighter Command.[11]

In the summer of 1940 Manchester University physicist Patrick Blackett was appointed as a scientific advisor to the Army's Anti-Aircraft Command, where he created an organization of scientists to study field use of gun-laying radar equipment.[12] This group was absorbed into the Ministry of Supply in 1941 and became known as the Air

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[9] On the Air-Sea Warfare Development Unit, see Air Ministry, *The Origins and Development of Operational Research in the Royal Air Force* (London: HMSO, 1963), p. 75. Files on the Air Fighting Committee can be found in various archives of the Air Ministry, TNA: PRO AIR.

[10] The name of the facility changed twice before the name TRE was finally chosen. After MAP split from the Air Ministry in 1940, the TRE came under the authority of MAP.

[11] The origins of OR in the prewar radar development project has been rehearsed many times. Most recently, see Rau, "British Origins"; Kirby, *Operational Research in War and Peace: The British Experience from the 1930s to 1970* (London:: Imperial College Press, 2003); Zimmerman, *Britain's Shield.*

[12] Gun-laying meant that the radar signals could be fed into "fire control" mechanisms that automatically aimed the guns.

Defense Research and Development Establishment[13] Operational Research Group (ADRDE (ORG)). We will examine its activities in more depth presently. Blackett himself left the British Army's Anti-Aircraft Command in early 1941 to aid RAF Coastal Command in developing their aerial anti-submarine warfare tactics, where he developed another new team around the young biologist John Kendrew (who was serving there as an "Operations Research Officer"—an adjunct to Watson-Watt's SAT post) with the aim of helping integrate Anti-Surface Vessel (ASV) radar into the Command's work. This group became the Coastal Command OR Section. However, Blackett had a mandate to analyze *all* aspects of antisubmarine warfare, which shifted the definition of OR away from technological integration and toward a new role as an aid to command decision making, paving the way for the relationship between OR and planning discussed in the previous chapter. Even still, a large fraction of the British OR effort remained dedicated to analyzing the performance of technology in the field. The discussion in the previous chapter of the studies of practical effectiveness of ASV radar undertaken at Coastal Command is one clear example of this effort.

When Blackett moved from Coastal Command to set up a small OR team at the Admiralty at the end of 1941, it was initially expected that he would primarily be responsible for evaluating the effectiveness of weapons and recommending future avenues for development. Later in 1942, the University College London chemist Charles Goodeve, who had been working at *H.M.S. Vernon*,[14] was appointed to the new post of Assistant (later Deputy) Controller for Research and Development, which pushed

[13] The ADRDE, which was primarily concerned with anti-aircraft radar, was the War Office/Ministry of Supply equivalent of the Air Ministry/MAP Telecommunications Research Establishment.
[14] Pawle, *Secret War* has extensive information on Goodeve's prior work at the Admiralty's Department of Miscellaneous Weapon Development and *H.M.S. Vernon*.

Blackett's work in the direction of operations and implementation. In this position Goodeve became quite familiar with Blackett's OR work and, after the war, he became Britain's greatest booster for the establishment of OR in industry. Meanwhile, in early 1941, the War Office appointed the head of the National Physical Laboratory, Sir Charles Darwin, grandson of the famous naturalist, as its own Scientific Adviser to the Army Council. He, like Blackett, was also initially supposed to advise on questions of R&D strategy, but once established, his position, like Blackett's, became more concerned with implementation of technology in the field and the Army's burgeoning OR effort. The physicist Charles Ellis took over this position in 1943 until the end of the war. When Tizard retired as independent advisor to MAP in 1943, the Air Ministry appointed Imperial College physicist Sir George Thomson to be its official advisor and overseer of OR activity. MAP's research leadership was perturbed by the move, because they felt all scientific aid to the Air Ministry should originate from MAP. The Air Ministry, however, reserved the right to employ their own scientific personnel.[15]

Other lower level advisory positions also proliferated in this same period. The zoologist Solly Zuckerman and the crystallographer J. D. Bernal, who had both been members of a research team at the Ministry of Home Security surveying bombing damage in England, became tandem advisers to Lord Louis Mountbatten at Combined Operations.[16] When Mountbatten requested either Bernal or Zuckerman for an adviser when he moved to Southeast Asia Command, he had to settle for T. W. J. Taylor, a scientific liaison in Washington, who became the head of a small, mostly military OR

[15] Although the positions have been mentioned in various histories, there is no history of the establishment of scientific advisory positions within the British services. However, see chapter six for further details.
[16] Combined Operations referred to activity intermingling naval, army and air force components. See Solly Zuckerman, *From Apes to Warlords* (New York: Harper & Row, 1978); Andrew Brown, *J. D. Bernal: The Sage of Science*, (Oxford: Oxford University Press, 2005).

team in Asia.[17] Zuckerman later became the adviser to Lord Tedder, General Dwight Eisenhower's deputy in Europe, and took on unusually strong and direct responsibility for planning bombing strategies.[18] The commissioned physicist Basil Schonland, who had been in charge of the British Army's OR Group, became the scientific adviser to Field Marshal Bernard Montgomery of the Allied No. 21 Army Group throughout the invasion of Normandy, where he was in close contact with, but not in charge of the group's OR section.[19]

As we will see in more detail when we revisit this story in chapter six, OR was so closely associated with scientific advice, because it provided scientific advisors with a means of tailoring their advice on the use of technology that was most pertinent to the military's most immediate needs. Conversely, by forging strong intellectual relationships not only with technical personnel, but with high level military commanders as well, and by sitting in on planning meetings where tactics and practices were discussed, advisers were in an excellent position to detect areas where the rationale behind plans and procedures was uncertain or weak, and where some study of available data could profitably be made by OR scientists. One member of the Admiralty's OR staff later recalled, "No account of the work of our group would be complete without a picture of Prof. Blackett putting his head round the door with half-a-dozen ideas for research a day or of his constant inspiration and guidance..."[20]

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[17] Materials relating to finding an advisor for Mountbatten as Supreme Allied Commander in Southeast Asia can be found in TNA: PRO WO 203/5745; additional materials can be found in TNA: PRO WO 203/3129.
[18] Zuckerman, *From Apes to Warlords.*
[19] Brian Austin, *Schonland: Scientist and Soldier* (Philadelphia: Institute of Physics Publishing, 2001); see also B. F. J. Schonland, "On Being a Scientific Adviser to a Commander-in-Chief," 2/1/1951, *ELB*, Box 44, Folder 7.
[20] Letter from D. Evans to E. C. Williams, 11/1/1950, TNA: PRO ADM 219/629.

Of course, some advisers to the military were tempted to speak without the force of facts drawn from the military hierarchy. In one instance, Tizard chastised Zuckerman for a paper he wrote, observing, "You might be exposing yourself to the criticism which is now levelled with some justice against scientific men, namely that you are getting away from science and making recommendations on strategy and tactics." His objection was not that scientists had no business in such matters, but that they had to back up their claims about them with evidence. He asked Zuckerman if he might "put a little more science" into his suggestions.[21] For Tizard, good advice was based on an analysis of the best available knowledge. If scientists could not add to policy knowledge, they were not supposed to offer arbitrary advice. The sole responsibility for arbitrary action still rested with the military.

In the United States, the need to improve the implementation of radar technologies led to a different, more limited scientific advisory system under Edward Bowles, who was recruited to be an Expert Consultant to the Secretary of War, Henry Stimson, in April 1942. Bowles was an electrical engineer at MIT with no doctorate degree who was fond of pursuing practical-minded projects in conjunction with private companies.[22] He served as the secretary of the NDRC's early Microwave Committee, but, when the Radiation Laboratory (Rad Lab) was set up at MIT by NDRC Division 14 head Alfred Loomis, MIT president Karl Compton, and laboratory director Lee DuBridge, the laboratory's culture swiftly came to favor professional physicists. The engineer Bowles was left as the odd man out. Unexpectedly, though, Bowles was picked up by the War Department within days of leaving the radar effort, and Stimson gave him the authority to

[21] Letter from Henry Tizard to Solly Zuckerman, 3/30/1943, *HTT* 360.
[22] On Bowles' career at MIT, see Martin Collins, *Cold War Laboratory: RAND, the Air Forces, and the American State: 1945-1950* (Washington, DC: Smithsonian Institution Press, 2002), pp. 17-19.

examine all aspects of radar development, procurement, training, planning and operations in order to advise him as to what actions he should take to facilitate the implementation of radar technologies. Bowles later suspected his occasional nemesis Vannevar Bush had recommended him for the position to stymie Compton's aspirations to place DuBridge in a similar position.[23]

At these dizzying heights in the military power structure, Bowles was primarily responsible for addressing the organization of the military's technological efforts. His first task was to evaluate and improve the radar defenses of the Panama Canal Zone, which Watson-Watt had critiqued while visiting from Britain. Soon thereafter, Stimson set Bowles to work on the growing German U-boat menace off the east coast. Bowles' first major response to this problem was to push for the establishment of a Sea Search Attack and Development Unit (SADU) for the Army Air Forces at Langley Field, which would help devise the best tactics for use with new technologies. This unit was not substantially different from the Anti-Submarine Warfare Unit recently established in the Navy (with which the Anti-Submarine Warfare Operations Research Group worked closely), or the British development units described above. Bowles subsequently supported the establishment of a dedicated Air Anti-Submarine Force in the fall of 1942.

[23] Bush, recently departed from MIT, knew Bowles well. The two had a strained (but generally cordial) relationship. Bowles suspected that Bush thought Compton wanted to claim the whole radar infrastructure as an MIT project. See interview with Bowles, 7/14/1987, *NASM*, RAND Oral History Project, pp. 41, 45-50; Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, Mass.: Harvard University Press, 1995 [1977]), pp. 309-312; Henry E. Guerlac, *Radar in World War II*, Vol. 2 (Los Angeles: Tomash; New York: American Institute of Physics, 1987), pp. 699-704. Bowles' power was great, but should not be overstated. The Secretary of War had no immediate control over research programs.

In the summer of 1943, however, the Navy took over all anti-submarine operations with the establishment of its Tenth Fleet.[24]

By this time, Bowles had begun to define the purpose of his position around what he referred to as the "fusion of the technical and operational". In order to make any worthwhile recommendations about this fusion, it was necessary to find out what military operations looked like, which meant he actually had to travel to the field. He later recalled that

> the place to find problems was at the Front, not in Washington. Granted, I had the advantage of contact [...] with the top people like [Navy Fleet Admiral Ernest] King, [Army Chief of Staff General George] Marshall and [Commanding General of the Army Air Forces Henry] Arnold. But all you heard in Washington was the troubles, because the troubles were all reported: shortage of ammunition, shortage of this, failure of communication. You heard all those things, and for some reason, I had sense enough to go to the Front, starting with the Panama Expedition, and I visited relatively all our headquarters ultimately, including Burma, India, China, and that exposure couldn't help but give one ideas.

While Bowles had apparently harbored some initial concern that he would not find enough work to keep even himself busy, in the summer of 1942 he recruited Julius Stratton, an MIT physicist then working for the Rad Lab, to work for him. Stratton, in turn, selected H. H. Beverage, the vice president of R&D at RCA Communications, to assist him in a study of communications problems along North Atlantic ferry routes. Bowles brought in the geologist David Griggs not long afterward to work on problems associated with airborne radar. By the end of the war his office employed over 70 consultants, many of whom were borrowed or otherwise pilfered from DuBridge's Rad Lab staff.[25]

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[24] See Allen V. Hazeltine, "A Summary of Activities: Office of Dr. Edward L. Bowles, Expert Consultant to the Secretary of War and Special Consultant to the Commanding General, Army Air Forces, April 1942 through August 1945" 11/1/1945, pp. 6-18; *ELB*, Box 43, Folder 3.

[25] "Fusion" quote from memorandum from Edward Bowles to Henry Stimson, 8/24/1943, *ELB*, Box 32, Folder 3; extended quote from interview with Bowles, *NASM* RAND Oral History, p. 60; on Stratton and Beverage, see Hazeltine, "Summary of Activities," pp. 20-22.

The work Bowles undertook, especially on antisubmarine problems, brought him into contact with both the results of the British OR sections and the Navy's Anti-Submarine Warfare Operations Research Group, which had been set up at roughly the same time as his office, and was run by his MIT colleague Philip Morse. In late 1942 Bowles assigned Rad Lab luminary Louis Ridenour to study the application of radar devices to the problems of particular commands in Britain and Northern Africa, and to get in contact with British OR groups "to measure the extent to which their organization and technique in this specialized civilian-military activity appear applicable to our own problems."[26]

While Bowles never entirely identified his consultants' work with OR, he did see a strong relationship. Bowles wrote in a memorandum to Stimson that after the Army Air Forces had established their own anti-submarine force, the "need for intimate constructive working relations between our antisubmarine group and the related technical effort in the research, development, and manufacturing centers was clearly indicated" and that this need "led to the establishment of an operational research activity in the Army on antisubmarine warfare problems." It is, unfortunately, unclear to what activity Bowles was referring, but later in the memorandum he elaborated:

> The concept of calling on specialized skill as it has been applied thus locally on communications matters can be usefully expanded in other regions. The British term such experts, when they attack operational problems, "Operational Research" men. [...] There is need for a few highly imaginative minds to be sent out with our theater commanders to study special operational problems.

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[26] See Hazeltine, "Summary of Activities," pp. 22-27; letter from David Griggs to Edward Bowles, "Subject: Ridenour and Operational Research," 8/13/1942, *ELB*, Box 29, Folder 6; letter from Edward Bowles to Louis Ridenour, 11/4/1942, *ELB*, Box 29, Folder 7; Louis N. Ridenour, "Trip Report," 1/20/1943, *ELB*, Box 39, Folder 6. Ridenour concluded his trip report by emphasizing just how important field studies were. The last line, underlined, of the 32 page report was "The greatest attention must be paid to the use of radar in the field." Ridenour's trip coincided with Philip Morse and William Shockley's first trip to England, and he partially coordinated his itinerary with them. See "Diary of Dr. Philip M. Morse and Dr. William Shockley, Visit to London Commencing November 19, 1942," *ELB*, Box 39, Folder 5. See also memorandum from P. M. Morse to E. L. Bowles, 1/4/1943, *ELB*, Box 31, Folder 7.

Bowles so strongly associated this sort of work with the work of his office, that responding to the Navy's establishment of the Tenth Fleet, he noted that they followed "substantially the pattern of our technical recommendations [...] including the idea of civilian scientific assistance in answer to our criticism of the lack of effective technical planning."[27] Again, it is unclear to what he was referring, but if he meant ASWORG, he was surely giving himself too much credit for Navy activities.

As Bowles became increasingly involved in the Army Air Forces' radar problems in 1943, on top of his War Department post, he was appointed Communications Consultant to the Commanding General of the Army Air Forces, Henry Arnold. Arnold's historical reputation pegs him as a great believer in the transformative effects of science and technology on warfare. However, unlike Curtis LeMay, his actual knowledge of how science and technology contributed to warfare seems to have been sketchy at best. Arnold was a manager of men who relied on trusted individuals such as Bowles to use his influence to achieve the appropriate ends.[28] Bowles largely used this influence to augment his network of consultants. At the top level, he assigned Hartley Rowe, the vice president and chief engineer of the Boston-based United Fruit Company, to be an advisor to the Supreme Commander of the Allied Forces, General Dwight Eisenhower. Bowles also recruited eminent scientists and engineers to form what he called Advisory Specialists Groups who acted as temporary *ad hoc* advisory mechanisms to Army and Air Force commanders. The first group sent to Europe was comprised of Ridenour, Griggs,

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[27] All quotes from letter from Bowles to Stimson, 8/23/1943.

[28] See interview with Bowles, *NASM* RAND Oral History, pp. 58, 104-105; Hazeltine, "Summary of Activities," pp. 61-68; John W. Huston, "The Wartime Leadership of 'Hap' Arnold," in *Air Power and Warfare: Proceedings of the Eighth Military History Symposium*, edited by Alfred F. Hurley and Robert C. Erhart (United States Air Force Academy, 1979), pp. 168-185.

and the physicist H. P. (Bob) Robertson (who had been involved in the establishment of the Army Air Forces' operations analysis program in Europe), Victor Fraenckel of General Electric, D. K. Martin of Bell Telephone Laboratories and H. W. Hitchcock of Pacific Telephone and Telegraph. Lee DuBridge was also sent over as a part of the group on temporary leave from his position as director of the Rad Lab. Impressed by what he saw in Europe, upon his return to America, DuBridge expedited the already significant flow of Rad Lab personnel to the field through new means of field liaison that were then taking shape.[29]

Field Laboratories

It was a relatively straightforward matter for British research laboratories to extend their contact with military field operations, particularly those of the RAF, since command headquarters, testing facilities and research laboratories remained in close proximity to each other. American research and testing facilities, on the other hand, remained at home, while all except the highest military headquarters went abroad. Until late in 1943, the OSRD's primary means of addressing the operational problems of field equipment was through its London Mission, which was housed at the American Embassy. Otherwise, it largely left operational problems for regular military specialists to handle. As usual, though, radar technology proved to be the catalyst to more aggressive action.

The foundation of the British Branch of the Radiation Laboratory (BBRL) in late 1943 was in many ways a curious byproduct of the nature of the OSRD and the pressures of the war. NDRC divisions were not contractually permitted to produce equipment. Ordinarily, they would turn completed prototypes over to more traditional suppliers who

[29] See Hazeltine, "Summary of Activities," pp. 155-161.

adjusted the prototype's design to conform to military specifications and standards, and, of course, could also handle large scale production. However, the Rad Lab soon came under pressure to move a limited number of prototype devices immediately into field use. This process became known as the "crash" program, and it often saved months of turnaround time from development to the appearance of the first units from large scale production facilities. The first crash order for ground-based early warning radar sets arrived a short ten days after the surprise attack at Pearl Harbor and the orders did not abate for the remainder of the war.[30]

One consequence of crash development was that ordinary equipment testing and design procedures were circumvented in favor of rapid deployment, meaning that the Rad Lab was responsible both for obtaining data from the use of the unit in the field rather than in ordinary testing conditions, and for helping field personnel set the equipment up and develop intermediary technologies for its use. As Henry Guerlac, the official historian of the Rad Lab, later commented,

> The field service program grew out of the "crash" production principle and was its logical extension. Once the Laboratory was committed to building a small number of sets for immediate use, field assistance in training and maintenance of this new equipment and supplying both manuals and test equipment was a service the Laboratory was bound to provide. The next step beyond the crash program was an appreciation of the importance—to the Laboratory as well as to the using service—of arrangements to permit extended tests by Laboratory members of new equipment under operational or quasi-operational conditions. [31]

Initially, personnel were simply sent over to Britain through the OSRD's London Mission, but in 1943 the Rad Lab established a full-fledged British Branch at Malvern, close to the

[30] Guerlac, *Radar in World War II*, pp. 683-687.
[31] *Ibid.*, p. 697.

TRE. Following the liberation of Paris, an Advanced Service Base was set up there as well.[32]

During the campaign in Europe, the BBRL and its Advanced Service Base found itself sharing responsibilities with Bowles' first Advisory Specialist Group. In principle, as an extension of the Rad Lab, the BBRL made its services available to all of the British and American services, while the Advisory Specialist Group was an advisory mechanism responsible to only the United States Army Air Forces. In reality, though, the BBRL also did most of its work for the Air Forces, leading to some conflict. Neither organization was in an entirely ideal position. On the one hand, the BBRL did not have close liaison with the Air Forces. On the other, Bowles' Advisory Specialist Group was detached from research and development program back in the United States. For his part Bowles believed that leadership was best concentrated in the hands of his consultants, such as Griggs (who, according to Bowles, had "pep and ginger and operational instinct") and Ridenour.

As for the Rad Lab adjuncts, Bowles confided to Ridenour, "I fully expect the BBRL people to be unable to go beyond the bounds of technical comprehension of the operational problems."[33] If operations were combinations of personnel, tactics and equipment, it was important to have a grasp of all three aspects, and here he felt his consultants had the advantage.[34] Whether he was right or wrong, according to Guerlac,

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[32] The Advanced Service Base also served Harvard's radar countermeasure program's remote laboratory, ABL-15; information on the BBRL and the Advanced Service Base can be found in Guerlac, *Radar in World War II*, chapters 37, 39, and 40.

[33] Letter from Edward Bowles to Louis Ridenour, 9/27/1944, *ELB*, Box 30, Folder 8.

[34] The presence of the Advisory Specialist Group also had a potential impact on the local Army Air Forces operational research section. According to Warren Weaver, "It is [Bowles'] general idea that it is very unfortunate and unprofitable to ship out to a Command a basket full of scientists and announce to the Command, 'Here these experts are: They will study problems for you.' He never said so, but he more or less implied that this was a description of the way Operational Research Groups have been sent out by the

> It proved easier for the Director of the field laboratory to acquire the status of high-level scientific advisor than for members of the Advisory Specialist Group to speak for BBRL. Inevitably, therefore, the Director of BBRL became willy-nilly a top-level advisor, largely independent of ASG, though cooperating with it, but without the official credentials at [the headquarters of the United States Strategic Air Forces] enjoyed by the Air Forces advisors.

The tension was finally resolved in November 1944 by replacing the Advisory Specialist Group's senior radar member, Ridenour, with BBRL head John Trump, thereby creating a solid link between the organizations. Ridenour proceeded with Bowles to the Pacific to survey the situation there, before ultimately returning to the Rad Lab.[35]

With Germany's defenses crumbling, operations in the Pacific were a growing concern. As Erik Rau has described in some detail, when field scientific work first began to take shape in the Pacific in late 1943, it was primarily with the example of operations research in mind.[36] For the first year and a half of America's war effort, Vannevar Bush had been opposed to following the British OR model by attaching OSRD personnel directly to the military, but by late 1943 he had begun to realize that there were certain advantages to doing so. In particular, it allowed the OSRD to retain control over personnel who were being siphoned from OSRD laboratories into groups such as (ASW)ORG and the AAF's operations analysis sections. The OSRD's new Office of

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AAF." Weaver was concerned that the operations analysis groups might deteriorate, but Bowles felt they "would get along all right if they restricted themselves to their proper field of activity," which he took to be "post mortem analysis of operational data." Weaver felt this definition was unusually narrow. Diary of Warren Weaver, 4/20/1944, *NACP*, RG 227, Applied Mathematics Panel: General Records, 1942-1946 [NC-138/153], Box 4, "Operational Research" folder.

35 See Guerlac, *Radar in World War II*, pp. 870-873, quote on p. 872. On Ridenour's accompaniment of Bowles to the Pacific, see Hazeltine, "Summary of Activities," p. 172; and John G. Trump, "A War Diary, 1944-5," entry for 11/14/1944, *ELB*, Box 42, Folder 1.

36 See, for instance, letter from Karl Compton to Edward Bowles, 10/26/1943, wherein Compton asks Bowles to persuade the Navy to release Philip Morse to undertake initial survey work on behalf of the new OFS in the Pacific; and Bowles' relay of the Navy's negative reply, 11/11/1943, both *ELB*, Box 32, Folder 5. On the relationship between OFS and OR in general, see Erik Peter Rau, "Combat Scientists: The Emergence of Operations Research in the United States During World War II', PhD Dissertation, University of Pennsylvania, 1999, chapter 6; as well as Erik P. Rau, "The Adoption of Operations Research in the United States during World War II", in *Systems, Experts and Computers: The Systems Approach in Management and Engineering, World War II and After*, ed. Thomas P. Hughes and Agatha C. Hughes (Cambridge, Mass.: The MIT Press, 2000), 57-92, esp. pp. 77-79.

Field Service (OFS), to be headed by Karl Compton, was initially imagined as the OSRD's officially sanctioned entry into the OR effort. In fact, Morse's ORG, which had always been under contract from the NDRC, became the largest OFS project.[37]

As an OSRD adjunct, though, it was perhaps inevitable that the OFS came to be shaped more in the mold of the BBRL and the Advanced Service Base than the Navy's ORG and the AAF's operations analysis sections. The OFS' new Operational Research Section in Hawaii was even led by the physicist Lauriston C. Marshall who had been the first director of the BBRL.[38] Supporting more technical than tactical work, the ORS' personnel were divided into scientific consultants, operations analysts, physicists and administrative assistants. Of 47 ORS members listed in the section's final report, only five were operations analysts (the work of four was listed as "work simplification" that seems to indicate an association with time and motion studies; one was listed as "statistics"). Thirty-seven were scientific consultants, and while two of these were listed as "analysts", most were technological specialists in various kinds of equipment.[39] A similar, but less effective group was established in Australia, and was left behind when the island hopping campaign began. Toward the end of the war a full scale Pacific Branch of the OSRD was to be established under Compton (with Yale physicist Alan Waterman taking over OFS), but the war ended before the branch could begin its work.[40]

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[37] See Lincoln R. Thiesmeyer and John E. Burchard, *Combat Scientists* (Boston: Little, Brown and Company, 1947), esp. p. 47; see also Rau, "Combat Scientists" and Rau, "Adoption of Operations Research."
[38] See Thiesmeyer and Burchard, *Combat Scientists*, pp. 50-51, 288-293.
[39] "Final Report of Activities, Operational Research Section, Headquarters AFMIDPAC, 31 May 1944 to 2 September 1945," *NACP*, RG 227, Office of Field Service: Records of the Operations Research Group, Pacific Ocean Area, 1944-1945 [NC-138/182], Box 331.
[40] See Thiesmeyer and Burchard, *Combat Scientists*, pp. 300-317; Rau, "Combat Scientists"; and Rau, "Adoption of Operations Research."

"Comments on a General Theory of Air Warfare"

While some American laboratory scientists were flying off to the far corners of the globe, Warren Weaver, the head of the NDRC's Applied Mathematics Panel, stayed in the United States.[41] In the waning days of the war, he wrote a document entitled "Comments on a General Theory of Air Warfare" (hereafter "Comments") that was intended as a part of AMP's set of final reports, but it defied its purpose as a retrospective summary of the war work and lunged headlong into speculative issues of science, technology, mathematics and their relationship to military decision making. It was an extraordinary if rambling and occasionally bizarre contribution to the philosophy of applied science, to which historians have only recently attempted to ascribe meaning.[42] The document, effectively, argued that because *no* military decision that was made could be isolated from any other, *every* military decision had to be evaluated in terms of its impacts on other facets of military activity—an economic calculus dealing in a currency called "military worth".[43]

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[41] There is no full or satisfactory history of the Applied Mathematics Panel, but see Larry Owens, "Mathematicians at War: Warren Weaver and the Applied Mathematics Panel, 1942-1945," in *The History of Modern Mathematics, Vol. II: Institutions and Applications*, edited by David E. Rowe and John McCleary (Boston: Academic Press, 1989), 287-305; Mina Rees, "The Mathematical Sciences and World War II," *American Mathematical Monthly* **87** (1980): 607-621; W. Allen Wallis, "The Statistical Research Group, 1942-1945," *Journal of the American Statistical Association* **75** (1980): 320-330; J. Barkley Rosser, "Mathematics and Mathematicians in World War II," *Notices of the American Mathematical Society* **29** (1982): 509-515; and Saunders MacLane, "The Applied Mathematics Group at Columbia in World War II," *A Century of Mathematics in America*, edited by Peter Duren, Vol. 3 (Providence: American Mathematical Society, 1989), pp. 495-515. Also, see material in David Alan Grier, *When Computers Were Human* (Princeton: Princeton University Press, 2005).

[42] See Martin Collins, *Cold War Laboratory*, pp. 112, 116-119; and Paul Erickson, "The Politics of Game Theory: Mathematics and Cold War Culture," Ph.D. dissertation, University of Wisconsin-Madison, 2006, pp. 111-116.

[43] Warren Weaver, "Comments on a General Theory of Air Warfare," AMP Note No. 27, January 1946. Copies are available at TNA: PRO AIR 52/106, and *ELB*, Box 43, Folder 5. Weaver pointed out that his notion of military worth was akin to "economic utility," which he noted had been reconsidered in John von Neumann and Oskar Morgenstern, *On the Theory of Games and Economic Behavior* (Princeton: Princeton University Press, 1944).

Weaver posited a scenario that might be encountered by a hypothetical colonel charged with selecting a new radar-guided bombsight for development. The choice meant weighing differences in the weight of the bombsight, how complicated it was to use, and the time it would take to develop it, each of which had to be considered in light of various other factors. Weaver observed,

> He cannot possibly himself know all the necessary things, but somehow he should certainly bring to bear on these questions a wide and precise knowledge of the probabilities of bombing accuracies; the logistics of the theatres in which these sights are to be used; the nature of the enemy targets; all the vast field of terminal ballistics; the importance of the time factor (which means war plans, among other things); the psychology and physiology of operation of bombsights, selection, and training of bombardiers; accessibility of qualified personnel; the basic cost of accomplishing the same objectives otherwise; the present and potential future effectiveness of the enemy's fighter attack against our bombers; etc., etc.[44]

To tackle the problem most efficiently, Weaver envisioned a "Tactical-Strategic Computer (TSC)" that would take into account the effect of the various ways an enemy, or nature, might be expected to impact a mission, and then a "set of decision variables – one suggested, for example, by the Air Forces Board, or by Eglin, or by some general with an astronomical number of stars" would be entered detailing various ways an operation might be put together. (Note, once again, who had intellectual control over the planning process.) Some values could be set precisely; others with less certainty might incorporate a range of values. Once all of the values or ranges of values were set, the computer went to work crunching all of these different factors, and then, when the computer was finished, "the Military Worth dial lights up and displays the numerical value of M.W."[45]

The process was not intended to be a passive act of inputting data and receiving an output, though. Only once the computer took over "do the really interesting things

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[44] Weaver, "Comments," p. 5.
[45] *Ibid*, pp. 12-16, quote on p. 15.

begin to happen," Weaver wrote. For one thing, one was not simply supposed to walk away from the computer as it went about its business—in true analogue computer style, one watched the value of "M.W." fluctuate as the computer mechanically tried out all the various "combinations of those basic variables for which ranges, rather than specific values, were set in. If the M.W. varies widely during this period, one may possibly conclude that, before attempting to proceed, *further research is necessary* in order to delimit these basic variables to more narrow ranges." One could then experiment with the machine to see how sensitive military worth was to certain variables, and one could change a plan until a maximum military worth had been discovered.[46] John Williams, a mathematician employed by AMP (and a pathological smartass), later joked, "The best design I ever heard for such a machine was one that contained Warren's home telephone number!"[47] The joke contained a grain of truth.

Weaver's computer was not one of the fanciful, hubristic ideas that are often said to have danced through the heads of scientists and mathematicians in the heady days after the Allied victory. He did not actually believe that such a machine could be built. It was illustrative of a larger point, which was that analysis and planning, whether "excellent, good, mediocre, or disastrous," were not things one chose to engage in—they were facts of life. Protests that the problems the military faced were too complex for analysis and had to be handled instead through common sense were not acceptable. An overall comparison, he insisted, "*has* to be made either by analysis, or magic, or blind guess." To Weaver, it was as though an imperfect version of his Tactical-Strategic Computer

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[46] *Ibid*, p. 15, emphasis added.

[47] Vaughn D. Bornet, "John Williams: A Personal Reminiscence (August, 1962)," 8/12/1969, RAND Document D-19036, p. 29; a copy of the document, which is an interview with Williams, is available in the John Williams papers, Box 1, RAND Corporate Archives, Santa Monica, California.

were in operation every time a decision was made. Judgments based on common sense were not made using some alternative method—they were nothing more than "disorganized and feebly intuitive shadows of a real analysis." He wrote, "I am simply arguing for facing the complexity and the facts, and pushing analysis to its usable limit."[48]

How was one to push analysis to the limit given the impossibility of a fully integrated analysis such as his imaginary computer could accomplish? Performing partial analyses was the only answer. Small decisions, "micro-problems" as Weaver called them, could always be handled theoretically. For instance, one could calculate the theoretical accuracy of a bombsight simply on the basis of its physical attributes. But how much accuracy should one buy at what price? If one wanted to know, in practice, what the bombsight's value would be, one had to discuss "macro-problems" incorporating into one's thinking the range of accuracies a bombsight could have, how accurate one needed to be to hit a target, how hard it was to train personnel in the bombsight's use for each level of accuracy, and so forth. Comparing micro-problems to bricks in a wall, Weaver noted that one had to understand *how* micro-problems related to each other before the consequences of their interrelationship could be understood.[49]

The best means of incorporating micro-problems into a macro-problem was by gathering many different perspectives of a macro-problem by means of liaison.[50] True to Williams' joke, Weaver, both in his position as the Chief of AMP and in his peacetime position as the Director of Natural Sciences at the Rockefeller Foundation, knew the

[48] Weaver, "Comments," pp. 17-18, Weaver's emphasis.
[49] *Ibid.*, pp. 33-34.
[50] For discussion of the relationship between bureaucratic organization and computing, see Jon Agar, *The Government Machine: A Revolutionary History of the Computer* (Cambridge, Mass.: The MIT Press, 2003); see pp. 248-252 for a discussion of OR and scientific advice, although not exactly in this context.

value of having good connections. Once one had gathered all sorts of perspectives, one could go about cobbling them together as best one could, and the most reliable mortar Weaver knew was mathematics. The more quantitatively one knew how one's decisions would impact other factors, the better decisions one could make. Some quantitative relationships could be derived from tests, or, better still, from actual operational research studies. He pointed to a memorandum issued by Patrick Blackett on the methodological value of comparing operational statistics by holding all factors except one constant (what Blackett called the "variational method").[51] Some factors, necessarily, had to be incorporated more arbitrarily, but it stood to reason that in most cases, the less arbitrarily—the more rationally—one made a decision, the better that decision was likely to be.

As the administrative head of AMP, Weaver took every opportunity to try and institutionalize his philosophy. In February 1945 Edward Bowles had asked Weaver about the requirements for setting up a new gunnery research and bombing assessment unit at Eglin Field.[52] This request gave Weaver the opportunity to formalize ideas he had been having during a recent study AMP had conducted on the B-29 bomber (to be discussed presently), and Weaver included his recommendations to Bowles as an appendix in his "Comments". According to Weaver, gunnery assessment was no straightforward task. "The moment ... one attempts to analyze this individual situation he becomes inescapably involved in the broader problem of the way in which the AAF as a whole provides for the development of new devices and techniques of aerial warfare,"

[51] *Ibid.*, p. 27. Blackett's 1943 memorandum, "A Note on Certain Aspects of the Methodology of Operational Research" was reprinted in P. M. S. Blackett, *Studies of War, Nuclear and Conventional* (New York: Hill and Wang, 1962), pp. 176-198.
[52] See Hazeltine, "Summary of Activities," pp. 108-111; and Weaver, "Comments," Appendix B (2/28/1945), p. 72.

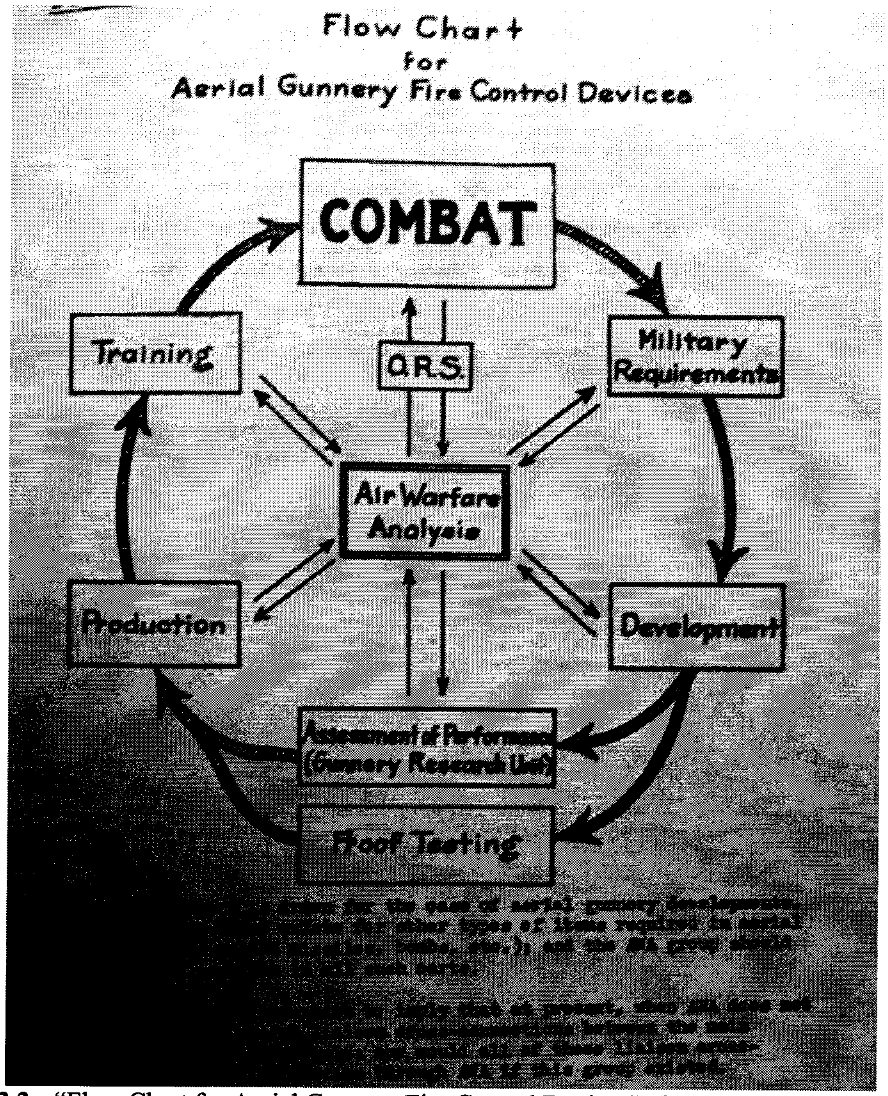

Figure 2.2. "Flow Chart for Aerial Gunnery Fire Control Devices"; the words beneath the chart are key: "There is no intention to imply that at present, when AWA does not exist, there are no useful liaison cross-connections between the main functions in the outside circle: nor would all of these liaison cross-connections necessarily function through AWA if this group existed." To Weaver, liaison and mathematical analysis were complementary concepts. Source: Warren Weaver, "Comments on a General Theory of Air Warfare," AMP Note No. 27, January 1946, Appendix B, *ELB*, Box 43, Folder 5.

he wrote. He laid the problem out in a chart (Figure 2.2), where something called Air Warfare Analysis (AWA), much like his fanciful computer, coordinated the formulation of requirements, technological development, equipment production, training and combat use in a way that was consistent with the military's goals.[53] Operations Research

[53] Weaver, "Comments," Appendix B, pp. 72-81, quote on p. 72. In April Weaver telephoned Bowles to let him know that the gunnery research unit was going forward with enthusiastic support, and that he had high

(represented on the chart in the OR Section, "O.R.S.") was a concept that was quite distinct from AWA, having mostly to do with the interpretation of combat experience for the Air Warfare Analysts to incorporate into their studies. However, unlike the Tactical-Strategic Computer, air warfare analysis was not an ideal—it was something that was used extensively during the war.

The Origins of Air Warfare Analysis

Air Warfare Analysis predates OR. To seek its immediate origins we must move backward seven and a half years from Weaver's "Comments", cross the Atlantic to Great Britain, and venture into the depths of the military's civil service organization to the Air Ministry's No. 1 Air Armament School where one could find a lecturer in ballistics named Dr. L. B. C. Cunningham (Education Officer, Grade III).[54] In 1937 he set about constructing a "quantitative method" interrelating approximations of the various probability factors affecting the outcome of an air duel, including the various aspects of their armaments such as caliber, precision, rate of fire and reliability; the vulnerable surface area of each airplane; and their employment of "stated tactical methods either of approach or withdrawal." This theory differed from "older theories of gunnery" (which were primarily geared toward weapons *testing* and not actual combat) "in that it [took]

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hopes that Air Warfare Analysis could get started as well, though it would "obviously take time to get top AAF officers used to the idea". Bowles assigned consultant William Shockley (whom we will discuss presently) to work on it. Source: Diary of Warren Weaver, 4/10/1945, *NACP*, RG 227, Applied Mathematics Panel: General Records, 1942-1946 [NC-138/153], Box 1, "W. W. Diaries – General" folder.

[54] We know unfortunately little about Dr. Cunningham, including the names behind his initials. Cunningham has rarely ever been mentioned (see Air Ministry, *The Origins and Development of Operational Research in the Air Force* (London: HMSO, 1963), p. 39, for a rare acknowledgement), and, to the best of my knowledge, this is the first ever historical analysis of his work. His civil service rank is taken from the *Imperial Calendar and Civil Service List*. His single publication is a discussion of some of his wartime work: L. B. C. Cunningham and W. R. B. Hynd, "Random Processes in Problems of Air Warfare," *Supplement to the Journal of the Royal Statistical Society* **8** (1946): 62-97.

account of the simultaneous exchange of fire between two opposing forces instead of considering the performance of only one side at a time, firing against a passive target." Thus, the vulnerabilities as well as the strengths of combatants could be mathematically considered in tandem. By his method, Cunningham hoped to calculate *a priori* the chances both sides had of coming out of the encounter victorious "if they adhere to their tactical programmes."[55] The ability of a design choice to contribute to overall success was, after all, the only real measure of correctness of choice. Recognizing that his formulation was entirely exploratory, in November 1937 he sent a short paper detailing his theory via the Air Ministry's Director of Armament Development to the Royal Aircraft Establishment (RAE) at Farnborough to gather more expert opinions.

At the RAE, it came into the hands of Ben Lockspeiser, a senior researcher, who, once the paper's mathematics were vetted and declared "unimpeachable", thought it might be valuable for laying out an analytical approach to settle certain technical and tactical questions.[56] For instance, he pointed out, it could help determine if it was better to use explosive or ordinary bullets. The paper suggested that the reduced rate of fire of explosive bullets would be compensated for by the increase in the effective area of the target. It also might be able to solve whether or not it was advantageous to open fire as

[55] Director of Armament Development to Chief Superintendent, Royal Aircraft Establishment, 11/9/1937, TNA: PRO AIR 13/879. The original paper is unavailable, but see a subsequent, longer draft of the paper: L.B.C. Cunningham, "The Theory of Machine Gun Combat: An Amplified Introduction" dated 2/3/1938, TNA: PRO AIR 13/879. Cunningham's later comments on his early theories can be found in L.B.C. Cunningham, "The Mathematical Theory of Combat Applied to Tanks," AWA Report No. 24, May 1941, TNA: PRO AIR 20/12944.

[56] Lockspeiser would go on to have a distinguished career in the civil service, becoming the Air Ministry's Director of Scientific Research, the Chief Scientist at the postwar Ministry of Supply, and the Secretary of the Department of Scientific and Industrial Research. He was elected a Fellow of the Royal Society in 1949, a rare honor for career civil servants. He would gain some academic notability as the first president of the CERN Council. See A.P.J. Edwards, "Ben Lockspeiser," *Biographical Memoirs of Fellows of the Royal Society* **39** (1994): 246-261.

soon a target came into range, or to conserve one's fire for shorter ranges.[57] "In terms of actual fighting conditions," Lockspeiser felt that the method was "immediately applicable" to weighing the factors involved in the first problem, but he deferred judgment on the complicated mathematical expression demanded by the second, and was "not prepared to say whether it can be related to practical considerations or not."[58]

Emboldened by the fact that his "fundamental method of attack" had been found valid, Cunningham set out to expand on his work, and in early 1938 he produced a paper entitled "The Theory of Machine Gun Combat: An Amplified Introduction". Building on his previous shorter paper, he improved on his statistical tools and then worked out some "simple illustrative examples" of aircraft duels involving various scenarios concerning the strengths, vulnerabilities and tactics of combatants, all the while stressing that the paper made "no serious attempt to achieve far-reaching conclusions." These were intricate calculations, but, he argued, "any labour devoted to it which gives an incontrovertible result will be more than repaid by the fact that this result will be quantitative, and may be used to settle finally the optimum choice between alternative proposals regarding equipment, or its tactical employment." He added prophetically, "This section [of examples of application] is capable of unlimited development." If his paper was so fortunate as to "survive" the RAE's "serious fundamental criticisms again, then," he felt, "the impetus necessary to ensure [the method's] rapid development will be provided."[59]

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[57] The famous Battle of Bunker Hill "Don't fire until you see the whites of their eyes" command was a similar—if rougher— approximation given available ammunition, the expected accuracy and rapidity of musket fire, and expectations of the damage the British soldiers would do by opening fire earlier. The problem would fuel much of the early research in game theory.

[58] Memorandum from B. Lockspeiser to Dr. Roxbee Cox, 11/19/1937, TNA: PRO AIR 13/879.

[59] L.B.C. Cunningham, "The Theory of Machine Gun Combat: An Amplified Introduction," 2/3/1938, TNA PRO: AIR 13/879, pp. 1, 13.

This paper, along with a few other papers he wrote on "statistical gunnery and bombing problems" were forwarded to the RAE in May 1938.[60] One on anti-aircraft fire was received by B. G. Dickins who would three years later become the head of the OR Section at RAF Bomber Command, but was then working for the Air Ministry's Director of Scientific Research (DSR). Dickins forwarded it along to Lockspeiser noting it "may be of interest, although it is thought that the results obtained in the paper would not be directly applicable to the problem" because it assumed spherical error functions of anti-aircraft fire and spherical fragment distribution, and neither was, in fact, true.[61] Although Cunningham's theoretical treatments of gunnery were hit-and-miss, he had successfully found his way into the consciousness of the Ministry's highest researchers, and in June he was named an adviser to the RAE on statistical problems by the DSR's office. This office did not consider it likely that Cunningham's approach could offer "complete solutions" to certain problems in air gunnery development because "it will not usually be possible to specify all the basic data," but "some useful guiding principles may nevertheless emerge." This point was important. The RAE, of course, had its own methods of selecting methods of armament based on statistical data, but these were also flawed, and Cunningham's high theory offered mathematical insights on how to find newer, more effective approaches.[62]

The relationship was mutually beneficial. After visiting the RAE for the first time, Cunningham decided to modify his anti-aircraft paper on the basis of his conversations

[60] R. S. Capon, Deputy Director of Research and Development (Armament) to Superintendent of Scientific Research, Royal Aircraft Establishment, 5/2/1938, TNA: PRO AIR 13/879.
[61] B. G. Dickins for Director of Scientific Research to Chief Superintendent, Royal Aircraft Establishment, attn: Mr. Lockspeiser, 6/10/1938, TNA: PRO AIR 13/879.
[62] D. L .Webb to Air Officer Commanding No. 25 (Armament) Group, 6/13/1938; and attached "The Statement of Statistical Problems," TNA: PRO AIR 13/879. The notion of emergence of guiding principles from theoretical formulations will remain with us for the rest of this dissertation.

with staff there, and he also wanted to obtain copies of several reports he had seen from the RAE's Physics and Instrument Department "dealing with the mathematical theory of scatter bombing, fragmentation of shells and analysis of errors."[63] Very quickly Cunningham and Lockspeiser began trading notes regularly. By the winter of 1938, despite some delays in Cunningham's work, their theories were coming along nicely, and he had become "convinced that the method as a whole is capable of giving important results—not merely *ad hoc* solutions of particular problems, but broad generalizations which might short-circuit numerical work to a large extent." He felt, "There is a full year's work waiting to be tackled once this elementary part has been placed on a satisfactory footing."[64] G. W. H. Stevens, a colleague of Lockspeiser's, told Cunningham not to rush himself as "the theoretical contribution we have so far will help us to sort out some of the fallacies that are existing." Furthermore, "Frankly, the data available so far is not worthy of application to refined analysis."[65]

By the summer of 1939 Cunningham had worked out the fundamentals of his "Mathematical Theory of Combat", and had also produced another paper entitled, "Report on the Influence of Numerical Superiority in Air Combat as Distinct from the Total Fire Power on Either Side" detailing combats between forces of M "fire-sources" on one side versus N on the other.[66] He later observed that this consideration "led to

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[63] BL for Chief Superintendent, RAE, to Under Secretary of State, attn: DSR, 6/22/1938; and W. J. Richards, for Chief Superintendent, RAE, to Under Secretary of State, attn: R.D.Arm.2b, 6/27/1938; both TNA: PRO AIR 13/879.

[64] Cunningham to Lockspeiser, 11/27/1938, TNA: PRO AIR 13/879. Cunningham was held up by an "intrinsic difficulty" in the work resulting from a lack of a textbook on the "use of Divided Differences as an analytical process," and the death of his mother and his duties as executor of her will and trustee of his late father's estate.

[65] G.W.H. Stevens to Cunningham, 12/6/1938, TNA: PRO AIR 13/879. Stevens would later become one of the founding members of the RAF Bomber Command Operational Research Section under Dickins.

[66] L.B.C. Cunningham, "The Mathematical Theory of Combat" AWAS Note No. 1, TNA: PRO AIR 20/12820; L.B.C. Cunningham, "Report on the Influence of Numerical Superiority in Air Combat as

some quite unexpected conclusions" which he developed into a "Theory of Multiple Combat" that later turned out to be widely applicable to problems such as tank combat.[67] When war broke out in September 1939, Cunningham found it actually freed him from his teaching duties at the Armament School, giving him more time to devote to practical applications of his theory until he received "pending news of [his] war-time appointment."[68] Finally, in February 1940, he was named the head of a new organization called the Air Warfare Analysis Section (AWAS), which Sir Henry Tizard, then the Scientific Adviser to the Chief of Air Staff, had recommended be set up to "investigate the best methods of collecting and analysing data on aerial bombing and anti-aircraft gunfire under war conditions."[69] Cunningham's "Mathematical Theory" became AWAS Note No. 1, and "An analysis of the performance of a fixed-gun fighter, armed with guns of different calibres, in single home-defence combat with a twin-engined bomber," which Cunningham wrote with members of his staff as an extension of his theories, became AWAS Report No. 1. It was a pertinent subject, given the likelihood that Britain would soon come under full-scale aerial attack.

Distinct from the Total Fire Power on Either Side," TNA: PRO AIR 20/371. In the summer of 1939 Lockspeiser moved to the DSR's office, and Cunningham began working with Stevens uniquely. Stevens felt the theory should be passed along to the researchers at the Admiralty and the Royal Arsenal at Woolwich who were responsible for statistical problems; Stevens to Cunningham, 8/18/1939, TNA: PRO AIR 13/879.

[67] See L.B.C. Cunningham, "Theorems of Multiple Combat," draft, TNA: PRO AIR 13/879. For discussion see also Cunningham, "Tanks," AIR 20/12944. I have not worked out whether this is a replication of Lanchester's theory.

[68] These practical applications could be quite involved, as we might infer from Stevens' advice on a particular problem: "My one objection in the list is your example of triplicated sub-targets. A hit on one hinge of an aileron is more likely to jam or restrict the movement of the aileron than to completely sever the hinge. Therefore I would put all control hinges and other moving control parts which come into contact with stationary parts into the (S-V) target area in view of the experience from trials."

[69] Air Ministry, *Origins and Development*, p. 39. After the split of the Ministry of Aircraft Production from the Air Ministry a few months later, AWAS became a part of the former organization. By contrast, OR groups would be run entirely by the Air Ministry.

Cunningham and his staff at AWAS adapted combat theory throughout the course of the war with the object of joining statistical results with a theoretically rigorous explanation of their origins. Once theoretical extensions proved demonstrably successful in correlating test and field data, the theory could then be used to anticipate the value of future modifications in equipment and tactics, which, once implemented, could then be checked once again with statistical data to make sure that the extrapolation had been valid. There was, of course, no such thing as a complete theory; only a theory that could be applied, with discretion, to pressing problems. For instance, AWAS Paper No. 48, "Notes on the Theory of Aerial Gunnery", was an attempt to develop knowledge of bomber-fighter duels. The current theory was known to be inadequate in cases dealing with accurate gunnery on account of rough approximations, and so could not be used to estimate the performance of gun sights, for example. Cunningham wanted "to place the errors due to these and other approximations between limits in order that rational choice may become possible of the simplest formula or formulae tolerable in a given calculation; instead of letting errors run wild." As with his initial steps in developing combat theory, much of the requisite statistical knowledge about vulnerability and accuracy could still be obtained from specially arranged tests, but some of the most useful data came from the field.[70] In the case of this paper, the ORS at Fighter Command kept data on the accuracy of British pilots against enemy bombers. After their establishment in 1941, the RAF's OR groups provided continual investigative support for AWAS theory development.[71] The theories themselves, though, were not considered to be an example of OR.

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[70] L. B. C. Cunningham, "Note on the Theory of Aerial Gunnery," AWAS Report No. 48, May 1943, TNA: PRO AIR 20/12712.
[71] The relationship between AWAS and the OR Sections was articulated at the first meeting of the Air Ministry's Operational Research Committee on October 31st, 1941, where it was decided that AWAS

Theories and Machines: The Case of Fire Control

To understand properly the role groups like AWAS, AMP, military research and testing facilities, and the various OR groups were playing with respect to each other, it is necessary to understand some of the history of the more technical aspects of research, development, design and implementation, and particularly the development of intermediary technologies. We will discuss these processes in terms of one of the most challenging areas of equipment development: fire control. Hunters of game have only their rifles and perhaps a telescoping sight to track and shoot their quarry. In the days of Zeppelins and biplanes, anti-aircraft gunnery employed much the same set of techniques (with a few modifications such as the employment of range tables and timed fuses on bursting shells), but, even during World War I, and especially as time passed and aircraft technology progressed, the goal of shooting down higher, faster and more maneuverable aircraft moved beyond the skill of even the most talented gunner to manage in real time. Automated fire control became increasingly vital if anti-aircraft gunnery was to prove any better than fist shaking.

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should act (at the suggestion of the physicist Patrick Blackett, then the scientific adviser to RAF Coastal Command) "as a pool of specialist mathematicians for high grade mathematical analysis, which was outside the scope of the staff normally available at O.R.S's," Operational Research Committee, Meeting No. 1, minutes, pp. 3-4, TNA: PRO AIR 2/5352. AWAS also became ensconced in a number of problems faced by other organizations. In one early report, Cunningham chastised bombing mission planners for allocating effort in bombing raids based on statistical fallacies, pointing out that their methods were "wholly irrational" and that "the correct, logical principle has been known to actuaries for over two hundred years and needs no more than a little adaptation to become applicable to bombing," L. B. C. Cunningham, "Note on the Correlation of Bombing Probability Calculations with Plans for Bombing Operations," AWA Paper No. 3, n.d., TNA: PRO AIR 20/371. The section was also involved in data analysis for a census on bombing damage in Britain conducted by the Ministry of Home Security, attempting to determine enemy techniques, tactics and accuracy. Until the establishment of an OR group in the British Army's Anti-Aircraft Command, AWAS was saddled with producing compilations of data from anti-aircraft "ZZ" forms and their predecessors. See, for instance, "Analysis of Anti-Aircraft Gunfire from data contained in the A.A. Intelligence Proforma," AWAS Report No. 16, 12/12/1940, TNA: PRO AIR 20/12936.

Automation did not come all at once, though. Instead, intermediary technologies began populating the borderland between technology and technique. Training exercises, designed to instill the principles of good gunnery into a gunner's instincts, obviously landed on the side of technique. Ballistics tables—often presented in a graphical format, and long used in artillery firing—were more akin to a technology, but augmented technique by permitting quick calculations of fuse settings and gun angles to be made based on range and height estimates; but because atmospheric conditions at anti-aircraft heights were less well known, and because anti-aircraft shells did not leave craters, these tables were not so well developed as their land artillery counterparts. When World War I broke out, the ability of the gunner to use a gun effectively was still hampered by the gap between technique, technology and requirement. It was difficult to hit even massive, slow moving Zeppelins.[72]

The World War I work of the talented young physiologist Captain (later Major) A. V. Hill and his Anti-Aircraft Experimental Section (AAES),[73] is sometimes described as a nascent form of OR, but it can be understood better as a group aiding in the development of intermediary technologies and weapons testing procedures. At the beginning of 1916, Hill was recruited to the Ministry of Munitions by Horace Darwin, the youngest son of the naturalist Charles Darwin, uncle of the aforementioned physicist Charles Darwin, and the co-proprietor of the Cambridge Scientific Instrument Company. Darwin wanted Hill to help implement a training device he had invented that could be used, given the gun's angle and the shell's fuse setting, to calculate the position where a

[72] On the history of anti-aircraft gunnery, see Ian V. Hogg, *Anti-Aircraft: A History of Air Defence* (London: Macdonald and Jane's, 1978), which includes information on Hill and the Mirror Position Finder, see pp. 45-46, 105.

[73] The AAES was a sub-division of the Munitions Inventions Department of the Ministry of Munitions.

dummy anti-aircraft shell would have exploded relative to an actual targeted aircraft if a shell had been fired at that instant. This device was meant to improve gunners' targeting instincts better than actual battle experience would allow. If employed in real time "by means of various graphical methods (including the use of what we now term graphic range tables)," Hill recalled in 1918, it was hoped that the device could provide continuous correction to gunners' firing by giving them "information as to how good or how bad their shooting was and in what direction their errors had occurred." However, while a modification of this device called the "mirror position finder" (Figure 2.3) was successfully developed, a plotting mechanism that was rapid enough to be used for training was not achieved during the war.[74] It was out the work on this device, though, that the AAES came into being with Hill as its chief. The Mirror Position Finder, along with other height and range finders proved apt as a means of recording the tracks of aircraft and the positions of live shell bursts. Actual testing results could be compared systematically with some precision against predicted results. Different guns, shells and fuses could be tested against each other; guns could be individually calibrated against variations between shell lots or in the gun itself; and, crucially, newer and better ballistics tables could be computed, taking into account corrections for wind and variations in atmospheric density with altitude. Anti-aircraft gunnery could now aspire to the intellectual sophistication that land and naval artillery had already achieved in their own low-altitude domains.[75]

[74] A. V. Hill, "The Anti Aircraft Experimental Section of the Munitions Inventions Dep't (1916-1918)," draft, 1918, quotes from p. 4, see also p. 17; *AVHL* I 1/37. Darwin's initial device employed a camera obscura, but it was found too difficult to track a target using it, so Hill replaced it with mirrors; see also Horace Darwin, "Aiming Practice with A.A. Guns," 12/26/1915, *AVHL* I 1/9.

[75] Hill, "Anti Aircraft Experimental Section"; and War Office, *Text Book of Anti-Aircraft Gunnery*, 2 vols. (London: HMSO, 1924/1925), copy available at *AVHL* I 1/33. On naval fire control see John Brooks, *Dreadnought Gunnery and the Battle of Jutland: The Question of Fire Control* (New York: Routledge,

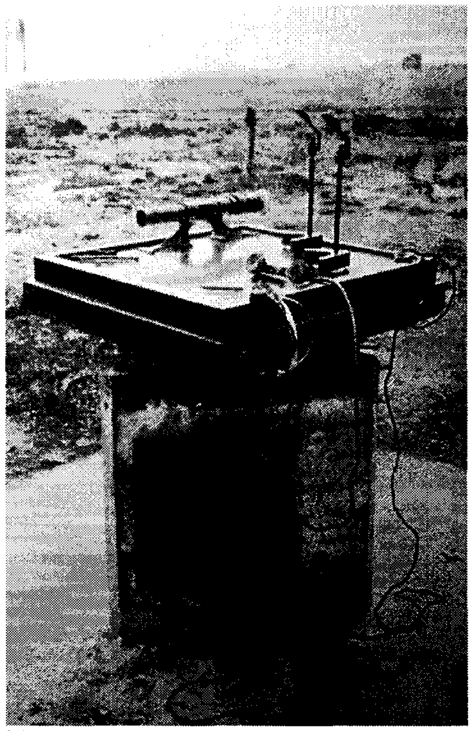

Figure 2.3. The mirror position finder, which was used to track the location of anti-aircraft shell bursts. Source: War Office, *Text Book of Anti-Aircraft Gunnery*, Vol. 1 (London: HMSO, 1925); copy available in *AVHL* I 1/33.

The new principles were collected into a two volume *Text Book of Anti-Aircraft Gunnery*, compiled largely by members of the AAES, and published internally in 1925. The vast bulk of the text was taken up by mathematical means of estimating where a fired shell would explode, where an airplane was when sighted, and where it would be at a future point in time. It discussed experimental means of testing the attributes of equipment, but did not devote space to battle testing, which proved impossible in conditions where one could not tell which shell burst came from which gun.[76] Obviously, operational experience was useful in determining what kinds of instruments would prove useful, and what factors needed to be tested, studied and incorporated into gunnery theories, but this sort of intellectual liaison was standard practice in the research and

2005); and David Mindell, *Between Human and Machine: Feedback, Control, and Computing Before Cybernetics* (Baltimore: Johns Hopkins University Press, 2002).
[76] War Office, *Anti-Aircraft Gunnery*.

development of weapons and other equipment. It was more likely the comprehensiveness of the body of knowledge surrounding antiaircraft gunnery and the rapidity with which it was developed that later made the work of the AAES seem akin to work in OR.

Quantitative knowledge of how an operation unfolded allowed one to develop better intermediary technologies. I have already mentioned the improvements of ballistics tables. A related intermediary technology, the slide rule, could also take various inputs and produce quick correction factors. For instance, the "parallax correction instrument," (diagrammed in Figure 2.4), was used to account for the offset of a

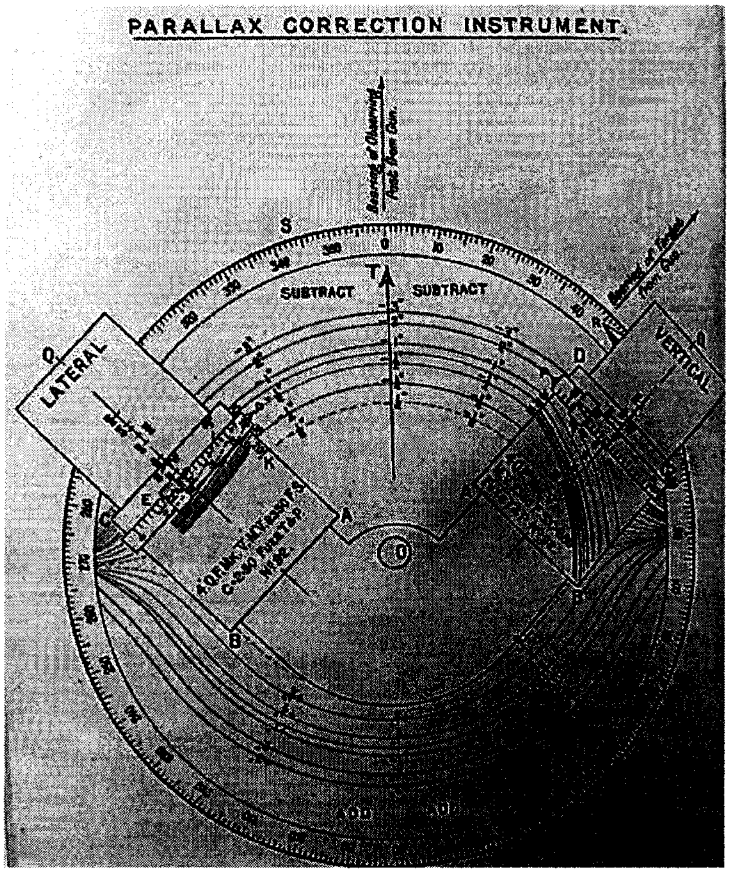

Figure 2.4. Parallax correction instrument diagram, used to calculate aiming corrections based on the separation of the observing post from the gun. The lines on the device were color-coded, which is not visible in this reproduction. Source: War Office, *Text Book of Anti-Aircraft Gunnery*, Vol. 2 (London: HMSO, 1924); copy available in *AVHL* I 1/34.

measurement made by a central observing post remote from a gun—and, as we can see, it was not a matter of simple trigonometry.[77] Similar corrections could be built into sights for guns and bombs which could use physical inputs to offset a target indicator automatically in order to show where a bullet, shell or bomb fired or released at that instant could be expected to hit. Actual predictors, which could aim a gun automatically based on a manually inputted track, were never made practical during World War I—although they remained an enticing vision for independent inventors. However, the fire control technology made by companies such as Sperry Gyroscope that was originally used on naval vessels soon made its way into anti-aircraft gunnery, and, between the wars, predictors joined calculating gun sights and replaced ballistics tables and slide rules at the vanguard of fire control technology.[78]

When the Battle of Britain began in 1940, however, fire control predictors were not proving up to the task, and Lt.-Gen. Frederick Pile, in charge of the British Army's Anti-Aircraft Command, became frustrated that the technique built into predictors did not correspond to the techniques necessary to combat maneuverable German bombers that stubbornly refused to maintain a steady course.[79] In August 1940 a meeting was called (and chaired by A. V. Hill) with the aim of appointing a scientific advisor who could provide research and development personnel with good, technical expressions of the military's operational needs.[80] It should not be assumed that there was no liaison relationship between predictor designers and military commanders, but it was apparent

[77] *Ibid*, Vol. 2, pp. 252-255.
[78] See Mindell, *Between Human and Machine.*
[79] "Notes on conference to be held on Friday, 9 August at 2.30 p.m. at Savoy Hill House, to discuss Anti-Aircraft Gunnery," *AVHL* I 2/3 Pt. I. "By General Pile[:...] All the factors on which the scientific instruments in connection with A.A. fire were based are proving illusory. The German rarely, if ever, flies a steady course, rarely, if ever, continues at the same height and generally employs dive bombing tactics."
[80] "Notes on Conference to Discuss A.A. Gunnery," 8/14/1940, *AVHL* I 2/3 Pt. I. Other accounts of the meeting can be found in various histories of the origins of OR.

that this system had thus far failed to produce equipment meeting anticipated requirements, and that it required immediate *ad hoc* augmentation. As we saw earlier, Pile received Patrick Blackett, who was at that time employed by the Royal Aircraft Establishment to design calculating bombsights, work that included doing such things as modifying sights using a Sperry-manufactured artificial horizon to keep them stabilized even as a plane ascended and rolled, and then testing the new contraptions to see how they would perform versus traditional sights under various conditions.[81] He was a logical choice to work with Anti-Aircraft Command's dysfunctional Sperry and Vickers predictors.

When Blackett arrived at AA Command he assembled a team that became known as Anti-Aircraft Command Research Group, or, more informally, as Blackett's Circus.[82] The first several months of their effort were spent primarily on making certain that guns were being calibrated properly and adjusted to deal with the terrain and obstructions specific to their location. The greatest problems, though, were in the mechanics of the predictors themselves. Radar operators tracking the course of enemy bombers were prone to wandering back and forth over the true bearing.[83] The predictors, which measured the tracks over only a few seconds, took the operators' inputs as the true track of the bomber, resulting in guns aiming "in almost every possible direction except that in

[81] "Note on Conference." The meeting also concluded that "cine-theodolites" should be obtained in order to assess the results of gunfire during enemy raids, and that the development of a proximity fuse would offer the greatest hope of increasing effectiveness. On Blackett's work at the RAE, see letter from Patrick Blackett to Henry Tizard, 9/16/1939, *HTT* 32.

[82] Although the origins of the name "Blackett's Circus" remain unknown, I would argue it had the mundane Latin connotation of "Blackett's Circle" rather than a more whimsical interpretation.

[83] The best account of this work is L. E. Bayliss, "The Origins of Operational Research in the Army," Army Operational Research Group Memorandum No. 615, 10/11/1945, TNA: PRO WO 291/887 (copies are also available in *PMSB*, PB/4/7/2/8, (formerly D.105); and in *HPR* 14.9). In tests where operators were told to maintain a smooth track their accuracy suffered accordingly. The winner of the prize for most inaccurate was off target by 25 degrees, but had a remarkably smooth track. For additional details, see I. Evans, "The Beginnings of Operational Research," *PMSB*, PB/4/7/2/16, (formerly D.112).

which the target was flying." Accordingly, AA Command, prior to Blackett's arrival, had already decoupled their predictors from gun-laying radar sets, and introduced additional old-fashioned plotting methods and range tables as additional intermediary technologies to find a temporary solution to the problem. Some of Blackett's Circus' work involved finding the best way of using this roundabout procedure. They also decided to have the Sperry predictor "amputated" with the help of engineers at the Admiralty Research Laboratory to accept input tracks smoothed over longer time spans. The Vickers predictors, however, could not be so modified. Fortunately, when using Vickers predictors, guns were aimed manually on the basis of the movements of a needle that responded to radar tracks. The object for the gun operators was to keep the needle from wandering by adjusting the gun according to the track, but savvy gun operators ignored the device's manual and the needle's human-induced twitching and adjusted the gun's aim smoothly. Studies of the method were made, and new rules—again, intermediary technologies—were issued to gun operators which instructed them to ignore "spurious movements of the needle and to respond to those which genuinely indicated the behaviour of the target."[84] This sort of work, like Hill's World War I group, was an extension of design and testing to incorporate additional phenomena pertinent to field use.

As we have seen, when Blackett departed Anti-Aircraft Command to become a scientific adviser to RAF Coastal Command in March 1941, the work at Anti-Aircraft Command continued apace under the auspices of the Ministry of Supply's Air Defense Research and Development Establishment (ADRDE), which was an entirely appropriate overseer for such work on account of its technological research on the same radar technologies on which Blackett's Circus had been working. The Circus was eventually

[84] Bayliss, "Origins".

renamed the ADRDE Operational Research Group (and later Army Operational Research Section 1), and its work was split largely between implementation of technology into its combat environment and research into operational conditions themselves, which required close liaison with field commanders. The first kind of work was extremely similar to what would later come under the rubric of the American Office of Field Service's OR groups. The second kind of work, considered by ADRDE (ORG) members to be real OR, was intended to help augment understanding of the fire control problem by the linking gunnery theory, equipment specifications and field conditions.[85] Only by determining which signals being sent to a predictor arose from artifacts of the human-machine system, which resulted from enemy behavior, and what sort of response could be expected from a gun of finite maneuverability and rapidity of fire, could the most effective equipment and techniques for tracking aircraft, and aiming and firing a gun be determined.

The group's efforts were intended in part to help the ADRDE and others produce (still more) new intermediary technologies—training regimens, user manuals, tracking methods, plotting methods—and, ultimately, newer and better predictors. Of course, most of the ADRDE's development and design efforts were the result of collating data received from equipment manufacturers, tests at experimental stations and laboratory tests done at other laboratories and at the ADRDE itself. However, the work of the ADRDE (ORG) was also an important component of the process. The group not only brought back data permitting the best priorities for research and development to be

[85] See ADRDE (ORG), "Memorandum on operational research sections, Ministry of Supply," June 1942, *HTT* 303.

determined in light of the most pressing needs; it also helped users implement technology into their operations effectively.[86]

In the United States, meanwhile, the National Defense Research Committee added its weight to American fire control research through its Division D-2, headed by Warren Weaver. Division D-2 was tasked with assembling similar theoretical pictures of the anti-aircraft problem as those being tackled in the ADRDE and the Admiralty Research Laboratory for use by fire control equipment manufacturers. While Weaver considered it useful to have a strong overarching theoretical framework, the most important thing was to develop models that could be translated immediately into updated equipment designs, which usually prioritized patching together disparate sets of data over developing elegant mathematics. The brief career of the MIT mathematician Norbert Wiener as a Division D-2 contractor is indicative of this set of priorities. While his mathematical talent was indisputable, he spent too much time trying to design an ideal predictor based on a fundamental theory he was developing, leaving Weaver to wonder whether Wiener's work was a "useful miracle or a useless miracle." Following the war Wiener's work, which took on the name cybernetics, was extremely influential as a basic mathematical language for discussing feedback mechanisms in all manner of ontological environments. Yet, while the war lasted, it was not producing a realistic picture of the anti-aircraft problem *as it stood*, and Wiener's contract was not renewed.[87]

[86] In addition to Bayliss, "Origins," see "AORG Radar Section 1941-5," c. 1946, TNA: PRO WO 291/1288; and D. W. Ewer, "The History of O.R.S. 1(b). A Summary of the Analysis made of the Operational Performance of the H.A.A. Defences. 1940-1946," Operational Research Group (Weapons & Equipment), Report No. 328, 12/6/1946, TNA: PRO WO 2901/304.

[87] On Wiener's work for NDRC Division D-2, see Mindell, *Between Human and Machine*, pp. 276-282. Quotes are from Warren Weaver diary, 7/1/1942, *NACP*, RG 227, Division 7 General Project Files [NC-138/86], Box 71, Collected Diaries, Vol., 4; quoted in Mindell, p. 280. See also Peter Galison, "Ontology of the Enemy: Norbert Wiener and the Cybernetic Vision," *Critical Inquiry* **21** (1994): 228-266; and Geof

Much of the work needed to develop useful studies was as much bibliographical as it was theoretical. In order to understand what the fire control problem looked like, it was necessary to amalgamate local studies already done by the research, development and testing facilities in the U.S. and Britain.[88] However, when preparing a device into which a great deal of technique was to be reified, one also always had to be especially aware of macro-problems, as Weaver later termed them. In designing a technology or an intermediary technology, one had constantly to make nearly irreversible choices such as what kinds of ammunition one wanted to fire, and how rapidly and accurately one wanted to fire it. How these choices were made depended on knowledge of what kinds of targets one was shooting at, how far away they were, how fast and erratically they were moving, and how accurately they were shooting back. If equipment designers did not weigh these problems carefully, they were likely to meet with the derisive comments of military commanders about how easily laboratory theories dissolved under the complexities of war.[89] Recall General Pile's dissatisfaction and how it led to the employment of Blackett at AA Command. To work with these problems Weaver began to incorporate operational data coming from England, including from the ADRDE (ORG).[90] But for *combining* different kinds of data, he was impressed by analytical techniques used in reports coming in from the Air Warfare Analysis Section.

Bowker, "How to Be Universal: Some Cybernetic Strategies, 1943-1970," *Social Studies of Science* **23** (1993): 107-127.

[88] On the importance of bibliography, see, for instance, Applied Mathematics Panel Study No. 25 (AMG-C No. 172), *NACP*, RG 227, Applied Mathematics Panel: Studies and Notes, 1943-1946 [NC-138/152], Box 6, Study 25, especially letter from Merrill Flood to Warren Weaver, 3/16/1943; letter from Warren Weaver to D. C. Lewis, 8/11/1943; and D. C. Lewis, "A Bibliography of AA Artillery," 5/19/1944.

[89] Weaver saw his Tactical-Strategic Computer as a linear descendent of fire control technologies; see "Comments," p. 16.

[90] See Rau, "Combat Scientists", 238-239.

In the waning months of 1942, Weaver moved from being head of Division D-2 (or Division 7, as it was renamed around that time) to being the head of the new Applied Mathematics Panel (AMP), which was set up as an independent division within the NDRC to provide specialized mathematical assistance for the work of the other divisions. As the former head of Division D-2, and the continuing head of Section 7.5 (the "analytical" section of the new Division 7), and as Division 7's representative on AMP, he grandfathered in as AMP Study No. 2 a particular fire control problem that had captured his interest: alternative arrangements of fighter armaments, which, according to his recollections in "Comments," had arisen

> out of the enthusiasm which a few of us [in Division D-2] had for the powerful and pioneering papers, "The Mathematical Theory of Air Combat" and "An Analysis of the Performance of a Fixed-Gun Fighter, Armed with Guns of Different Calibres, in Single Home-defence combat with a Twin-Engine Bomber"

none other than AWAS Note No. 1 and Report No. 1. According to Weaver, "We showed and praised this paper [sic] to various officers of the Army Air Forces and of the Naval Bureau of Aeronautics until, in self defense, they suggested that we try to digest and simplify these papers, interpreting them in terms not so formidably mathematical." Once convinced, the Navy contracted with AMP for the development of the theory for their needs, and the project was handed over to the statistician W. Allen Wallis and a Statistical Research Group (SRG) that was established at Columbia University.[91]

The development of the project proceeded much in the way that AWAS developed its theories in conjunction with military commands and OR personnel. The first application that the SRG considered was whether configurations of eight .50" caliber guns, or four 20mm guns were preferable. The first task was to set down what kinds of

[91] Weaver, "Comments," p. 35; on the assignment of the problem, see files on Applied Mathematics Panel Study No. 2, *NACP*, RG 227 [NC-138/152], Box 1, Study No. 2, which includes a fragmentary J. Wolfowitz, "Notes on *An Analysis of the Performance of a Fixed Gun Fighter etc.*," 7/25/1942.

information "(or of estimates, or of guesses)" were needed "before the answer would be forthcoming." As in the tactical planning in the previous chapter, the theory was based on military knowledge. Weaver recalled,

> These questions (nature of combat, bomber and fighter armament, value and variations of accuracies, ammunition, vulnerabilities, etc.), were then discussed at very considerable length with experienced officers. As a result estimates were arrived at which everyone agreed were almost certain to bracket the true values, although in many instances the true values were admittedly unknown.

These values, and the relationships between them, could be explored mathematically to ascertain their known implications. Again, as with mission planning, the point of analysis was not to eliminate uncertainty from the problem, but to eliminate enough uncertainty as was necessary to feel acceptably confident that one had arrived at the appropriate choice. In this case, for four out of five assumptions of vulnerability, the analysis favored the .50" gun configuration; for the fifth assumption, the 20mm guns had a 20% advantage in securing a favorable outcome of the duel. "The study was thus necessarily inconclusive," Weaver recalled. "It did, however, make clear just what sort of information was necessary to obtain a conclusive answer, and it furnished the necessary analytical methods." Or, as one of the SRG's reports on Cunningham's theory pointed out, "in conjunction with statistical theory, [the Mathematical Theory of Combat] can suggest ways to get the most information with the least experimenting, which is important when the planes, pilots, scientists, and time for experimenting are scarce."[92] Of course, like a military doctrine, once choices were in place, one had to reevaluate the

[92] Weaver, "Comments," pp. 35-37; and Statistical Research Group, Columbia University, "The Mathematical Theory of Combat," TNA: PRO AIR 52/97. As by-products, this study also issued important memos on the subject of optimum ammunition mixtures, on the optimum interrelation of aiming and dispersion errors. This concern is startlingly close to the question of quality control testing of munitions that prompted the development of sequential analysis in AMP; see chapter four.

problem continuously to make certain that one had formulated the problem correctly and that circumstances had not changed.

Warren Weaver and Operations Research

Although Weaver saw the greatest potential in the work of AWAS, he was also impressed by the advent of OR as more than just another source of data. Shortly after he began setting up his Panel at the end of 1942, he found himself running into it at nearly every other turn. Although the Panel ended up working quite differently, he originally had intended to have the panel comprised of representatives of the various NDRC divisions, and a small group of core mathematical specialists. To find his representative for Division 6, which was dedicated to subsurface warfare, Weaver telephoned the head of the division, University of Minnesota physicist and *Physical Review* editor John Tate, who informed him that the best man was Philip Morse, who had then been head of ASWORG for almost eight months. At that particular moment, though, Morse was in England with William Shockley, investigating the OR work going on there, and could not immediately reply to the offer.[93] Among Weaver's first choices for his core group of mathematicians was Samuel Wilks, a statistician in Princeton University's high-powered mathematics department. Weaver soon found out he was already working part time developing analytical methods for ASWORG. Having already made significant progress with their problems, Wilks agreed to join the Panel full time while maintaining a connection with the OR group.[94] Five days later, consulting with the head of NDRC Division 2 (researching impacts and explosions), Weaver was informed that Bob

[93] Diary of Warren Weaver, 12/7/1942; and letter from John Tate to Warren Weaver, 1/12/1943; both *NACP*, RG 227, [NC-138/153], Box 10, "Division 6" folder.
[94] Diary of Warren Weaver, 12/16/1942, *NACP*, RG 227, [NC-138/153], Box 9, "Wilks, Samuel S." folder

Robertson was their "chief theoretical man," but that he, like Morse, was "in England doing operations research for the AAF."[95]

It is unclear exactly how much Weaver learned and when about the various activities of ASWORG and the operations analysis section that had just been established in the Eighth Air Force. However, it seems likely that he would have gained at least some sense from Wilks of some of the more theoretical work being done in the former organization. The most theoretically rigorous problem ASWORG was then investigating was the question of how to search for a U-boat that had not been successfully attacked. Typically, U-boats remained on the surface, because their technology only allowed them to move so fast and remain submerged so long before they had to surface to replenish air supplies and charge their batteries. So, the search problem entailed balancing an appreciation of the U-boat's desire to accomplish its mission, its technical capabilities, and the technical capabilities of search aircraft into a guess of where the U-boat could most likely be found again. The Navy, of course, had its own tactics. The pressing need was to find out how good those tactics were versus how good they could be. In addition to some of the other problems being dealt with in Division 6 and at ASWORG, such as calculating the probability of torpedo hits (which had close similarities to calculating hit probabilities for anti-aircraft fire), this was just the sort of problem that Weaver's new panel was after.[96]

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[95] Diary of Warren Weaver, 12/21/1942, *NACP*, RG 227, [NC-138/153], Box 10, "Division 2" folder. The head of Division 2 was the MIT architect John Burchard.

[96] On search theory, see Bernard Osgood Koopman, *Search and Screening: General Principles with Historical Applications* (Elmsford: Pergamon, 1980); and Philip M. Morse and George E. Kimball, *Methods of Operations Research* (New York: The Technology Press of Massachusetts Institute of Technology and John Wiley and Sons, 1951), pp. 86-94.

Through December 1942 and January 1943, Weaver increasingly began to embrace the idea that there was a close relationship between the sort of mathematical analysis involved in fire control computations and certain kinds of analysis done by mathematically-trained personnel in OR sections. To find out a little more about what OR was really supposed to be, he wrote to Bennett Archambault, the head of the OSRD's London liaison office, who consulted with Bob Robertson, who was still working with the Army Air Forces in setting up the operations analysis section in the Eighth Air Force. Robertson, in turn, solicited the RAF's Operational Research Centre,[97] the head of which pointed out that a report had been put together on the subject for OSRD head Vannevar Bush some months earlier.[98] Bush had commissioned the report to settle the relationship between OSRD and OR. As we have seen, he preferred to keep his organization out of it, feeling that research into operations was a military rather than a research and development issue—a position that determined the structure of the AAF's operations analysis sections. Weaver was aware of Bush's position,[99] but he was also growing increasingly sensitive to tensions created by the different functions of OR as an aide to military planning, and as a bridge between equipment design and field requirements. On account of the mobile boundary between tactics and technology, though, neither activity really stood separate from the other. Military practice was based on the capabilities of equipment, and equipment design was based on military practice.

[97] The RAF's Operational Research Centre was an organization set up in the fall of 1941 to correlate and distribute RAF OR reports; see chapter six.
[98] See letter from Warren Weaver to Vannevar Bush, 2/8/1943, *NACP*, RG 227, [NC-138/153], Box 4, "Operational Research" folder.
[99] He was aware of it at least as early as November 1942, see memorandum from MHT (OSRD) to WW, 11/6/1942: "With regard to operational research, Ward Davidson has this to state: That there is no operational research done under NDRC direction. This is because of a conviction of Dr. Bush that operational research must be integrated so closely to operations that it would not be feasible for an outside organization to maintain direction." *NACP*, RG 227, [NC-138/153], Box 4, "Operational Research" folder.

Weaver felt that working out the relationship between OR and equipment design had a direct bearing on the future of AMP. He wrote to Bush in late February 1943:

> I am officially interested in [Operational Research] for three reasons: first, because various activities of Section 7.5 [Fire Control Analysis] and of the Applied Mathematics Panel depend upon getting information back concerning operational results; second, because some of the present mathematical activities would actually appear to fall within some broad definition of Operational Research; and third, because the Navy has informally indicated more than once their possible interest in adopting the personnel of one of our contracts as an Operational Research group.

Simply on a practical basis Weaver was having difficulty figuring out what relationship AMP work should have with OR work. He observed that the "general subject has suffered distinctly from a lack of clear definition of words. The term 'Operational Research' is actually being applied to activities of very different characters; and certain observations and conclusions concerning Operational Research are confused and vague." He drew upon the antisubmarine problem to illustrate his point. "I suggest," he wrote to Bush, "that the subject would be clarified if we agreed [...] to speak on the one hand of 'Operational Research – Submarine Warfare', and on the other hand of 'Submarine Warfare Analysis.'"[100]

Weaver felt that OR, proper, was any ongoing analysis of data collected directly from the field that helped the military to make better decisions. Much of this work would entail the analysis of new equipment in use in the field, and, in fact, some OR groups might be dedicated expressly to such problems. He could not think of a good name for this purely equipment-related kind of activity, but described groups performing it as "a sort of engineering service crew" specializing in the "operational deficiencies of new equipment." Weaver felt that Bush was correct, and that these groups should be separate from the OSRD, but that the OSRD would want to keep in close contact with them.

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[100] Letter from Warren Weaver to Vannevar Bush, 2/25/1943, *NACP*, RG 227, [NC-138/153], Box 4, "Operational Research" folder.

Warfare analysis, on the other hand, meant working out the underlying theory "of tactical procedures, of instruments, and of systems." Such groups, he felt, should work "under fundamental scientific (OSRD) auspices". Unlike OR personnel, such groups did not have to work in the field, though he did feel they should maintain "cordial and cooperative" relations with the military.[101]

Weaver then sorted the work of various OR activities with which he was familiar into one or the other category. The RAF OR Sections, he felt, did proper operations research, as did two smaller OR groups that had been set up in the United States Army Directorate of Air Defense and the Army Signal Corps, which were really only doing what he was calling engineering service work. However, certain projects in NDRC Division 7 and Cunningham's AWAS were clearly dedicated to warfare analysis. The ADRDE (ORG) and ASWORG (with its analytical work on searches) did work in both categories. Whatever arrangements were made, Weaver felt that "the analysis groups are sorely in need of better contact with the Operational Research groups. At the present time the former simply are not getting the data from the latter." Weaver, already excited about the possibilities of Cunningham's combat theory, obviously wanted AMP to have a major part in the development of warfare analysis in the United States. As on the British side of things, though, reliable knowledge of field conditions was required before such work could be of any real practical value, which, as we have seen, was Weaver's paramount concern.[102]

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[101] *Ibid.*

[102] *Ibid.* It is at this point that consequences of Philip Mirowski's misapplication of the labels "American OR" and "British OR" to mean different types of OR begins to become clear. What Mirowski sees as American OR is close to what Weaver called "warfare analysis". By *geographically* separating the concepts, Mirowski seems to dismiss the notion that Americans interested in warfare analysis were *also* interested in field research, and yet, as is plain, Weaver (whom Mirowski calls a "Grandmaster Cyborg") was a key advocate for linking field research ("OR") with warfare analysis. (Mirowski is also apparently

Weaver never made any attempt to pull ASWORG's analytical work on the problem of search into AMP, even though he clearly associated it with the "warfare analysis" he was appropriating into AMP.[103] But AMP found its finger in ASWORG's mathematical pie soon enough—much to ASWORG's benefit. Morse joined an AMP executive committee meeting on March 8th, 1943, and informed the panel of various theoretical problems arising in the work of Division 6 and ASWORG.[104] Weaver saw ample opportunity for collaboration. In a letter to John Tate, he pointed out the similarities between torpedo-shooting problems and anti-aircraft gunnery problems, and seemed especially excited about the search problem. It was, he wrote, "entirely new to us," and he felt it was "of sufficient importance and novelty to justify consideration by more than one group".[105] The Applied Mathematics Group at Columbia University (AMG-C), an AMP contractor headed by Northwestern University mathematician E. J. Moulton, subsequently did undertake some work on the search problem on behalf of ASWORG, and included the local Columbia mathematician Bernard Koopman in it, even

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unaware that much of what comprised warfare analysis, was, in fact, a British invention.) This point is like a thread that if pulled far enough threatens to unravel Mirowski's entire argument about the nature of economic theory and its relationship to its ontology. By confining practical checks to a British context (he further claims British OR was summarily destroyed in the Cold War backlash against the leftist planning of science movement; but see chapter 6 here), Mirowski removes what, to Weaver and many others, was the entire reason abstract theorization was a permissible activity. Theory, such as Wiener's cybernetics, was not something that was expected to be immediately applicable; rather it was something that could provide *insights* into real behaviors. It is possible that the barrier between OR and neoclassical economic theory provides Mirowski some breathing room, but given his failure to understand the nature of OR, further analysis on the side of economic theory is clearly warranted; see Appendix A of this dissertation for further discussion. Mirowski discusses the distinction between American and British OR in Philip Mirowski, "Cyborg Agonistes: Economics Meets Operations Research in Mid-Century," *Social Studies of Science* **29** (1999): 685-718; and Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (New York: Cambridge University Press, 2002).

[103] A Washington-based branch of ASWORG that would eventually be called its "Operations Research Center" (not to be confused in form or function with the RAF's "Operational Research Centre") was, effectively, an in-house AMP.

[104] E. J. Moulton, "Diary of Executive Committee Meeting," 3/8/1943, *NACP*, RG 227, [NC-138/153], Box 1, "AMP Meetings, 1943-1946" folder.

[105] Letter from Warren Weaver to John Tate, 3/15/1943, *NACP*, RG 227, [NC-138/153], Box 10, "Division 6" folder.

though Koopman had no formal affiliation with AMG-C. That summer Koopman, on his own initiative, produced a document for Moulton entitled "A Quantitative Aspect of Combat" that detailed the statistical effects fighting groups of differing sizes and different fighting strengths would have on each other.[106] It did not have any immediate applications, but it generated considerable interest at AMP, which distributed it for comment. Morse and others soon informed Weaver that the paper essentially replicated the theoretical work done by British engineer Frederick Lanchester during World War I.[107] The incident embarrassed AMP slightly, but Moulton and the AMG-C was still impressed enough with Koopman's (avowedly independent) work to bring him on full time beginning in 1944.[108] Almost immediately, though, AMP decided to step up its liaison with ASWORG, and Koopman transferred over to Morse's organization, where he would become the figure most associated with search theory. When OR eventually became a theory-generating profession, search theory would in retrospect come to be recognized as the only major theoretical OR product of the war.[109]

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[106] B. O. Koopman, "A Quantitative Aspect of Combat," 6/23/1943, *NACP*, RG 227, [NC-138/153], Box 7, "Koopman, B. O." folder.

[107] See letter from Philip Morse to Warren Weaver, 10/1/1943, *NACP*, RG 227, [NC-138/153], Box 10, "Division 6" folder., and letter from E. J. Moulton to Philip Morse, 10/6/1943, *NACP*, RG 227, [NC-138/153], Box 7, "Koopman, B. O." folder. Lanchester's work had been held up as a sort of predecessor to OR. See Blackett, "Methodology of Operational Research," p. 198; Morse and Kimball, *Methods of Operations Research*, pp. 63-77. One of the top prizes given out by the Institute for Operations Research and Management Science is called the Lanchester Prize. For the original work, see F. W. Lanchester, *Aircraft in Warfare: The Dawn of the Fourth Arm* (London: Constable and Company, 1916). Earlier mathematical work along similar lines had apparently done under U. S. Navy auspices as early as 1902; see Bradley A. Fiske, T*he Navy as a Fighting Machine* (New York: C. Scribner's Sons, 1916) and references to earlier material listed therein; references in memorandum from Dr. Winsor of Frankford Arsenal for Merrill Flood, 10/21/1943, *NACP*, RG 227, [NC-138/153], Box 7, "Koopman, B. O." folder. Koopman's document also made an impression on Weaver; see "Comments," p. 31.

[108] Letter from E. J. Moulton to J. F. Ritt, 12/3/1943, *NACP*, RG 227, [NC-138/153], Box 7, "Koopman, B. O." folder.

[109] AMP's decision to increase its liaison with ASWORG and the Navy was apparently inspired by a December 1943 talk given by the visiting Patrick Blackett. AMP originally proposed to send the mathematician Leonid Hurewicz to ASWORG. Wilks "talked it over with Phil Morse and the other ASWORG people, and they turned thumbs down on [Hurewicz] because of accent, recent naturalization, general personality." See memorandum from WW to EJM, 1/8/1943 [sic, 1944], *NACP*, RG 227, [NC-

John Williams, Edwin Paxson, AMP, and OR

[Operations analyst James] Clarkson said he thinks it is the best paper on bombing that he has ever seen – apparently it isn't possible to like or dislike [Berkeley mathematician Jerzy Neyman's] papers just a little. I remarked that we had felt the recommendations were overstrong and that the tacit assumptions were more-than-passably unrealistic. He replied that yes these things were true, but that nobody would take the results of a paper written here and rush out to apply them in the field, and he is sure JN is such a reasonable fellow that you'd only have to mention these objections in order to get him promptly to make the necessary changes.

from the wartime travel diaries of John D. Williams, 2/13/1945[110]

It has never been clear to me why mathematicians adopt a trade union attitude toward attempts to apply mathematics to such matters as biophysics and psychology. The fields are sensibly virgin, complex, and demand an unrelenting comparison with experiment. And experiment is mathematical prophylaxis.

Edwin Paxson to Oswald Veblen, 2/15/1945[111]

While in March 1943 Warren Weaver felt that warfare analysis activities of AMP projects would only "eventually" require access to operational data, over the remainder of the war, the distinction between AMP's activities and OR grew increasingly blurred. In addition to projects from the various NDRC divisions, AMP also took on contracts from the military itself to investigate the theoretical underpinnings of the same military activities that OR studied, such as bomb aiming.[112] AMP studies of military problems were mostly quick, isolated affairs, but were always performed with their operational context in mind in order to ensure their validity relative to more arbitrary solutions. They were, effectively, "micro-problems" being solved within a macro framework, to employ Weaver's terminology. To provide a flavor of how AMP studies were accomplished, I would like to make a swift survey of the wartime careers of two AMP mathematicians, John Williams and Edwin Paxson, who, following the war, would go on to join the

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138/153], Box 7, "Koopman, B. O." folder. Because ASWORG worked so closely with various members of the military, it was considered important that the rank and file not be put off by scientists' personality (or, apparently, foreignness). Instead, ASWORG decided they wanted Koopman on a permanent basis.

[110] Diary of J. D. Williams, 2/13/1945, *NACP*, RG 227, [NC-138/153], Box 1, "Williams, J. D. Diary" folder.

[111] Letter from Edwin Paxson to Oswald Veblen, 2/15/1945, *NACP*, RG 227, [NC-138/153], Box 8, "Paxson, E. W. Correspondence" folder.

[112] On the ongoing relationship between AMP and OR, "Excerpt from WW's AMP diary," 12/17/1943, *NACP*, RG 227, [NC-138/153], Box 4, "Operational Research" folder, is especially useful.

RAND Corporation, where they would extrapolate their wartime experience into newer, more ambitious research programs.

John Williams did not graduate from high school, instead devoting his later youth to "such matters as racing cars, pocket billiards, and philosophy," with a dose of "the physical sciences and mathematics" reflecting an "old fashioned notion of natural philosophy." When he got into college at the University of Arizona he trained as an astronomer, but switched to mathematics as a graduate student at Princeton where he worked under Bob Robertson. After World War II broke out in Europe, he decided he wanted to make some sort of contribution, and so Robertson introduced him to a senior ordnance officer at the Aberdeen Proving Ground, who gave him a host of problems to work on. Then, "to get a little diversification," he "went touring." He later recalled that he rather brazenly "showed up at the front door" of various military agencies "with no credentials except my mouth and talked my way in, talked the problems out of them, and talked my way out. I came back home heavily laden with new problems. I found myself working for three or four government agencies."[113]

The problems with which Williams dealt were of the familiar equipment and tactical development kind. For instance, "the Field Artillery Board wanted to know the optimum way to use fragmentation shells in pattern fire against personnel protected in various ways; in trenches or standing or prone, etc." The Aberdeen Proving Ground wanted him to sort out the various factors affecting the firing of long range guns, "effects due to winds from the intrinsic variations in the guns themselves; in the munitions, the

[113] Bornet, "John Williams," pp. 4-5, 30. It seems safe to say that Williams was tending toward hyperbole when saying he had "no credentials".

rifling, and what not."[114] Williams' work eventually brought him to the attention of Weaver, then still the head of Division 7, who thought it might be a good idea to "'legitimatize' these bootleg activities," and a contract was later arranged with Princeton for Williams to work with them.[115] He diversified his experience still further when he became the AMP liaison with the Guided Missiles Division (Division 5) of the NDRC, as well as a consultant with the Twentieth Air Force's Pentagon staff. He recalled later,

> There were episodes such as flying as bombardier in a B-17, with a bunch of experimental AZON (guided missiles) in the back, at Eglin Field. Or arguing with Jerzy Neyman of Berkeley on how to compute a certain function. I spent more time with people like Neyman than with things like bombers. It was something like life in RAND, but gone to hell: too much time in bad airplanes, in bad hotels, producing bad studies too quickly.[116]

What "bad studies too quickly" entailed was something similar to what Curtis LeMay meant by a "wrong" plan. Solutions to decision problems in war were inelegant: they were not the best possible solution that could be found if sufficient time could be given to developing an analysis. Like a military doctrine, an analysis might not only not be optimal, there was no guarantee it was not outright harmful; but it was assumed in most cases to be better than an arbitrary guess.

This swift and loose method of analysis was exemplified in a discussion Williams had with fellow AMP members, Allen Wallis, Jacob Wolfowitz and Abraham Wald on unspecified problems faced by ASWORG relating to convoys. Williams suggested

> that one should attack this problem, in the first instance at least, by 'quick and dirty' methods; rather than, say, to square off promptly on a six month's computing program. To this end it was recommended that they begin with a first rough approximation; however owing to the things which they thought should be considered in a first approximation (e.g. errors in torpedo speed, maneuvering, etc.), we gradually worked

[114] *Ibid.*, pp. 5-6. He had some success with this last problem, where, apparently, the great John von Neumann had not produced a solution, although Williams did not know how long von Neumann had actually worked on it.
[115] *Ibid.*, p. 7.
[116] *Ibid.*, p. 10.

backwards until I recommended that they start with the 0^{th} approximation and finally that they start with the -1 th approximation.[117]

As with Cunningham's combat theory, the object was to find a foothold for understanding the problem. By explicitly stating a generalized mathematical theory, it became easier to discern in what ways the theory was perturbed by other factors. This understanding would ultimately lead to more nuanced formulations, until, ultimately, one was able to recommend alterations to plans with more nuanced reasoning than that possessed by military planners. For instance, one could determine if there were ways to determine optimal bomb patterns to use against a target, or whether there was a better tactic for aiming bombs. These recommendations were sometimes encapsulated in physical computers, such as nomograms and slide rules (such a slide wheel designed by Edwin Paxson is shown in Figure 2.5). Planners could use them to take a measured or estimated input and determine the appropriate response. The actual construction of such simple computers was doubtless one inspiration for Weaver's fanciful Tactical-Strategic Computer.

The way to make recommendations more sophisticated than existing practice was not through the virtue of mathematical formulation, but by finding a way to convert what planners already knew into mathematical language and manipulating the mathematical representation of the problem in useful ways to tell planners something they did not already know. In his wartime travels, Williams frequently came into contact with a variety of individuals who might be identified as gatekeepers to the military's knowledge about technical and tactical developmental problems. Judging from his travel diaries, he spent a great deal of time talking with officers and technical experts at military

[117] Diary of J. D. Williams, 11/20/1943, *NACP*, RG 227, [NC-138/153], Box 1, "Williams, J. D. Diary" folder.

experimental establishments attempting to ascertain what their tactics were, what the rationales behind those tactics were, what kind of data was available for determining tactical and technical effectiveness, and, of course, determining whether the tactics and technology involved in plans was actually used in real combat situations. Oftentimes the most useful gatekeepers to military knowledge turned out to be OR personnel. Once the bombing campaign against Japan began to escalate dramatically in 1945, Williams became especially deeply ensconced with AAF operations analysis personnel who proved to be excellent interlocutors on the intricacies of bombing techniques, and often provided a source of information and data for AMP analyses.

AMP work was, on the whole, more sophisticated than the work of the operations researchers whom he met. In a meeting with operations analysts on their way to working with the Thirteenth Air Force, Williams and some of his AMP colleagues gave them a swift overview of AMP work on areas such as the

> train bombing problem, principally as applied to a single airplane, including some discussion of the bombardier's calculator. There was also some discussion of the maneuvering target problem; a very brief discussion of guided missiles; still briefer mention of incendiaries; balloon barrages were converted into mine fields and discussed as such.

They also discussed tracer fire, the "adaption [sic] of a lead-computing tail gun sight to the chin turret position" and "problems of aerial free-gunnery." Williams recorded in his travel diary that after giving them all of this information, "the visiting firemen were turned loose for the night (at their request), to cope with the intellectual indigestion as best they could."[118] Occasionally, Williams was even able to find data for operations researchers. Bob Youden, a senior operations analyst, called Williams "his first class moocher" after Williams secured a promise of data from Lt. Col. Robert McNamara, a

[118] Diary of J. D. Williams, 10/6/1943, *NACP*, RG 227, [NC-138/153], Box 1, "Williams, J. D. Diary" folder.

member of Army Air Forces' Statistical Control Section (and, of course, a future Secretary of Defense). Two weeks later, McNamara indeed "came through very nicely" in providing Williams with a "nice fat bunch of Consolidated Statistical Summaries from the 21st [Bomber Command]".[119]

Still, OR's unique role as investigators of command activities provided them with comprehensive views of operations through the accumulation of data from across specialist boundaries. Neither AMP nor statistical analysts such as those in McNamara's team had the power to assemble such pictures rapidly. To illustrate: on visiting the Combat Analysis Branch of the AAF's Statistical Control Section, Williams and Samuel Wilks were informed by George Dantzig (who would become an icon in postwar OR for developing the linear programming optimization technique) that a study on bridge bombing "from the operational side" would take time to develop, "since it would have to be pursued along slightly informal lines." A formal study "would never get to first base, because collecting the data would necessitate cutting across half a dozen departmental lines such as Photo Interpretation, Intelligence, etc, and that it would just naturally get bogged down somewhere." Combat Analysis Branch was supposed to "determine practice and combat accuracies" and was not in a position to undertake such wide-ranging studies.[120] OR personnel were often not so inhibited, and that is what made them such an invaluable complement to mathematicians such as Williams who generally worked far removed from field commands and did not know how to relate their work to the military's needs.

[119] Diary of J. D. Williams, 1/30/45 (Second Report), and 2/13/1945, *NACP*, RG 227, [NC-138/153], Box 1, "Williams, J. D. Diary" folder. On Youden see Hugh J. Miser, "Craft in Operations Research," *Operations Research* **40** (1992): 633-639.
[120] Diary of J. D. Williams, 4/26/1944, *NACP*, RG 227, [NC-138/153], Box 1, "Williams, J. D. Diary" folder.

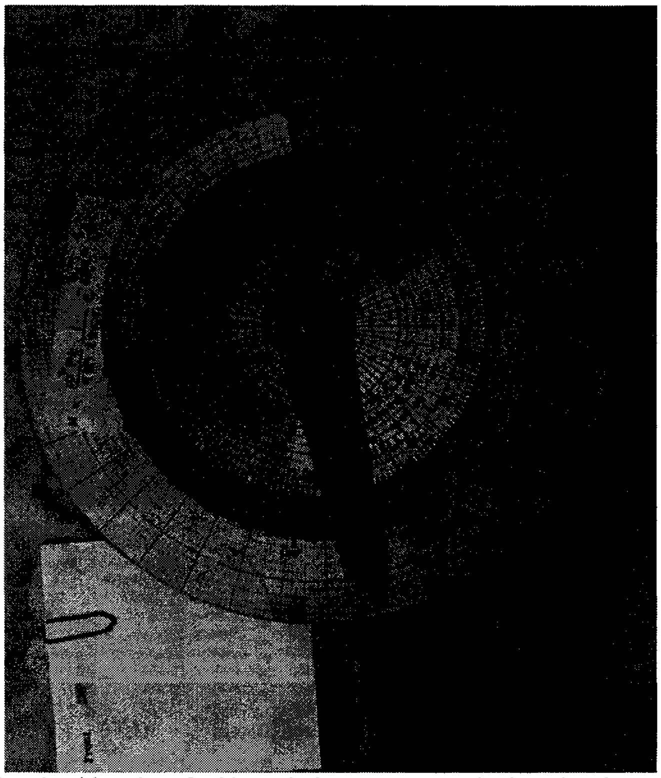

Figure 2.5. A slide wheel hand crafted by Edwin Paxson to calculate the chance of scoring a kill in an aerial battle. Source: *NACP*, RG 227, Applied Mathematics Panel: General Records, 1942-1946, Box 8, "Paxson, E. W. Correspondence" folder. This is a photograph taken by the author of the actual slide wheel.

Meanwhile, Edwin Paxson had a rather different background and wartime career trajectory from Williams. In 1934 Paxson received his bachelor's degree in mathematics from the California Institute of Technology, and three years later he received his PhD from there as well. From 1937 until 1942 he was a professor of mathematics at Wayne University in Detroit, but after Pearl Harbor he became an expert on the mathematics of aerial gunnery while designing training films for gunners for the Detroit-based Jam Handy Organization, a film production company.[121] These films used stop-motion models to demonstrate what kinds of combat situations gunners on bombers were likely

[121] See Paxson biography in RAND Corporate Archives, Box 13, Bob Specht's RAND biographies, M-R. To give some notion of the Jam Handy Organization's typical wartime activities: according to the Internet Movie Database <imdb.com>, wartime credits of the Jam Handy Organization include such scintillating titles as "Uncrating and Assembly of the P-47 Thunderbolt Airplane".

to witness and told them where they should aim in each case.[122] In January 1944, at the instigation of Edward Bowles, the OSRD obtained Paxson and its Office of Field Services sent him to Eglin Field in Florida to become a scientific adviser to General Grandison Gardner, who was in charge of the proving grounds there. The idea, Paxson thought, was to take on a position similar to Bowles' other consultants. Upon arriving, he was surprised to find that Gardner had been informed by General Arnold that "the Germans have a sort of braintrust who 'sit in a room and think.'" Arnold wanted to try something similar at Eglin. The so-called "Technical Planning Committee" that resulted was comprised of Paxson, an industrial physicist, "a man with considerable ordnance experience from the Sperry Company, and a small-town lawyer from New Jersey, who had for several years been an attic enthusiast about radio." Once this experiment had been tried long enough to report back to Arnold that it did not work, Paxson took on a role much closer to the advisory one he had been expecting in the first place.[123]

In this position, Paxson soon came into contact with AMP, and had a number of ideas about what role it ought to be playing. At one meeting, Paxson chided Weaver and other members of the panel for not doing any work on a certain kind of sight, to which Weaver, taken aback, replied that the panel had nothing to do with the sight. According to Weaver's notes on the meeting, Paxson, in turn, pointed out that "certain tactical problems have to be solved quickly and on the spot in the various theatres of war. These are presumably to be handled by the Operational Research Group. Then there are certain basic long-range developments [...] which the NDRC is particularly well equipped to

[122] On Paxson's activities at the Jam Handy Organization, see Rosser, "Mathematics and Mathematicians in World War II"; and Thiesmeyer and Burchard, *Combat Scientists*, pp. 241-242.
[123] See memorandum from Edward Bowles for General Arnold, 1/3/1944, *ELB*, Box 30, Folder 5; Diary of Warren Weaver, 2/24/1944, *NACP*, RG 227, [NC-138/153], Box 8, "Paxson, E. W. Correspondence" folder.

handle." However, there were also "a lot of stop-gap problems." These could be addressed with "methods or pieces of equipment which are admittedly not perfect, but which are nevertheless very much better than nothing." Included in this category was the sight in question. He had chosen the correct words. Inelegant but innovative solutions to diverse problems were AMP's bread and butter. Weaver replied that AMP's leadership had "been preaching exactly this same philosophy on every possible occasion during the last twelve or eighteen months." Returning to New York, he felt that AMP should do everything in their power to help Paxson.[124]

By July 1944 Weaver decided that he wanted Paxson to work for AMP as the panel's resident expert on aerial gunnery, and, after securing the permission of Gardner and Bowles, Paxson moved the next month.[125] To replace him, Eglin Field was to receive an operations analysis section (which, Paxson had tried to explain to the general, was different from NDRC aid in that it would work on general command problems, not just help with certain kinds of equipment tests).[126] Paxson's duties at AMP included correlating "the large number of existing activities in this field," collecting and maintaining "a complete library of the subject," and preparing and distributing "frequent bulletins which could be distributed by AMP to all interested agencies."[127] Effectively, he was to AMP on aerial gunnery what A. V. Hill had been to the Munitions Inventions Department on anti-aircraft gunnery almost thirty years earlier. However, his most

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[124] Diary of Warren Weaver, 2/24/1944, *NACP*, RG 227, [NC-138/153], Box 8, "Paxson, E. W. Correspondence" folder.
[125] Letter from Warren Weaver to E. W. Paxson, 7/28/1944, *NACP*, RG 227, [NC-138/153], Box 8, "Paxson, E. W. Correspondence" folder.
[126] See memorandum from E. W. Paxson, 7/23/1944, "Paxson, E. W. Diary" folder; and letter from E. W. Paxson to Warren Weaver, 8/15/1944, *NACP*, RG 227, [NC-138/153], Box 8, "Paxson, E. W. Correspondence" folder. No record of such a section exists in the postwar list of wartime operations analysis sections. It is unclear whether, in the end, Gardner received any new personnel to replace Paxson.
[127] Letter from Warren Weaver to Edward W. Paxson [sic], 8/21/1944, *NACP*, RG 227, [NC-138/153], Box 8, "Paxson, E. W. Correspondence" folder.

important new job at AMP was to assist on a new project the panel had landed to determine the best tactical use of the B-29 bomber. Paxson would play a central role in the evaluation of the B-29's central fire control system, but studies of the B-29 technology are extremely important in understanding a number of postwar developments, so it will make sense to look at the question of the B-29 from a broader perspective.

Studies of the B-29

During World War II operations research groups caused a certain amount of misery by making demands for the scientific personnel employed by others. In January 1944, though, it was Edward Bowles who scored an unusual personnel coup against an OR group, enticing ASWORG's director of research, Bell Telephone Laboratories physicist William Shockley, to become one of his expert consultants.[128] According to Bowles' later recollections, Shockley had been frustrated by ASWORG's inability to obtain field data from the Navy, and had, in fact, been receiving much of the information he wanted from Bowles and decided to go to work for him instead. For his part, Bowles felt Shockley's OR experience made him well suited for tackling the questions of technological implementation handled by his office. Accordingly, Shockley's first task was "to assist in the exploitation of the operational potentialities of the B-29 bombardment aircraft, particularly by integrating into the [Very Heavy Bombing] program all those special devices, instrumentalities and techniques which will

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[128] On Shockley, see Joel N. Shurkin, *Broken Genius: The Rise and Fall of William Shockley, Creator of the Electronic Age* (New York: Macmillan, 2006). Shockley is most famous as one of the inventors of the transistor, for which he would win a share of the Nobel Prize in Physics. He later became a figure of public controversy because of his views on genetics, intelligence, and race.

appreciably enhance its effectiveness."[129] The B-29 Superfortress, built by Boeing, was by all accounts a magnificent piece of aeronautical engineering, flying higher, faster, farther and more heavily loaded than any bomber before it. It had, however, been rushed into production without any thorough consideration of its operational deployment, especially with respect to its defensive armament.[130] Taking into account the aircraft's physical capabilities, and the operational results of its use, Bowles wanted to develop a clearer idea of how it could be best integrated into the war machine.[131]

Shockley threw himself into the B-29 problem with the sleuthing gusto characteristic of OR work. After studying the problem in the whole, he soon identified H_2X navigation (a radar that produced a "map" of surrounding terrain on the radar's scope) and the training of radar operators as areas requiring further scrutiny. Over the course of 1944, Shockley studied the problem and made recommendations focusing largely on the development of training practices, such as familiarizing operators with the technique involved in using a radar scope image as a navigational aid. Shockley's emphasis on training resonated well with the meticulous General LeMay, once he took over the XX Bomber Command, and a new emphasis was placed on training forthwith. In the wake of these programs, Shockley made studies comparing bombing runs using well-trained and poorly-trained personnel that showed that even a small amount of effort

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[129] Letter from Edward Bowles to William Shockley, 1/4/1944, *ELB*, Box 32, Folder 6. Bowles obtained Shockley just as he secured Paxson as an adviser to Grandison Gardner at Eglin Air Force Base. Bowles' first Advisory Specialist Group was still over two months from being established. On Shockley's activities in Bowles' office, see Hazeltine, "Summary of Activities," pp. 84-87. On the circumstances surrounding Shockley's move to Bowles' office, see Edward Bowles' note, 12/15/1971, *ELB*, Box 42, Folder 4; and interview with Bowles, 7/15/1987, *NASM* RAND Oral History Project, p. 66; and Philip M. Morse, *In at the Beginnings: A Physicist's Life* (Cambridge, Mass.: The MIT Press, 1977), p. 187.

[130] Although preparations for the tactical use of the B-29 are not covered, see Wilbur H. Morrison, *Birds from Hell: History of the B-29* (Central Point, OR: Hellgate Press, 2001).

[131] See Hazeltine, "Summary of Activities," pp. 84-87.

taken from running operations would yield eight or even ten-fold improvements in bombing accuracy.[132]

Bowles approved of these economic studies showing the pay-off of decisions such as emphasizing training, and upon finishing his work on radar navigation training, Shockley devoted himself to determining to what extent B-29 bombardment was worthwhile by making estimates of economic damage done in Japan to the cost of bomber losses and running the program. The result was a study entitled, "A Quantitative Appraisal of Some Phases of the B-29 Program," which estimated that damage caused exceeded losses by a factor of sixty; or an investment of .5% of the American war effort would do 25-50% damage to the annual Japanese war effort. As we will see, this study made an impression on Bowles.[133] It also seems to have put Shockley in the mood for playing with statistics, leading him to propose still more ambitious studies. The title of a February 1945 memo Shockley wrote to Bowles, "Discussion of a Proposed Program on the Quantitative Aspects of Modern Warfare," seems to indicate an ominously open-ended extension of his B-29 report issued some months earlier. Drawing inspiration from certain studies performed in Britain by Patrick Blackett at the Admiralty on ship production allocation, and by the Ministry of Home Security estimating varying amounts of effort required to create different effects on German industry, and making a few of his own quick calculations, Shockley now questioned the B-29 bombing campaign by comparing casualty rates of different kinds of combat, pointing out that the Air Forces

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[132] Shockley settled on H_2X training early; see memorandum from William B. Shockley to E. L. Bowles, 2/3/1944, *ELB*, Box 32, Folder 6; and Hazeltine, "Summary of Activities," pp. 85-86.

[133] Hazeltine, "Summary of Activities," p. 86. See also interview with Edward Bowles, *NASM* RAND Oral History, pp. 65-66, 70-71; from a second interview on 8/20/1987, pp. 92-93, and 112. At one point in the 7/14/1987 interview (p. 73), Bowles compared his office's work on the economics of warfare with the work Patrick Blackett was doing at the Admiralty.

were losing one man for every four man-years of "Jap labor" destroyed. This estimate compared to a ratio of seven Japanese losses to one American loss due to infantry combat, and ten-to-one losses (rising to thirty-to-one losses) in fighter combat.[134] In any case, he decided, "What is required is an evaluation of the program in respect to the [war] plan as a whole. It may be that particular returns of this effort are already worth the cost, and it is highly probably [sic] that with decreasing range and improved operations, a large profit ratio will be achieved." More study was, of course, needed, but never accomplished.[135]

Meanwhile, when Shockley was still ensconced in radar navigation training programs and his economic studies of attrition in the summer of 1944, Bowles also decided to gather a mixed military and civilian team to see what means of attacking the Japanese mainland from bases then available, or expected to be so soon, would be feasible. The idea was to assess

> the instrumentalities available to do the job, including aircraft, radar, television [H_2X radar] and other means available for guiding aircraft to the targets, to study the mission to be performed, to formulate a specific technical plan for accomplishing it, to coordinate preparations for the execution of it, and to give the necessary technical support to the ultimate operations themselves.

As a first step, Bowles signed on Arthur Raymond and Frank Collbohm of the Douglas Aircraft Company to assess the problem from the standpoint of the capabilities, limitations and possible modifications to existing aircraft. Including Edward Wells, the chief engineer at Boeing, attention turned toward the reassessment of the B-29. By October the group had concluded that it was imperative that advantage be taken of Japanese defensive weaknesses by refitting B-29s to fly as high, fast and as heavily

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[134] It is not clear how or whether he compared civilians to soldiers.
[135] Memorandum from W. B. Shockley to E. L. Bowles, "Discussion of a Proposed Program on the Quantitative Aspects of Modern Warfare," 2/15/1945, *ELB*, Box 31, Folder 1. Shockley continued to work on issues of training, however. See, for instance, memorandum from W. B. Shockley to Edward L. Bowles, 6/28/1945, *ELB*, Box 31, Folder 2.

loaded with bombs as possible. They felt the best way to do so was to pursue a suggestion then in circulation to strip the aircraft down and remove the bulk of defensive armament, which would allow it to escape almost all Japanese fighter attacks—only tail guns would be required—and to equip the plane with the most advanced navigational radar. By the war's end, this suggestion had been implemented in one bombardment wing with a great improvement in accuracy.[136]

Also in the summer of 1944, the Army Air Forces contracted directly with the NDRC to perform a study of "the most effective tactical application of the B-29 airplane." Warren Weaver and AMP were given a central, coordinating role in the project. AMP took a decidedly different approach to its study (generally referred to by its contract number, AC-92) than Bowles did in his, preferring not to single out one aspect of the B-29 problem for examination on the basis of expert opinion. Rather, the panel took the wide definition of the problem as a *carte blanche* to demonstrate the power of mathematical warfare analysis by pushing it as far as the data available from component analyses and extant mathematical techniques could take them in pursuit of an answer to the problem as stated. AC-92 was by any measure an ambitious research program, but we must recall in evaluating AMP's attitude that part of the power and allure of Cunningham's combat theory was that it helped reveal what aspects of a broad combat problem had the most bearing on probabilities of success. In light of this fact, it made no sense to Weaver and the AMP staff to choose which problems needed the most scrutiny

[136] See Hazeltine, "Review of Activities," pp. 115-119 on the Special Bombardment Project, quote on p. 116.

without first studying how the various "micro-problems" bore on the problem as a whole.[137]

Unfortunately for AMP, there was very little established theory or empirical work to guide their analysis. Whereas OR relied on generally clear doctrines and practices to decide what problems needed to be analyzed, the B-29 problem was almost wholly unknown territory. As Weaver pointed out in "Comments," the Army Air Forces Board had itself observed that "no scientifically controlled or scientifically evaluated investigation of various defensive formations of bomber aircraft had ever been conducted," that "no scientifically controlled or evaluated investigation had ever been made of the B-29 defensive armament," and that "no standard central fire control gunnery doctrine had ever been developed, no standard manual of B-29 gunnery developed, nor any detailed training standards developed for B-29 gunnery."[138] Effectively produced on the same principles as the MIT Rad Lab's "crash" program, no orderly process of aircraft design and development had been followed, resulting in the theoretical vacuum concerning its use.

To address the problem adequately, AMP felt that an enormous set of studies had to be established, including a large number of experimental tests which were done at the University of New Mexico in Albuquerque and at the Army Air Forces testing range near Alamogordo, New Mexico in collaboration with the Second Air Force. A further set of gunnery model experiments were done at the Mount Wilson Solar Observatory in Pasadena, California in which special lights were affixed to model bombers in place of

[137] The best concise history of the study is Weaver, "Comments," pp. 37-53. Hazeltine, "Review of Activities," pp. 107-109 offers a brief look at the project from outside of AMP. Owens, "Mathematicians at War," pp. 292-293 offers a very brief overview.
[138] Weaver, "Comments," p. 37.

guns, and the resulting light patterns were measured by photocells to determine the firing characteristics of bomber formations with various armament configurations. In addition, the panel felt it was necessary to conduct tests on the central fire control system of the bomber, gun camera tests at Eglin Field, collaborative studies with General Electric, psychological studies on gunners with the NDRC's Applied Psychology Panel, analytical studies of special problems such as aircraft vulnerability and flak, and, most importantly, "broad analytical studies, such as those which relate to bombing effectiveness, general 'economic' theory of bombing, etc" to be done under contract with Princeton University.[139]

The Army Air Forces were not of the same mind as AMP, and only took a real interest in the aspects of the project being worked on in New Mexico, even though Weaver had "steadily and stubbornly" insisted that all aspects of the problem needed to be worked out simultaneously in order to interrelate test designs properly and to understand the significance of results achieved. Eventually, the AAF came around to support the Pasadena model tests as well, but were colder toward the others, and, according to Weaver, were "totally (and in most cases emphatically) uninterested" in the Princeton studies. So, AC-92 came to an end as soon as the work in New Mexico and at Pasadena had produced their major conclusions. Weaver could not hide his consternation that the Princeton study, "which should appear as the crowning jewel in this chapter on general studies in air warfare, was a total flop. It was a total flop," he was bitterly convinced, "because it was never given a Chinaman's chance of being anything else." He allowed that some members of the Twentieth Air Force in the Pacific had been, in principle interested, but that "they considered that such studies should be carried out

[139] *Ibid.*, p. 40.

within their own organization, say by their own Operations Analysis personnel." Most, he felt, had never really meant it when giving the NDRC such a broad directive.[140] Odds are, they did not understand how a man like Weaver would interpret a demand to find the "most effective tactical application".

Even still, a massive amount of AMP, NDRC and military effort had gone into the AC-92 project. Weaver estimated that some 350 military and civilian personnel had been involved, and it dominated his own schedule for much of the latter half of 1944.[141] Despite the effort put into the project, though, all of the studies were done under the pressure of looming deadlines and with limited data availability. Weaver and his mathematicians were always torn between the need to at least improve on wholly arbitrary decision making methods, and the concern that the inadequacies of their own methods would prevent their own synthesized conclusions from attaining any real improvement over arbitrary choices. This point should not be taken to mean that AC-92 did not yield results that made decisions more informed, but the ability of the project to produce important *definitive* conclusions was severely hampered.[142]

For instance, a study of the bombing accuracy of the B-29 was undertaken by an AMP group that included John Williams, and the analysts Andy Clark and Leonard (Jimmie) Savage. They produced their report for the project steering committee three days before the November 1st, 1944 deadline that had been set by the group. Williams warned Weaver in a cover letter that he and his team had been working on the report until 7 a.m. and: "We have some fears that we may have exceeded our directive. If so, we apologize and confess that the urge to do so was too strong to be resisted in our weakened

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[140] *Ibid.*, pp. 41-42.
[141] *Ibid.*, p. 46.
[142] Weaver listed numerous important results achieved under the AC-92 contract, see *ibid.*, pp. 48-49, 52-53.

condition. The data from Muroc never arrived, so there was nothing to impede us." In the main text, he reported, "We have been asked to estimate the effectiveness of the B-29 at very high altitude, say 35,000 feet, compared to its effectiveness at a more moderate altitude, say 25,000 feet." He was forced to admit,

> The assignment embarrasses us: Little time is available in which to contemplate the subject: few data exist; not all of these are at hand; some which are, are [sic] of questionable pertinency [sic]; and the theory is fragmentary. Hence, while numbers will sometimes be written, the reader should not mistake the discussion for a completely quantitative one; it becomes essentially qualitative before we finish.

Going through a long list of potential factors bearing on the success of operations, Williams and his team concluded that, in the end, there was no overwhelming reason to suppose that a stripped, "stuffed," high-flying B-29 would be any more valuable than a conventional B-29, and so the report recommended that the program continue as planned until some evidence arose to the contrary. In light of the notion that it "is dangerous in warfare to reject any weapon without a trial" they decided it would be worthwhile to outfit a group or a wing with revised armament after some battle experience had been obtained.[143] Whether or not the rushed report had any bearing on the matter is questionable, but, as we have seen, the approach Williams and his team suggested was exactly the approach the AAF took.

Even though AC-92 fell far short of what Weaver wanted to do with it, he nevertheless believed that he had caught a glimpse of the future of an intertwined process of technological and tactical design. While the project itself had been a disappointment, to him it proved the principle that "in a reasonable length of time, it is possible to obtain the kinds of experimental data necessary for a more general, more inclusive, and more

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[143] "Discussion of Bombing Effectiveness at High and at Very High Altitudes," SRG-P No. 70, 10/28/1944, *NACP*, RG 227, [NC-138/152], Box 19, "Study No. 128 – Papers by J. D. Williams" folder. Other papers were included as well, so it is possible Williams' cover note, dated 10/28/1944, was not referring to exactly this study. The study it was clipped directly to was dated 10/26/1944.

penetrating study of the employment of large bombers." Weaver wanted a chance to prove his convictions. In the wake of the contract's closure, Edward Bowles made his request to Weaver about how to institute a gunnery research unit at Eglin Field to continue and complement the work begun under the NDRC contract in New Mexico. Weaver, in reply, produced his memorandum on the role of warfare analysis in the activities of a gunnery research unit. This report became an appendix to "Comments on a General Theory of Air Warfare". As with so many other such glimpses of the future, Weaver's thinking culminated in the expression of a utopian vision: the Tactical-Strategic Computer. To Weaver, this device represented the power of analytical thinking combined with investigation to make more robust decisions than could have been made had they been made arbitrarily (as they had too frequently been), and he and other members of AMP—not to mention their colleagues in OR—were eager to capitalize on this power in the postwar period. "Comments on a General Theory of Air Warfare" was his manifesto attempting to lay out the basic principles, the -1^{th} approximation, of a path forward that his exhausted mind could at that point barely discern.

Conclusion

Many of the individuals who had been involved in the technical war effort felt the Allies were lucky to have been able to muster the power to counter initial setbacks, and were confident that British and American efforts had discovered certain virtues that allowed them to overcome the aggressiveness of the German and Japanese war machines. On April 20, 1945, in a shattered Germany, John Trump, the head of the British Branch of the Radiation Laboratory, interviewed two of the heads of the German company

Telefunken's radar development program. He recorded in his diary that "this interview proved to be quite thrilling because we had long been deeply interested in the organization and thinking of the German radar people." In addition to the historical details of the development effort, he found out that industrialists did not work closely with physicists in university and private laboratories; that the industrial laboratories had received their orders from technical personnel at the Luftwaffe; but that, just "as there was a gap between German scientists and industrialists, there was also an even greater gap between both of these and the military. It was virtually impossible for a scientist or engineer to accompany radar equipment into combat areas to observe its performance or to assist in training." Trump also recorded his doubts that the Allies would learn much from the Germans beyond the occasional technical detail.[144]

There should be no mistake: France, Germany, and Japan all had technologically sophisticated militaries, and all had suffered devastating defeat despite the fact that they were facing only slightly more sophisticated adversaries. What impressed Trump and so many other members of the technical effort was how important intangible factors such as coordination of effort and intelligent choice of research priorities were to maintaining the crucial edge that prevented disaster. The fateful success of the Manhattan Project only served to grant the point a greater sense of urgency. With the lessons of the war still fresh, many of the wartime changes in the performance of research and development, in the assembly of war plans, in the development of training regimens, and in the hierarchical structure of the military command itself were perpetuated in the postwar era. However, good management and coordination of effort are not easily defined phenomena.

[144] Trump, "War Diary," entry for 4/20/1945. For a parallel story, see the end of chapter 4 in Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

Instead "operations research" came to be an icon representing more general developments.

So it is commonplace for even careful historians to conflate OR with any application of science to problems situated along the technology-technique divide: with the mathematics of fire control, or with the British system of scientific advisors. Others who are more reckless have even taken OR to be representative of a moment when warfare somehow became scientific. I do not want to diminish the importance of OR too much, but this chapter should have demonstrated that it is dangerous to conflate it with more general trends. It was the *tandem relationship* between OR and sophisticated methods of design, mathematical analysis of combat, and scientific advice that rendered each new level of sophistication a demonstrably useful and, therefore, legitimate extension of scientific activity. OR's close relationship to existing military practices, as examined in the first chapter, helped these *other* scientists to align their contributions with the requirements of the military, but we must also remember that OR was only a supplement to ordinary liaison channels, not a thing that caused some preexisting boundary between technology and technique to collapse.

The truth is that the most significant scientific and technological legacies of World War II in America and Britain are still not fully understood, because there is not yet a conceptual basis rich enough to describe what happened. Operations research was important, but so were other organizational innovations throughout the established branches of the military services and their regular contacts in industry, which were not as visible as the introduction of scientific advisors from the academic world, or the establishment of new institutions such as the Office of Scientific Research and

Development. For instance, the importance of the design techniques developed by the British civil servant L. B. C. Cunningham has never been recognized, but we will see in future chapters how enormously influential his theory of combat ended up being, even as his name slipped back into obscurity. It will not be until we are able to distinguish the obviously novel from the more hidden changes in the broad picture of the way technologies were developed and used that we will be able to come to solid conclusions about what happened and what role OR played in it. As it turned out, operations research attained a special *rhetorical* resonance during the war for reasons we will discuss in chapter six. In the next chapter, however, we will ignore broad rhetoric about science and warfare, and will turn instead to the postwar developments in the military stemming directly from the events detailed in the first two chapters.

CHAPTER THREE

The Conceptual War: OR, Systems Analysis, and the Postwar Military

Operations research had first arisen as an *ad hoc* supplement to the operational planning, tactical development, and technology design efforts of the British and American militaries, their research facilities, and their contractors. Its rise had been accompanied by a series of related developments, such as the proliferation of intermediary technologies, new tactical development and training units, improvements in military record keeping and intelligence analysis capabilities, and other sundry augmentations in the military's ability to appreciate and respond to the shifting demands of a full scale modern war. At the end of the war, despite the fact that there was still no clear sense of what exactly OR was, *whatever* it was, there were few within the scientific or military communities who dismissed its value, or who resisted its continuation into the postwar era, even though peace meant there would be no more combat operations to investigate.

This last issue might have represented an existential threat to OR, since what had largely made it so valuable during the war was the intellectual link it had created between the most pressing exigencies of actual combat and the development of tactics, training regimens, and technology designs. And in fact, with the closure of the Office of Scientific Research and Development and its Office of Field Services at the end of the war, the last vestiges of OR as the name for the link between research and development (R&D) laboratories and the field perished. While OR would continue to concentrate on studies of technologies in use, no OR group would be *specifically* tied to the deployment of new laboratory technologies again. Nevertheless, there was no reason to suppose that research on the expected effectiveness of plans and doctrines would not prove a valuable supplement to postwar planning activities. The real question was how OR should be regularized, given the fact that peacetime R&D, testing, training, and planning activities were already rather well integrated military specialties.

The clearest response to this question was to emphasize the status of operations research as a specifically civilian research activity that was independent from military or laboratory ties. In America the civilian aspect of OR came quite strongly to the fore as the administration of many OR groups was contracted out to universities and, increasingly, non-profit corporations in order to recruit personnel who would not be attracted to civil service pay scales, and to ensure their intellectual independence.[1] A second response was to reemphasize the nature of OR studies as almost scholastic investigations that synthesized the work of the military's specialists into a systematic overview of the conceptual viability of plans and doctrines in various potential combat scenarios. However, even as OR found ways to distinguish itself from military planning, it blurred into other new kinds of civilian research organizations that were being set up to offer the military independent advice, not on operational planning, but on R&D, design, and procurement policies.

These R&D-oriented analysis organizations were totally new—they did not have direct intellectual ties to wartime OR—and they were endemic to America. Air Force Project RAND (which became the nonprofit RAND Corporation in 1948) was largely the

[1] The United States Air Force, like the British services, made heavy use of the civil service system to staff its in-house operations analysis work, but the R&D research organization, the RAND Corporation, set a strong precedent for the use of independent research organizations that would be copied many times.

brainchild of Edward Bowles, the Expert Consultant to the Secretary of War and General Henry Arnold's scientific advisor on communications. Bowles pushed for RAND because he wanted to create new institutions where equipment designers and military planners could coordinate their activities.[2] A second organization was the Weapons Systems Evaluation Group (WSEG), a creation of the new Department of Defense's Research and Development Board (RDB) as part of a vain attempt to bring some order to budget allocation among the services. WSEG was eventually supplemented by the creation of the Institute for Defense Analyses (IDA), which swiftly diversified its activities by sponsoring a variety of new civilian research groups. As civilian research organizations with tight connections to the military OR groups; RAND, WSEG, and IDA were often included among the numbers of the swiftly expanding military OR constellation, even though they did not perform OR in the traditional sense.

This chapter shows how all of these civilian research organizations integrated their work with military planning activities. It begins with a review of the institutional organization of the major British and American military OR groups, and continues with a discussion of the origins of the R&D policy analysis groups, and an analysis of their relationship to military OR. It then discusses the intellectual and institutional strategies employed by military OR personnel to make their work relevant to the concerns of military planners. Finally, the last half of the chapter will be dedicated to a reexamination of the intellectual basis of the new techniques of systems analysis at the RAND Corporation. RAND's initial formulation of systems analysis has been examined several times before, but always as an ambitious attempt to create a "science of warfare", and never as a continuation of the design methods of L. B. C. Cunningham and AMP's

[2] RAND was an acronym for research and development

studies of the tactical uses of the B-29 bomber. In the 1950s, systems analysis and OR would become closely related to each other to the point where systems analysis came to be seen as a progeny of OR. However, I argue, this intellectual conflation only made sense following radical transformations in the nature of both activities and a substantial reconsideration of the way they intersected with each other and with the policymaking process. Here we will only consider them as independently evolving concepts, leaving discussion of their merger for chapters four and five.

An Institutional History of Postwar OR

Following the war, all of the British and American services' OR groups decreased substantially in size as analysts, like so many of the military's soldiers, returned to their prior lives, including most of the wartime leadership in OR. However, all of the services' main OR organizations survived the transition to peace, and all grew continuously through the postwar era. Before examining how the *concept* of OR transferred to a peacetime military, it will first be useful to review briefly the postwar growth and organization of OR in the British and American militaries, summarized in Table 3.1. The term OR was actually used idiosyncratically. In the RAF, OR was split to include administrative, personnel and training research, and many of these duties were retained in the commands in what simply came to be called "research branches".[3] The United States Navy's Operations Research Group was renamed the Operations Evaluation Group

[3] See, for instance, "Responsibilities of the Scientific Adviser to the Air Ministry" 8/3/1945, TNA: PRO AIR 30/3146. Training and Personnel research were ultimately included as subsets of Administrative Research, which was paired with Operational Research as being under the scientific adviser's jurisdiction. Administrative Research entailed especially problems of manpower allocation, which had been done during the war in certain OR projects, notably in work on Coastal Command's Planned Flying and Planned Maintenance scheme.

Table 3.1: Military OR Groups in the US and Great Britain, 1945-1962	
United States	
Air Force	• Operations Analysis Division (later called Office of the Assistant for Operations Analysis) in USAF Headquarters • Operations Analysis Offices in local commands, notably at Strategic Air Command • Project RAND (1946-1948), RAND Corporation (1948 onward), not precisely an OR group, but frequently named alongside them
Navy	• Operations Evaluation Group (OEG, administered by MIT, 1946-1962) • Naval Warfare Analysis Group (NAVWAG, established in 1956 and also administered by MIT) • OEG Applied Science Division (established in 1960) • OEG, NAVWAG, and OEG ASD absorbed into the Center for Naval Analyses (CNA) in 1962 (administered by the Franklin Institute)
Army	• Operations Research Office (ORO, administered by Johns Hopkins University, 1948-1961) • Various local groups, notably the Combat Operations Research Group at Continental Army Command • ORO was dissolved and reconstituted as Research Analysis Corporation in 1961
Defense	• Weapons Systems Evaluation Group (WSEG) was founded in 1949, and supplemented by the Institute for Defense Analyses beginning in 1956; neither was exactly an OR group, but WSEG was often referred to as one
Great Britain	
RAF	• Research Branches in commands practicing "operational" and "administrative" research, under the Scientific Adviser to the Air Ministry (SAAM)
Royal Navy	• Department of Operational Research (DOR)
Army	• Operational Research Group (Weapons & Equipment) in the Ministry of Supply (1945-1948) • Military Operational Research Unit (MORU) in the War Office (1945-1948) • ORG (W&E) merged into MORU in 1948; the combined organization was renamed the Army Operational Research Group, and renamed the Army Operational Research Establishment in 1962 • All were under the Scientific Adviser to the Army Council (SA/AC) until that position was abolished in 1959

(OEG). When the United States Air Force became independent of the army in 1947 it continued to use the term operations analysis. Nevertheless, terminology continued to shift with some frequency in the military where titles were considered important for delimiting organizational functions. The British Admiralty retained the term operational research into the mid-1960s, but after that OR became operational analysis and then operational studies. Even though names differed, all service OR groups generally identified themselves as such. A 1980 internal history of OR in the British Admiralty was entitled "A Rose by Any Other Name" to convey just this point.[4]

In Britain in the immediate postwar period, as the nation's recognized scientific elite left the services and supply ministries, many of the highest research policy, scientific advisory and OR positions created during the war were retained as arms of the British government's scientific civil service, and personnel frequently transferred between R&D and OR duties.[5] Aside from some shifts in names and organization, however, OR's institutional arrangements remained more or less untouched. At the Admiralty, OR underwent only minor bureaucratic changes.[6] In the RAF, the research branches in the

[4] T. H. Pratt, "A Rose by Any Other Name: An outline of Operational Analysis in Admiralty Headquarters 1947-1970," March 1980, TNA: PRO ADM 219/731; printed in the *Journal of Naval Studies* 7 (1981) 1-9, 104-113, 161-169, 218-227.

[5] There were several changes in the organizational structure of R&D after the war. The Ministry of Aircraft Production was absorbed into the Ministry of Supply. When army production returned to the War Office in 1959, the Ministry of Supply became the Ministry of Aviation, which was rolled into the Ministry of Technology in 1967. The Admiralty continued to retain control over its R&D and supply matters in the decades following the war. This history is reviewed in David Edgerton, *Warfare State, Britain 1920-1970* (Cambridge, UK: Cambridge University Press, 2006), pp. 244-246.

[6] The Naval Operational Research Directorate (NORD) became the Department of Operational Research (DOR), and received its staff from the Royal Naval Scientific Service (RNSS). After World War II, all Admiralty scientific work was coordinated through the RNSS, but from the end of the war until mid-1960s, DOR reported directly to high naval officers. DOR was under the Deputy Chief of Naval Staff until that post was abolished, at which time it came under the Vice Chief of Naval Staff. In the mid-1960s, Admiralty OR came directly under the authority of the RNSS' Chief Scientist and the Deputy Controller

commands continued to be closely linked to the Scientific Adviser to the Air Ministry (SAAM).[7] In 1948 the War Office's and the Ministry of Supply's OR groups consolidated into a single Army Operational Research Group (AORG), which reported to the War Office's Scientific Adviser to the Army Council (SA/AC).[8]

Although OR is frequently associated with developments in high science policy in Britain (for reasons to be discussed in chapter six), military OR's actual connection with policy requires some clarification. OR studies continued to play a marginal role in setting R&D and procurement policy in the individual services by informing deliberations via the Army and RAF scientific advisory posts as well as the Admiralty's Deputy Controller for R&D. Britain's *overall* military R&D policy remained split between the services until the Ministry of Defence eventually asserted itself politically

(R&D), but it retained strong links to operational concerns; see Pratt, "A Rose by Any Other Name," p. 1 (marked incorrectly as p. 7).

7 SAAM remained within the Air Ministry's chain-of-command; the post was independent of the Ministry of Supply and the later Ministry of Aviation.

8 Between the end of the war and 1948, the War Office's OR group was called the Military Operational Research Unit (MORU), while the Ministry of Supply's group was called Operational Research Group (Weapons & Equipment). In 1948, the Ministry of Supply relinquished its group to the War Office; this new organization fell under the authority of SA/AC. In 1959, when control over army R&D and production also reverted to the War Office, the SA/AC position was abolished. In 1960 Army OR was placed under a Director of Army Operational Science and Research (DAOSR) who reported to the Army's Chief Scientist, but retained close contact with operational staff. DAOSR had an assigned military adviser, and AORG had military staff assigned. DAOSR also oversaw remote sections in field commands. In 1962, the AORG was renamed the Army Operational Research Establishment. See A. W. Ross, "An Introduction to the Army Operational Research Group," AORG Memorandum No. F.10, August 1955, TNA: PRO WO 291/1437; and G. Neville Gadsby, "The Army Operational Research Establishment," *Operational Research Quarterly* **16** (1965): 5-18. Notably, early holders of the SA/AC post were important figures in the professional establishment of OR beyond the military in Britain. The second SA/AC, Sir Charles Ellis, went on to establish one of the first industrial OR groups at the National Coal Board. See Rolfe C. Tomlinson, *OR Comes of Age: A Review of the Work of the Operational Research Branch of the National Coal Board* (London: Tavistock Publications, 1971); and Maurice W. Kirby, *Operational Research in War and Peace: The British Experience from the 1930s to 1970* (London: Imperial College Press, 2003), chapter 8. (Another industrial group, at the British Iron and Steel Research Association, was started by the wartime Deputy Controller (R&D) at the Admiralty, Sir Charles Goodeve.) The third SA/AC, Sir Owen Wansbrough-Jones, became the first president of the Operational Research Society in 1953. Wansbrough-Jones had been SA/AC from 1946 (when Charles Ellis left) to 1951. He was Chief Scientist at the powerful Ministry of Supply from 1953 until 1959, when army research, development, and supply reverted to the War Office. Before the establishment of the OR Society, Wansbrough-Jones had been an active member of the less formal Operational Research Club. See Donald Hicks and David Smith, "Sir Owen Haddon Wansbrough-Jones," *Journal of the Operational Research Society* **34** (1983): 105-109.

two decades after the end of the war. Thus, whatever coordination that did occur was effected on a voluntary basis. From 1947 to 1963 the Ministry of Defence sponsored a Defence Research Policy Committee (DRPC) to aid this coordination.[9] High level officers, scientific advisers, R&D budget controllers, and chief scientists were all represented on the DRPC, so SAAM and SA/AC would have represented the OR viewpoint for the RAF and the Army, respectively.[10] The Admiralty was represented scientifically by the Chief of the Royal Naval Scientific Service and by the astronomer John (later Sir John) Carroll, who was Deputy Controller of R&D as well as Scientific Adviser to the Board of the Admiralty for the lifespan of the DRPC. It is, unfortunately, unclear what relationship Carroll had with the Admiralty's Department of Operational Research (DOR), but the Deputy Chief of Naval Staff, to whom the DOR reported, was also a member.[11]

In 1964 the topography of British defense science changed significantly in Britain. Ironically, the creation of the Ministry of Technology (Mintech), and the appointment of Patrick Blackett, Britain's patron saint of OR, as the highest advisor at Mintech has no

[9] The DRPC was an elite organization where services could coordinate or otherwise debate their research priorities. It was chaired by a scientific advisor to the Ministry of Defence who did not, otherwise, have much power. Chronologically, this post was occupied by Sir Henry Tizard from 1947 to 1952 (who spearheaded the development of military OR during the war), Sir John Cockroft from 1952 to 1954 (a member of the scientific elite with close relationships to important wartime OR supporters), Sir Frederick Brundrett from 1954 to 1959 (who came up through the Admiralty's scientific civil service and made speeches praising the development of OR and scientific advisory positions; see chapter six), and Sir Solly Zuckerman from 1960 to 1963 (who is often associated with OR). See Jon Agar and Brian Balmer, "British scientists and the Cold War: The Defence Research Policy Committee and information networks, 1947-1963," *Historical Studies in the Physical and Biological Sciences* **28** (1998): 209-252.

[10] After 1960, only the Army's Chief Scientist, Walter Cawood, attended DRPC meetings and would have represented both the OR as well as the R&D perspective of the Army.

[11] The first Deputy Controller (R&D) was Charles Goodeve, who, we will recall, became Britain's top postwar OR booster. The second Deputy Controller (R&D) was A. P. Rowe, who, while Chief Superintendent of the Telecommunications Research Establishment is said to have coined the term operational research. It stands to reason that Carroll also would have had a close relationship with DOR. I have not uncovered much information relating to this point, but see letter from J. A. Carroll to Sir Henry Tizard, 12/20/1946, TNA: PRO DEFE 9/6.

relationship to the history of OR. Not only did Mintech have little to do with military operations, or even for its first few years most military R&D, by 1964 Blackett himself had become completely divorced from the British OR community.[12] However, the integration of the three service ministries into the Ministry of Defence earlier that year brought about a total rearrangement of administrative and advisory mechanisms in the services, which included the creation of a united Defence Operational Analysis Establishment.[13] This move was orchestrated by Sir Solly Zuckerman, who was the last chair of the DRPC and, subsequently, the first Chief Scientific Adviser to the British Government.[14] It is still unclear how the military's OR work bore on higher policymaking decisions after the demise of the DRPC and the rise of Zuckerman, but we must relegate any detailed discussion of developments at that late date to beyond the pale of this dissertation.

In the United States Navy, the wartime ORG had, administratively, been a part of the OSRD, first as a contract organization run out of Columbia University, and later as a part of the OSRD's own Office of Field Service, which worked almost exclusively (and

[12] Blackett compared the Ministry of Technology job to his wartime experience and advocated setting up "appraisal groups," see Edgerton, *Warfare State*, p. 247. However, Blackett was far removed from professional OR by this time. Nevertheless, the link between Blackett's job at Mintech and OR is presented in Lovell, p. 80; and reiterated in Nye, p. 160. In his history of OR, Kirby devotes considerable space to the Ministry of Technology for unclear reasons, see Kirby, *Operational Research*, pp. 320-329; this material is based on Kirby's Blackett Memorial Lecture to the Operational Research Society, printed as M. W. Kirby, "Blackett in the 'white heat' of the scientific revolution: industrial modernisation under the Labour governments, 1964-1970" *Journal of the Operational Research Society* **50** (1999): 985-993. Kirby argues that Blackett could not have the influence in peace that he had during the war. Blackett's wartime influence is surely vastly overstated. On the nature of Blackett's wartime influence, see Malcolm Llewellyn-Jones, "A Clash of Cultures: The Case for Large Convoys," in *Patrick Blackett: Sailor, Scientist, Socialist*, edited by Peter Hore (Portland, OR: Frank Cass, 2003), pp. 138-158. Given the significance OR had achieved as its own profession by 1964, it is also important that we cease associating Blackett's time at Mintech with OR.

[13] The service departments appear to have retained their own distinct OR groups as well. Kirby, *Operational Research*, discusses the DOAE briefly, pp. 346, 348.

[14] Solly Zuckerman, *Monkeys, Men and Missiles: An Autobiography, 1946-1988* (London: Collins, 1988), pp. 362-363.

highly unusually) within the military chain of command. After the war, the renamed Operations Evaluation Group was administered by contract with the Massachusetts Institute of Technology. In 1955, a small Naval Warfare Analysis Group (NAVWAG) was created as an adjunct to OEG, and was dedicated to studying long term strategic issues for the Navy's own Long Range Objectives Group, and in 1960 an Applied Science Division (ASD) of the OEG was also established to study R&D-type problems. In 1962 all three groups were absorbed into the new Center for Naval Analyses (CNA).[15] Meanwhile, at the end of the war, the various operations analysis section of the Army Air Forces were closed down and replaced with a central Operations Analysis Division (OAD) at the Army Air Forces' headquarters in Washington under the Deputy Chief of Staff for Operations. This division was retained when the Air Force (USAF) attained independence from the army in 1947.[16] While during the war this headquarters staff had been purely administrative and had left research programs up to sections in the commands, it was now responsible for directing its own research. The headquarters office was soon supplemented by numerous Operations Analysis Offices housed at the various commands, which had only loose connections to the headquarters division. Most only employed a couple of analysts, but the office at Curtis LeMay's Strategic Air Command (SAC) in Offutt, Nebraska actually employed more analysts than the

[15] On the Navy end, the contract was administered by the new Office of Naval Research, but on a day-to-day basis the OEG worked for the Office of the Chief of Naval Operations at the Pentagon, which had no budget of its own for contracts. Keith Tidman, *The Operations Evaluation Group: A History of Naval Operations Analysis* (Annapolis: Naval Institute Press, 1984) is a detailed and useful history of OR in the Navy. Also see Joseph H. Engel, "Operations Research for the U. S. Navy Since World War II," *Operations Research* **8** (1960): 798-809.

[16] It was later called the Office of the Assistant for Operations Analysis, although, based on archival records, usage of either term seems to have been permissible for a time.

Washington group.[17] As of the end of the war, the Army had no substantial OR group, but, as we will see, this would soon change.

New Developments in America: RAND, WSEG, and IDA

In America, as OR came to be defined around the idea of civilian scientific analysis, there was one military contractor that would come to play a key role in the development of American OR, even though it was not actually an OR group in the traditional sense of the term. In the fifteen years following World War II, the RAND Corporation would eclipse all of the military OR organizations in notoriety, and would come to be seen as a logical extension of wartime OR, but it was founded around problems that usually remained far removed from military OR groups: R&D policy. While the most visible aspects of the postwar science policy debate in America centered around the organization of contracting and funding mechanisms and the role of the university, other schemes began to proliferate as to how the military would organize its own R&D activities and contracts. The Navy responded by establishing its Office of Naval Research, and for the first five years after the war, the ONR was *the* pre-eminent sponsor of military *and* civil research in America.[18] The Air Force expanded its own

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[17] In 1948 the SAC office had 15 to headquarters' 10; in 1950 headquarters had gained two more. There is no comprehensive history of operations analysis in the United States Air Force, but see I. B. Holley, Jr., "The Evolution of Operations Research and Its Impact on the Military Establishment; The Air Force Experience," in *Science, Technology and Warfare, Proceedings of the Third Military History Symposium, USAF Academy* (Office of Air Force History, 1969); and LeRoy Brothers, "The Office of the Assistant for Operations Analysis, US Air Force," *The Second Tripartite Conference on Army Operations Research, Volume II: Technical Proceedings, 23-27 October 1950, NACP*, RG 319, General Decimal File {Security Classified Correspondence, 1950-55} [A1/137A], 1950-51: Box 40, (020, ORO). Some additional information can be found in Charles R. Shrader, *History of Operations Research in the United States Army, Volume I: 1942-1962* (Washington, DC: U.S. Government Printing Office, 2006). A frustratingly unrevealing account of the SAC Operations Analysis Office can be found in Carroll L. Zimmerman, *Insider at SAC: Operations Analysis Under General LeMay* (Manhattan: Sunflower University Press, 1988).

[18] See S. S. Schweber, "The Mutual Embrace of Science and the Military: ONR and the Growth of Physics in the United States After World War II," in *Science, Technology, and the Military*, edited by Everett

R&D programs under a new Air Materiel Command, and set up a Scientific Advisory Group under the aerodynamicist Theodore von Kármán,[19] but the Expert Consultant to the Secretary of War, Edward Bowles, felt bolder institutional steps needed to be taken.

Unlike many of his university colleagues, Bowles had decided it was in his and the country's best interests to remain in government after the end of the war. Ultimately this move did not turn out well for him—he was dismissed following the unification of the services in 1947 shortly after having cut his ties with MIT.[20] Before that happened, however, he did make at least two major accomplishments. The first was the establishment of a Deputy Chief of Staff for R&D post in the Army Air Forces, which was filled by Curtis LeMay. This victory was fleeting. The post was dissolved and replaced with one at the echelon below it when the Air Force attained independence from the Army.[21] Meanwhile, though, Bowles was also keen to create strong venues where military planners and industrial contractors could develop a close working relationship in

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Mendelsohn, Merrett Rowe Smith, and Peter Weingart, (Boston: Klewer Academic, 1988), pp. 1-45. Harvey M. Sapolsky, *Science and the Navy: The History of the Office of Naval Research* (Princeton: Princeton University Press, 1990).

[19] A more extensive account of the organization of science in the Air Force can be found in Nick A. Komons, *Science and the Air Force*, (Arlington: Office of Aerospace Research, 1966).

[20] Once Henry Stimson and General Henry Arnold, his primary patrons, retired after the end of the war, and the War Department itself closed down in 1947, he found himself isolated, and ultimately dismissed by the final caretaker Secretary of War, Kenneth Royall, in the transition to the new National Military Establishment, which became the Department of Defense in 1949. Bowles called this treatment the "Royall brush-off"; see autobiographical note later attached to a letter from Dwight Eisenhower to Bowles, 8/27/1947, Bowles Papers, Box 35 Folder 5. Out of a job, Bowles became a full-time consultant, primarily to Raytheon and was also accepted back at MIT as a "consulting professor".

[21] On the position, see Martin Collins, *Cold War Laboratory: RAND, the Air Force, and the American State, 1945-1950* (Washington, DC: Smithsonian Institution Press, 2002), chapter 2. Bowles also felt that LeMay would require a civilian research director to help him evaluate technical data and research proposals. Canvassing his scientific contacts for recommendations for a civilian director, Bowles emphasized how important it would be to find someone who could work easily with academics, industrial research scientists, industrial managers and the military, "someone who [had] had a good deal of experience working with the military, preferably the Air Forces, and who had demonstrated his capabilities as a diplomat and one who comprehends the significance of basic science and technology." He thought Philip Morse might work well, given his Navy experience and his administrative skill. In the end, though, neither Morse nor any other candidate would take the post before the DCS/R&D position was abolished. See Collins, *Cold War Laboratory*; on the inability to fill the civilian assistant post, see p. 38n28. On the civilian assistant, see letters from Bowles to Karl Compton, 12/20/1945, and 1/2/1946, *MIT*, AC 4, Box 34, Folders 6 and 7; quote from the 1/2/1946 letter.

defining future R&D projects and in performing studies of the "economics of warfare" to devise the payoffs for various R&D decisions.[22] With the studies he commissioned on the uses of the B-29 bomber done by William Shockley and the representatives of Douglas Aircraft and Boeing still fresh in his mind, on October 1st, 1945, he and General Henry Arnold met with Frank Collbohm, Arthur Raymond, and Donald Douglas of Douglas Aircraft to discuss the development of a new contract organization that would lay the theoretical groundwork for the development of future technical and tactical developments for missiles.[23]

Early the next year their plans came to fruition with the establishment of Air Force Project RAND in Santa Monica, California, under Douglas Aircraft's management and Collbohm as its director, and with an expanded mandate to study all means of non-land-based intercontinental warfare.[24] Although at first very pleased with RAND, Bowles soon began to sour on it. The open-endedness of RAND's contract allowed it to expand beyond the point he felt was amenable to practical work, and he feared that RAND would lose its focus on specific R&D problems. In his mind, RAND should have been followed by the establishment of other RAND-type organizations under other contract organizations.[25] Bowles' fears were further stoked by the 1948 decision to sever RAND from Douglas Aircraft and to form the independent non-profit RAND

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[22] Interview with Bowles, 7/15/1987, pp. 70-71; 8/20/1987, pp. 92-93, *NASM*, RAND Oral History Project.
[23] See especially Collins, *Cold War Laboratory*, pp. 40-54.
[24] RAND's oft-quoted contract stated: "The Contractor will perform a program of study and research on the broad subject of intercontinental warfare, other than surface, with the object of recommending to the Army Air Forces preferred techniques and instrumentalities for this purpose." See, for instance, Bruce L. R. Smith, *The RAND Corporation: Case Study of a Nonprofit Advisory Corporation* (Cambridge, Mass.: Harvard University Press, 1966), p. 30.
[25] For instance, to study the question of air defense, he urged that an organization similar to RAND be set up at MIT, but the problem was given to the extant RAND. See letter from Bowles to James Killian and attached memorandum to Major-General F. L. Ankenbrandt, 4/26/1948, *MIT*, AC 4, Box 34, Folder 7. Not long thereafter, though, MIT established the Lincoln Laboratories, which developed the SAGE radar defense network.

Corporation, but by this time Bowles had lost his position, and was helpless to influence the matter further.[26]

From its establishment, it would take RAND several years before it began to establish a unique identity as an R&D analysis organization. We will examine the unique methodology developed there later in this chapter. Even in its earliest years, though, as an independent civilian contract organization dedicated exclusively to theoretical analysis, RAND appeared in some circles, notably the new Department of Defense's Research and Development Board (RDB), to be similar in form and function to the Navy's OEG.[27] The RDB had been set up in 1947 under the chairmanship of Vannevar Bush to help coordinate defense R&D policy across the services, and had immediately become buried beneath the sheer weight of the 18,000 potential R&D problems to be evaluated.[28] To sort through this jungle, the board employed over 2,000 technical consultants to no avail.[29] The issue was only complicated further by the fact that the technologies had to be evaluated not only by some mysterious measure of intrinsic worth, but in terms of their relevance for future military planning.

This last problem was to be addressed by a group of part-time scientific advisors to the RDB's "policy council" who were charged with evaluating the RDB's allocation

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[26] This decision was taken primarily on account of Douglas' discomfort with its unique tie to Air Force policymaking. The main concern was that prejudice would direct development contracts away from Douglas to avert accusations of prejudice in favor of Douglas. On this history, see especially Collins, *Cold War Laboratory*, chapter 3.

[27] At one point, the temporary name given to the RDB's new evaluation group was the Operations Evaluation Group; see memorandum from Lt. Col. Black to Dr. O'Bryan, 4/27/1948, *NACP*, RG 330, Research and Development Board, Records Concerning Organization, Budget, and the Allocation of Research and Development, 1946-1953 [NM-12/341-A], Box 521, Folder 3. At this point the Department of Defense was still called the National Military Establishment.

[28] The RDB was a continuation and expansion of prior joint development committees with which Bush had been associated during the war.

[29] Shrader, *Operations Research*, p. 61, including note 70. On the RDB in general, see Allan A. Needell, *Science, Cold War, and the American State: Lloyd V. Berkner and the Balance of Professional Ideals* (Amsterdam: Harwood Academic, 2000), chapter 4.

effort.[30] The group was chaired by the physicist I. I. Rabi, who had been an assistant director of the Rad Lab during the war. The other advisors were physicist and Wall Street tycoon Alfred Loomis, the entomologist Caryl Haskins, and William Shockley, who had returned to Bell Laboratories after the war. At the group's fifth and final meeting on October 20th, 1947, at Loomis' New York City apartment, the advisors decided that even with access to individual services' analytical reports, including those being produced by OR groups and RAND, its own resources were too meager to approach its task in any intelligent way. They concluded that "a full-time Section be established within the RDB to evaluate the general balance of the national military research and development program." This section, "would be responsible for determining, through the application of operational analysis techniques, the relative emphasis to be placed on various research and development programs in the light of current strategy."[31]

The group developed their ideas further in a letter and memorandum, which they sent to Bush. The memorandum was prepared from Shockley's notes from the last meeting, and outlined the need to pursue what it called the "conceptual war" in a rigorous and quantitative fashion. "The conduct of a 'conceptual war'," the memorandum explained, "presents a complicated problem in cooperative enterprise. It cannot be carried out by any one, small, isolated group of people no matter how well they are supplied with reference material. Instead," it continued,

> hypothetical problems must be taken from time to time to experts in the services, in industry and in science – all of whom would, in time of war, have to deal with problems of the same nature. The synthesis of these individual problems into a meaningful, directed, military research and development effort can be achieved only by placing

[30] On the Policy Council and the recommendation to found WSEG, see Needell, *Berkner*, pp. 114-122.
[31] Minutes, "Fifth meeting of the Scientific Advisors to the Policy Council," 10/20/1947, *NACP*, RG 330, [NM-12/341-A], Box 521, Folder 3.

> responsibility and authority in the hands of a competent, full-time, central operational analysis group.

Although Shockley's experience with OR during the war influenced the language of the proposal, the role of the RAND Corporation in producing studies in line with anticipated future military requirements was clearly on the group's mind. There was, up to that point, no major precedent for the use of OR in evaluating R&D programs. The memorandum went on to recommend that a full ten percent of RDB personnel be devoted to long-range efforts; that it should work in close connection with "already existing service agencies working in similar, but more restricted fields," namely the Navy's OEG, the Air Force's OAD, and Project RAND; and it passed along the idea of establishing groups at the Army and the Joint Chiefs as well.[32]

The advisors' recommendations made their way swiftly into the military bureaucracy. On February 4th, 1948, following a discussion of the issue with Army Chief of Staff General Dwight Eisenhower, Secretary of Defense James Forrestal made a formal request that a means be established of obtaining "objective and competent" technical advice through the RDB.[33] Amid horse trading between Forrestal and the Joint Chiefs over the allocation of authority, however, the request stalled.[34] Meanwhile, the Army reacted much more quickly. Following the Navy's example of contracting with MIT, it began serious negotiations to contract with Johns Hopkins University for the administration of a "General Research Office" in May 1948. The office began work in

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[32] See Lt. Col. Edwin Black to I. I. Rabi, 11/26/1947, and attached letter and memorandum drafts; *NACP*, RG 330, [NM-12/341-A], Box 521, Folder 3.

[33] Memorandum from James Forrestal to the Joint Chiefs of Staff, 2/4/1948, *NACP*, RG 218, Central Decimal File, 1948-50 [UD/5], Box 129, "334 (WSEG) (2-4-48) Establishment of Weapons Systems Evaluation Group Sec. 1" folder.

[34] In the spring the RDB set up an *ad hoc* committee consisting of Loomis, Shockley, Purdue University president Frederick Hovde, with Lloyd Berkner as chair. See their final report, "Weapons Systems Evaluation: Report of the Ad Hoc Committee Appointed to Study the Problem of Weapons Systems Evaluation," 5/6/1948, *ELB*, Box 45, Folder 5; and Needell, *Berkner*, The rest of the year was consumed by the drafting and revision of memoranda.

August at Fort McNair in Washington, DC, and was renamed the Operations Research Office (ORO) in December.[35] Back at the Defense Department a Weapons Systems Evaluation Group (WSEG, pronounced "wesseg") was finally established at the Pentagon in January 1949, thereby completing the American constellation of military OR groups.[36]

Among civilian research organizations, RAND and WSEG stood apart from OEG, OAD and the new ORO in that both were clearly dedicated to evaluating potential R&D programs in light of the military's long-term strategic requirements. They were, however, also very different from each other. As an independent contractor RAND was able to attract a large staff by building a reputation as an enclave of highly talented and well-paid individuals who boasted diverse intellectual interests and were free to examine whatever problems they saw fit. Not only did RAND staff evaluate top secret R&D programs, they performed cutting edge work in mathematics, and increasingly, computing and microeconomics as well. To cap it all off, RAND was located in the sunny climate of southern California far from the prying Washington bureaucracy. While it was often difficult to point to precise impacts of RAND's "long term" studies—a fact that led to

[35] The ORO would later move to a former girls' school in nearby Chevy Chase, Maryland; and later still to another facility in Bethesda, Maryland. The most complete history of the ORO can, of course, be found in Shrader, *Operations Research*; but also see W. L. Whitson, "The Growth of the Operations Research Office in the U.S. Army," *Operations Research* **8** (1960): 809-824. Despite being the only OR group in Britain or America set up within the logistical, rather than operational, branch of a military service, the ORO, as we will see, proved extraordinarily dedicated to providing advice on all kinds of operational topics.
[36] The history of WSEG is fairly shrouded, and the outline presented here is about the same level of detail as other sources. See, for instance, George E. Pugh, "Operations Research for the Secretary of Defense and the Joint Chiefs of Staff," *Operations Research* **8** (1960): 839-846; Shrader, pp. 60-62; and Bruce L. R. Smith, *The RAND Corporation*, pp. 4-5. Philip Morse provides a good outline of the first year and a half of WSEG; Philip M. Morse, *In at the Beginnings: A Physicist's Life* (Cambridge, Mass.: The MIT Press, 1977), pp. 244-261. Considerable documentation is available in the papers of the Research and Development Board and the Joint Chiefs of Staff at the *NACP*, Record Groups 218 and 330.

criticisms and even the occasional existential threat—RAND was widely considered to be an important asset to the Air Force, a productive and generally happy place.[37]

WSEG, on the other hand, was not in RAND's class, and it had difficulty recruiting more than a dozen analysts. Its budget remained in the anemic hundreds of thousands of dollars,[38] and its analysts were paid on civil service scales. It was bombarded by research requests from the Joint Chiefs of Staff, most of which were extraordinarily broad in scope, leading to the production of multi-volume reports running into the hundreds of pages.[39] Because of the still rampant competition between services, the results of these studies were especially prone to sparking controversy. This fact only exacerbated the problems of overwork and the lack of clear impact its studies were capable of producing. Further, WSEG also suffered from an instability of leadership. Whereas RAND made military analysis into an attractive career choice for civilians, research directors at WSEG seemed to view taking the post as more of a patriotic duty. They were all high profile academic scientists with wartime experience, but, eager to return to higher paying and more appealing university posts, they came and went quickly. Against his own inclinations, Philip Morse agreed to become the first research director in

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[37] General references on the origins and character of Project RAND and the RAND Corporation are numerous. See especially Smith, *RAND Corporation*; David R. Jardini, "Out of the Blue Yonder: The RAND Corporation's Diversification into Social Welfare Research," Ph.D. dissertation, Carnegie Mellon University, 1996; David A. Hounshell, "The Cold War, RAND, and the Generation of Knowledge, 1946-1962,"*Historical Studies in the Physical and Biological Sciences* **27** (1997): 237-268; Collins, *Cold War Laboratory*; and Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, Mass.: Harvard University Press, 2005).

[38] "National Military Establishment Weapons Systems Evaluation Group, Preliminary Budget Estimate, Fiscal Year 1950," 10/1/1948, *NACP*, RG 330, [NM-12/341-A], Box 521, Folder 3. In initial planning, total expenditure for 1950 was projected to be $280,415. RAND was founded on a $10 million grant from the Air Force. When that grant ran out, RAND began receiving annual appropriations. In 1950, the second year RAND received appropriations, its expenditures were projected to be at $4 million.

[39] As of fall of 2005, most WSEG reports housed at the National Archives in Record Group 330 remained classified, but declassified reports (some of which were only available in part) were unusually large in comparison to those of the other civilian analysis organizations.

March 1949, but for only one year.[40] He stayed for a year and a half. He was followed by Bob Robertson, then of the California Institute of Technology, who left in 1952. Then Bright Wilson came from Harvard's chemistry department, but he stayed for only a year. He was replaced by William Shockley who stayed for two.[41]

The personnel problems at WSEG affected the nature of the work being done in ways that detracted from its overall utility. By Wilson's and Shockley's tenures, the problems were deemed chronic. In April 1953 Wilson and Lieutenant-General Geoffrey Keyes, the military director of WSEG at the time, visited RAND to see what could be done to improve their organization. Ahead of their visit, the bemused author of RAND's management committee minutes recorded parenthetically, "They assume RAND is successful and can assist them."[42] After the visit, though, the committee could only marvel at the situation at WSEG:

> If RAND has a problem in correlation of its studies, WSEG has several times the problem trying to correlate and sift out the right answers being done by some 30 RAND-type organizations. Some of the basic problems of WSEG are the level in the Defense Department at which they work, inability to obtain competent help, etc.[43]

Essentially, WSEG was attempting to collect and assemble technical information, intelligence estimates of enemy capabilities, and strategic thinking being produced by the

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[40] Philip Morse wrote to Karl Compton (Bush's successor as chair of the RDB, whom Morse knew well from MIT), "...I have accepted the position ... with some reluctance from a personal and professional point of view, only because I realize the importance of the task." Letter from Morse to Compton, 2/7/1949, *NACP*, RG 330, [NM-12/341-A], Box 521, Folder 1.

[41] It was oddly difficult to piece together the chain of directors. Morse's tenure is well-known, but many others were only identifiable from the archival files of the Joint Chiefs of Staff's searches for replacement candidates. Shockley's tenure is only known to me from documents mentioning him at the RAND Corporation (see below), and a letter from John von Neumann to William Shockley, announcing his resignation as a WSEG consultant in 1955; John von Neumann to William Shockley, 4/15/1955. In response to a letter from John von Neumann urging Robertson to take the post, he replied, "Thanks for your helpful letter concerning the WSEG job; you may be sure I'll remember it when I'm wondering where to turn for succor! Fact is, my conscience has already wrestled me into taking it on, but your account of the situation reinforces my decision," H. P. Robertson to John von Neumann, 5/23/1950; both letters are from *JvN*, Box 19, Folder 8.

[42] RAND Management Committee meeting minutes, 4/15/1953, RAND Corporation Archives, Box 32.

[43] RAND Management Committee meeting minutes, 4/22/1942, RAND Corporation Archives, Box 32.

military into comprehensive statements about the worth of proposed weapons systems. While these studies were rather useful compendiums of policy knowledge, they could not even put a dent in the problem of coordinating R&D programs from which RDB's part-time scientific advisors had rightly shrunk half a decade earlier.

By 1954 WSEG had already outlived the Research and Development Board, which had been folded into an office of the Assistant Secretary of Defense for Research and Development the previous year. In effect, the Defense Department had given up trying to bring a semblance of order to the free-for-all that characterized inter-service R&D budget allocation, since it had neither the intellectual wherewithal to justify budget allocations on a fair and rational basis, nor the political backing to do it arbitrarily. In 1961 the new Secretary of Defense Robert McNamara would famously attempt to impose order on the unruly system using both President John Kennedy's support and a new budgeting scheme concocted by RAND economists, but he would cause unrelenting military resentment toward him in the process. Before that happened, the Defense Department felt it could at least lend some guidance to the services when they did agree to coordinate their efforts through the Joint Chiefs. Hence, rather than fold the struggling WSEG, the department and the Joint Chiefs heeded suggestions for turning it into a RAND-like non-profit entity. The result was a contract signed with MIT in 1955 to recruit analysts. In 1956 this contract was superceded by the foundation of the Institute for Defense Analyses (IDA).[44]

[44] On IDA and related science policy developments, see Finn Aaserud, "Sputnik and the 'Princeton Three:' The National Security Laboratory That Was Not To Be," *HSPS* **25** (1995): 185-239.

IDA was a non-profit institution that was supported through a consortium of universities for the specific purpose of aiding WSEG.[45] IDA's civilian liaison to WSEG doubled as WSEG's director of research, while WSEG itself began to move increasingly toward being a military organization. This arrangement proved to be an astoundingly successful cure for WSEG's immediate ills. Within a few years IDA had reached its goal of recruiting one hundred university scientists to work on WSEG problems, and owing to its swift recruitment, it had also moved into new areas, building alliances with other big scientific projects, such as the Defense Department's new Advanced Research Projects Agency (ARPA), and providing contract research assistance for blue ribbon panels such as the 1957 Gaither Committee and the 1958 Draper Committee.[46]

In many senses, IDA was akin to RAND in that it was intended as an environment for broad, cutting edge studies of national security issues. However, as we will see in this and coming chapters, RAND analysts tended to learn and hone their craft and make their reputations at RAND. IDA, on the other hand, seemed to inherit the characteristic WSEG trait of serving as a revolving door for established academics, only with a more positive connotation. It styled itself as an organization that would allow willing university scientists to leave their academic perches for a time to contribute to national security issues.[47] Like the President's Scientific Advisory Committee and the Cosmos

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[45] MIT, the Case Institute of Technology, the California Institute of Technology, Tulane University, and Stanford University. This list soon expanded considerably.

[46] The best consolidated information on IDA remains its annual reports, which can be found in libraries. Morse was critical of the separation of WSEG's civilian staff, and the later rapid expansion of IDA; Morse, *In at the Beginnings*, pp. 260-261, 287.

[47] IDA did have a large contingent permanent analysts farmed out to various projects, but only a minimal staff of its own. The president of IDA, James McCormack, Jr, wrote to the IDA trustees in 1957 that 10-20% of IDA analysts were academics, and that "we definitely want to rotate this group of individuals who have special knowledge and motivation. They are our new brooms, our insurance against falling into a rut, our valuable messengers when they return to their campuses broadened in their knowledge and

Club in Washington, DC, it became a magnet for many members of the nation's scientific elite, attracting participation from high-powered university presidents such as MIT's James Killian and Case's T. Keith Glennan, administrative maestros of government science such as Herbert York, and up-and-coming physicists such as the laser pioneer Charles Townes.[48] In fact, Townes was largely responsible for creating IDA's Jason Division, which had the explicit objective of fostering a scientific policy elite for the post-Manhattan Project generation.[49] Essentially, though, IDA was a ready supplier of scientific personnel for whatever purpose the Department of Defense or the National Security Agency might need.

Somewhat strangely, in playing this role, IDA was cast within an ascendant historical narrative then being built around operations research. IDA's annual report for 1959 stated that, "Operations analysis, once a minor addendum in the decision-making machinery, has accepted a crucial role in the determination of our defense posture and capabilities."[50] The annual report for 1960 was more explicit:

> Involvement of scientists and scientific methods in solution of military operational problems has created a new branch of science, variously called Operations Research, Operations Analysis, Weapons Evaluation and the like. As events have progressed since World War II, the scope of military problems thus considered has broadened to include the larger issues of national security. IDA's role in this was intended mainly to make more easily and productively available the scientific resources of the academic community. The disciplines employed are the old and familiar ones but when applied to

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understanding." See memorandum from James McCormack to IDA trustees, 6/4/1957, *PMM*, Box 8, Folder 2.

[48] Killian and Glennan were the first two chairmen of IDA's board of trustees, and around the same time became, respectively, scientific advisor to President Eisenhower and the first head of the National Aeronautics and Space Administration. The role of membership in the Cosmos Club in the lives of America's scientific elite has yet to be explored by historians, but becomes apparent when sifting through scientists' personal papers in archives; see also a brief mention in Daniel S. Greenberg, "The Myth of the Scientific Elite," *The Public Interest* **1** (1965): 51-62, p. 53. The Atheneum in London seems to have played a similar role for British scientists.

[49] See Aaserud, "Sputnik"; and the more journalistic Ann Finkbeiner, *The Jasons: The Secret History of Science's Postwar Elite* (New York: Viking, 2006).

[50] The Third Annual Report of IDA, 1959.

> national security they take on new dimensions, possibilities of which are still far from being fully developed and understood.[51]

The connection that was being drawn between IDA and OR can best be understood as a combination of an historical understanding of wartime OR as the intervention of well-known academic outsiders such as Patrick Blackett and Philip Morse in military operational planning on the one hand; and IDA's close relationship with WSEG, which had been consistently described as an operations analysis group, on the other. We should not, however, automatically accept this historical framing of IDA's activities. Despite some similarities in the work performed and their historical roots, military OR at the service level had become something that was intellectually different from the intervention of outsider scientists in military and R&D policymaking.

The Intellectual Basis of Postwar Military OR

OR groups working within the military services did not see their activities as a "minor addendum in the decision-making machinery," even though the description is accurate enough. The phrasing implies that they, as scientists, were theoretically entitled to a higher level of participation in planning, but that they were intellectually controlled and confined to narrow problems by established military administrators, and that it took ambitious organizations such as RAND and IDA to unleash their real potential. By and large, the scores of scientists who participated in military OR did not embrace that point of view. Postwar military OR was built on a different interpretation of the wartime experience that emphasized the regularized interaction of doctrine building, planning, and independent analysis. As I argued in chapter one, the very legitimacy of wartime OR was

[51] The Fourth Annual Report of IDA, 1960.

grounded in the function OR played with respect to the military's heuristic infrastructure, and while operations researchers identified themselves as scientifically-trained civilian outsiders who could speak competently on technical issues, the idea that OR was in and of itself scientific was more of a rhetorical convention than a central driver of OR practice. OR made planning more robust; it did not make it scientific, at least not in any clear sense. This perspective held true for military OR in the postwar era.

While RAND and WSEG were taking civilian analysis into R&D planning in the late 1940s and early 1950s, the OR groups working for the British and American services charted out a much more integrated role in peacetime military planning that was more closely connected to the wartime OR experience. This wartime connection stemmed, at least in part, from certain continuities of leadership. Unlike WSEG, the service OR groups were led by relatively unknown figures who served for extended periods. LeRoy Brothers, the last chief of the Operations Analysis Section at the Twentieth Air Force in the Pacific, remained the USAF's Assistant for Operations Analysis until 1957, when he was replaced by the head of the SAC Operations Analysis Office, Carroll Zimmerman (who had a more ambitious but shorter tenure).[52] The physicist Ellis Johnson (Figure 3.1), who had served in the Navy during the war and had led a technology-oriented Mine Warfare Operations Research Group, was the director of the Operations Research Office from its foundation in 1948 until its was replaced by the Research Analysis Corporation in 1961.[53] The physical chemist Jacinto Steinhardt was head of the Operations Evaluation Group from the time Morse left in late 1945 until it was absorbed into the

[52] Holley, "Air Force Experience".
[53] The best biographical information on Johnson is to be found in Shrader, *History of Operations Research*, pp. 164-165.

Figure 3.1. Ellis Johnson of the Operations Research Office, from a profile of OR in, *This Week Magazine*, a large syndicated Sunday newspaper supplement. The lead is representative of the especially hyperbolic science journalism of the 1950s. Source: David B. Parker, "Our Greatest Secret Weapon," *This Week Magazine* (August 5, 1951); clipping from *NRC*, "PS: Com on Operations Research 1949-1954, Supporting Data: Articles & Speeches" folder.

Center for Naval Analyses in 1962.[54] The British programs were subject to more frequent shifts in leadership, with heads of OR groups frequently moving to higher ranking positions in the scientific civil service.[55] This habit of promotion from within

[54] Tidman, *Operations Evaluation Group.*

[55] The scientific advisors to the Army Council and Air Ministry, for instance, seem to have frequently been drawn from the OR groups. It is difficult to obtain precise information on the bureaucratic structure of postwar military OR organizations in Britain. In most instances, the civil service lists only offer the civil service rank of scientific personnel, not what position they held. Some information can be gleaned from reports, but tend to reveal only spotty information. For instance it is clear that the Chief Research Officer of RAF Fighter Command Research Branch was, for a period, "A. Potts", an alumnus of the wartime section, but it is not obvious what Potts' first name was, or how long he was the head of the research branch. Additional archival work could reveal additional source material. Pratt, "A Rose by Any Other Name," p. 2 does list the Admiralty Directors of Operational Research: E. M. Gollin (1945-1948), I. G. Slater (1948-1949), E. C. Williams (1949-1955), E. Lee (1955-1958), H. C. Calpine ("1958 onwards"). The history follows DOR through "about 1970" so it is presumable Calpine stayed until at least then.

and close organizational ties to the military bureaucracy might have allowed British groups to maintain a fairly stable intellectual tradition in spite of less stable leadership.

Of course, some intellectual continuity was also provided by studies of the data generated by the war that went on for years afterward—the Admiralty DOR did not finish its last study of German U-boat logs until 1966.[56] Retrospective studies were useful not only for clarifying the past, but for providing planners with sets of expectations for future scenarios. Examples include a 1947 report produced by the operations analysts at USAF headquarters, which was a fairly technical recreation of the impact of various factors on bombing accuracy of Eighth Air Force Combat Operations. Using the "working assumption" that the factors combined linearly, the study's authors even produced a slide rule calculating what accuracy would have been expected given various bombing conditions.[57] A 1951 RAF Coastal Command Research Branch study sought to reexamine instances of actual combat between U-boats and bombers between 1943 and 1945, because even though shooting proved a poor choice compared to evasion for U-boats, it was suspected Soviet submarines might attempt to improve the tactic's efficacy.[58] A 1954 OEG study of captured U-boat logs estimated that the interception and decryption of Allied signals offering timely intelligence on convoy locations allowed U-boats to double their contact rate with Allied convoys.[59]

Yet, despite offering the only available view of actual combat experience in the immediate postwar period, as time progressed, this experience became increasingly

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[56] Pratt, "A Rose by Any Other Name," p. 1.
[57] "The Causes of Bombing Errors as determined from analysis of Eighth Air Force Combat Operations," Operations Analysis Report No. 3, 7/15/1947, quote on p. 13; *NACP*, RG 341, Technical Operations Analysis Reports 1945-57 [NM-15/208], Box 12.
[58] "Aircraft losses due to U-boat flak 1943-1945," Research Branch Coastal Command Report No. 389, August 1951, TNA: PRO AIR 15/734.
[59] "Effects on U-Boat Performance of Intelligence from Decryption of Allied Communication," OEG Study 533, 3/22/1954, copy provided by courtesy of the CNA Corporation library.

obsolete. Most data for military OR studies came from equipment tests, military exercises, and speculative theorization. Wartime experience would prove no closer a guide for the development of expectations than more artificial means of anticipating the form of future battles. However, the major problem for operations researchers was not that a lack of genuine combat data robbed their conclusions of scientific value—OR had never offered any scientific guarantees about future results. Rather, the issue was that OR had no well-defined role with respect to peacetime planning. The way operations researchers worked around this problem was by assisting in the design of tests and exercises and interpreting their results with respect to overall military needs in ways that were not always immediately obvious to individual planning groups. Even still, OR groups continually made recommendations with respect to existing military specialties, which meant that OR had to be conducted in even closer collaboration with these specialists than during the war, and that study results were carefully marked as purely informative in nature and clearly fenced off from policymaking processes.

Postwar OR studies ranged from extremely specific problems to fairly expansive service-wide issues. At the most technical level, OR was scarcely distinguishable from the work of personnel at military development and testing facilities. For example, in December 1946 the RAF Fighter Command Research Branch issued a report entitled "Training in Air to Air Firing with the G.M.II. Fixed Ring Gun Sight and the Gyro Gun Sight, Film Assessment as the Method of Recording the Results from the Training Exercises".[60] The study was very similar to work done by Edwin Paxson during the war at the Jam Handy Organization, Eglin Field and AMP. Few studies, however, were

[60] J. E. Henderson, "Training in Air to Air Firing with the G.M.II. Fixed Ring Gun Sight and the Gyro Gun Sight, Film Assessment as the Method of Recording the Results from the Training Exercises," RAF Fighter Command Research Branch Report No. 680, 12/20/1946, TNA: PRO AIR 16/1045.

dedicated to exclusively technical material. In fact, OEG studies were marked as relating specifically to operational and *not* technical issues. The bulk of OR work was dedicated to relating technical capabilities to operational capabilities, resulting in studies such as the ORO's 1955 study, "An Operational Evaluation of the LACROSSE Guided Missile, the 155-mm Gun, and the 8-in. How (C),"[61] which assessed the tactical suitability of the weapons in question to the tasks for which they might be used. Recall the simpler, but conceptually related discussion of methods of blowing holes in walls done for the British Army during the war.

Some OR studies investigated overall technical and tactical capability to deal with various strategic situations. For instance, a 1950 study done by the Admiralty DOR examined the ability of the United Kingdom to counteract a Soviet mining campaign in coastal waters. The study began by estimating how many mines the Soviets could lay based on their assumed capabilities, and how many mines the British had the ability to sweep, and thus how many minelayers the British could expect to need to destroy in order to avoid being defeated by a mining strategy.[62] These broad studies could focus future research on specific tactical problems. A follow-up DOR study on the mine defense problem examined at what depth different kinds of available minesweepers could safely operate, because in the event of a mine war, it would be necessary to assign minesweepers in the most appropriate and efficient way in order to reduce the overall number of minelayers that needed to be destroyed to avoid defeat.[63]

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[61] W. F. Druckenbrod, et al, "An Operational Evaluation of the LACROSSE Guided Missile, the 155-mm Gun, and the 8-in. How (C)," Technical Memorandum ORO-T-312, September 1955, *NACP*, RG 319, General Decimal File {Security Classified Correspondence, 1955} [A1/137-B], 1955: Box 18, (040, ORO).
[62] "A Study of Defence in a Mining War against the United Kingdom," Admiralty DOR Report No. 15, August 1950, TNA: PRO ADM 219/406.
[63] "The choice of the Magnetic Safe Depths for Coastal and Inshore Minesweepers," DOR Report No. 18, August 1951, TNA: PRO ADM 219/409.

In addition to studying ongoing problems of conventional warfare, military OR also entailed studies of problems related to the increasing array of unconventional weaponry that always threatened to transform future wars. While in both Britain and America atomic energy research was cordoned off within independent agencies, the military still had to make plans to fight nuclear wars. The headquarters operations analysis group in the USAF had an atomic warfare division that was actually separate from its combat operations division, and produced studies with titles such as "A Method for Estimating the Radius of Blast Damage From High Explosive or Atomic Bombs", "Accuracy and Target Coverage with Atomic Bombs", and "The effect of thermal radiation from the super bomb on the bombing aircraft".[64] Likewise, the ORO dedicated special projects to unconventional warfare. ORO Project ATTACK dealt with atomic warfare, while Project COBRA studied the potential impact of biological, chemical and radiological weapons. Study titles from these projects included "Vulnerability of the Infantry Rifle Company to the Effects of Atomic Weapons", "The feasibility of chemical warfare in the defense of a perimeter in the Naktong Valley Basin", "Atomic Play COUNTERTHRUST II", and "Redstone Missiles as Atomic Warhead Carriers".[65]

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[64] See Norman C. Dahl, "A Method for Estimating the Radius of Blast Damage From High Explosive or Atomic Bombs," Technical Memorandum No. 12, 3/7/1949, *NACP*, RG 341, [NM-15/208], Box 13; Alonzo C. Cohen, "Accuracy and target coverage with atomic bombs," Technical Memorandum No. 36, 8/15/1952, *NACP*, RG 341, [NM-15/208], Box 13 (actually produced by the ordinary Combat Operations Team); J. C. Mouzon, "The effect of thermal radiation from the super bomb on the bombing aircraft," Working Paper No. 32, 4/15/1952, *NACP*, RG 341, [NM-15/208], Box 11.

[65] On ORO projects, see Shrader, *Operations Research*, p. 98. See W. A. Taylor, et al, "Vulnerability of the Infantry Rifle Company to the Effects of Atomic Weapons," Technical Memorandum ORO-T-1 (CONARC), CORG-FER-1, August 1955, *NACP*, RG 319, [A1/137-B], 1955: Box 18, (040, ORO); Robert J. Best, "The feasibility of chemical warfare in the defense of a perimeter in the Naktong Valley Basin," Technical Memorandum ORO-T-5 (FEC), 1/31/1951, title recorded in examination of *NACP*, RG 319, ORO decimal files; "Atomic Play COUNTERTHRUST II," Technical Memorandum ORO-T-11 (USAREUR) and "Redstone Missiles as Atomic Warhead Carriers," Technical Memorandum ORO-T-311 have been removed from their boxes in the ORO decimal files; only the titles are available.

In both the United States and Britain, Army OR took on a peculiarly expansive character, seeming to define OR as encompassing investigations of *any* aspect of military planning that was poorly understood and was likely to have a bearing on mission success. This tradition went back to the war—recall the vast array of problems facing the British Army OR group in southeast Asia. In peacetime, these more non-technical kinds of studies were organized into bona fide research programs with their own dedicated specialists. Given the importance of the soldier to army activity, it is not surprising that physiological research was common in both the British AORG and the American ORO. Under Ellis Johnson's ambitious leadership, the ORO established research programs to work on problems such as assessing and improving infantry performance (Project DOUGHBOY), on issues in psychological warfare (Project POWOW), and on the principles and practices of partisan and guerilla warfare (Project PARABEL).[66]

Not all OR studies after World War II were dedicated to issues of planning for future combat scenarios. Actual combat studies resurfaced with the Korean War in the early 1950s. While the war was not characterized by the same emphasis on novel technologies and measure versus countermeasure battles that characterized World War II, operations such as naval bombardment, aerial interdiction of roads and railroads, and prosecution of ground combat still required considerable investigation to assess effectiveness and improve policies for future combat.[67] Furthermore, the particular

[66] Overview of British and American Army OR problems can be found in the proceedings of the Second Tripartite Conference on Army Operations Research.

[67] The war even temporarily brought Edward Bowles back to government service on a mission for WSEG. Upon returning, Bowles sought to use the occasion to ingratiate himself once again with the military decision making infrastructure, prompting his secretary, who had been with him since his time in the War Department, to implore him to "make, for once and for all, a realistic assessment of [his] position in Washington." It is an unusually frank and astute assessment of the factors that lead to influence, and a scathing indictment of Bowles' tendency to remain an outsider working on a variety of problems and still believe he could affect policy. The bulk of the letter concerns the jeopardy Bowles was in at that time of

conditions of combat in Korea required directed analysis. One USAF operations analysis study, for instance, evaluated the effectiveness of the MiG-15 against American aircraft.[68] The war also provided an environment for Ellis Johnson and the ORO to pursue its broader vision of OR in an applied environment. While the conflict lasted, field teams produced studies of the effects of combat on infantry performance, the effects of propaganda on soldiers and civilian populations, the problems of administering occupied territories (Project LEGATE), and other problems not related to the conventional problems surrounding the interrelation of technology and tactics.[69]

The ORO's most famous study—and one of its few publications to reach the open literature—was "The Utilization of Negro Manpower" (Study CLEAR), an extensive sociological analysis of racially integrated units in Korea that was performed in 1951. The study employed six members of the ORO staff, nine consultants, and three subcontractor organizations.[70] Relying on interviews of soldiers' attitudes and morale, and assessments of combat effectiveness, the study's reports, ultimately collected into two sizeable volumes, compared the fighting effectiveness of integrated and segregated units. It observed that even though racial tensions persisted, such as in off-duty socialization, integrated units had good morale and fought together effectively, and that

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losing his worth as a consultant to the Raytheon corporation, which was only exacerbated when he had so eagerly answered the "siren song of Washington" and run off to Korea for over a month. The letter did not stop him from trying to wield some influence, such as by writing a long letter to the military director of WSEG about the flaws in the Research and Development Board. Letter from Minnie Mae [Emmerich] to Edward Bowles, 10/3/1950, *ELB*, Box 36, Folder 2.

68 Martha A. Olson and Richard T. Sandborn, "Aircraft Attrition in Korea: An Analysis of MIG-15 Effectiveness," Operations Analysis Technical Memorandum No. 31, 2/11/1952, *NACP*, RG 341, [NM-15/208], Box 13.

69 Discussions of the use of OR personnel in Korea can be found in both Shrader, *Operations Research*; and Tidman, *Operations Evaluation Group*. Discussions of the use of the social sciences by RAND, other contract research agencies, and army OR in Korea and elsewhere, see Ron Robin, *The Making of the Cold War Enemy: Culture and Politics in the Military-Intellectual Complex* (Princeton: Princeton University Press, 2001).

70 The subcontractors were the American Institute for Research, Inc.; International Public Opinion Research, Inc.; and the Bureau of Applied Social Science Research of Columbia University.

black soldiers had higher morale and improved performance in integrated units. It recommended that the army should continue to implement desegregation, and argued that there were no substantial barriers to doing so.[71]

While the military's operational needs worked their subtle influences on the contents of military OR, in general, military OR organizations declined to participate in the mathematical innovations that increasingly characterized the professional field of OR in the 1950s. Partially, this attitude was a result of a discrepancy in subject matter. Professional OR tended to study logistical problems, which usually occupied the attentions of entirely separate branches of the military. In fact, some of the most important methodological developments in non-military OR came from the military, *but not military OR.* Most notably, the optimization technique of linear programming was originally a product of mathematicians working for the Air Force Comptroller Branch's Project SCOOP. As we will see in the next chapter, the RAND Corporation also became a major source of methodological innovation, and that RAND's association with military OR helped integrate these innovations into the nascent OR profession. While the service OR groups certainly acknowledged the utility of the new methods, they only tentatively engaged with them. Some mathematicians employed by them produced the odd memorandum on these techniques; the ORO dedicated a small project to methodology (Project OPSEARCH); and military OR did occasionally cross over into logistics

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[71] The collected edition is Alfred H. Hausrath, et al., *The Utilization of Negro Manpower in the Army*, Report ORO-R-11, August 1955, *NACP*, RG 319, [A1/137-B], 1955: Box 24, (040, ORO). It was later published: Johns Hopkins University Operations Research Office, *The Utilization of Negro Manpower in the Army: a 1951 study* (McLean: Research Analysis Corporation, 1967). An early summary was made publicly available: Alfred H. Hausrath, "Utilization of Negro Manpower in the Army," *Journal of the Operations Research Society of America* **2** (1954): 17-30. See also Robin, *Cold War Enemy*, p. 52.

studies.[72] Historically, though, this divide over methodology represented the first clear schism in the postwar practice of OR. The major exception to this trend was in military gaming and digital computer simulations. Both RAND and ORO were important players in this burgeoning area, and the techniques spread to other services and across the Atlantic to British OR groups, but also worked their way into the toolkits of professional operations researchers who worked in academics and industry.[73]

It should be understood, however, that military OR could probably never have easily followed along with the course of professional OR because of the need to retain credibility with military operations researchers' regular collaborators in operational planning—and planning, with its heavily arbitrary elements, did not lend itself to cleanly structured optimizations. The RAND Corporation was allowed wide latitude on account of the fact that it was supposed to produce studies that would prove valuable for the Air Force in the long term, and by the 1960s, even RAND was reined in to an extent.[74] Ellis Johnson's continually expanding ambitions for the ORO—resulting in, among other things, the foundation of a high school summer program (see Figure 3.2) and moves to develop studies of emergency aid and economic development assistance—won him few new friends in the Army, and may have contributed to the eventual closure of the ORO in

[72] See, for example, Joseph A. Joseph, "The Application of Linear Programming to Weapons Selection and Target Analysis," Operations Analysis Technical Memorandum No. 42, 1/5/1954, *NACP*, RG 341, [NM-15/208], Box 13. The ORO was exceptional in that it established a major presence in the professional OR community, for instance, publishing frequent reviews of theoretical works in the *Journal of the Operations Research Society of America.*

[73] See Alfred H. Hausrath, *Venture Simulation in War, Business, and Politics* (New York: McGraw-Hill, 1971); Hausrath was a high-ranking member of the ORO and of its successor, the Research Analysis Corporation. Also see Sharon Ghamari-Tabrizi, "Simulating the Unthinkable: Gaming Future War in the 1950s and 1960s," *Social Studies of Science* **30** (2000): 163-223, although her focus on extraordinary nuclear war simulations belies their routine use in gaming various kinds of strategies and tactics. On gaming in Britain, see "Preliminary Note on the Use of Gaming in Operational Research," Admiralty DOR Memorandum No. 181, TNA: PRO ADM 219/533.

[74] See Smith, *RAND Corporation*, pp. 125-139 ; The later chapters of Jardini, "Blue Yonder" cover the divergence of RAND from the Air Force.

Six of the Washington area students involved in The Johns Hopkins University's ORO project in Chevy Chase discuss their report. From left are Richard Bentz, John Donaldson, William Chace, Jerome Horowitz, Paul Grim and David Gunlock.

A-Attack to Kill 80%, 11 Boy Scientists Predict

By Hal Willard
Staff Reporter

Eleven high school students, concerned about today's problems, predict a nuclear attack on Greater Washington would kill eight of every 10 persons.

Their technical report says the area should start building shelters which would protect persons from nuclear fallout and be able to accommodate refugees for "long periods" of time."

It says Civil Defense evacuation plans should be "reevaluated in terms of probable post-attack fallout conditions" and

area to allow people who lived through the blast and heat to get there in time to avoid lethal radiation.

Sections of Northern and central Virginia proved to be fallout free about 90 per cent of the time. The rim of this area begins about 30 miles from Washington and is accessible by five main highways. The problem is: How to get there before it is too late.

Maryland No Refuge

The youths concluded from their research that it would be useless to flee to Maryland and expect to live. Lethal fall-

Figure 3.2. A clipping from an article about the results of the ORO summer program. The strategic nature of the study should not be taken as typical of OR fare, but the results were approved as credible by Ellis Johnson, and the students' work was published: Richard Bentz, William Chace, Gordon Doerfer, John Donaldson, Thomas English, James Graves, Paul Grim, David Gunlock, Jerome Horowitz, Leland Miller, Albert Small, "Some Civil Defense Problems in the Nation's Capital Following Widespread Thermonuclear Attack," *Operations Research* **5** (1957): 319-350. Source: Hal Willard, "A-Attack to Kill 80%, 11 Boy Scientists Predict," *Washington Post*, 12/28/1956, page B1. © The Washington Post. Reprinted with Permission.

1961.[75] As a rule, though, military OR gained credibility by producing reports that were deemed immediately relevant to military policymaking, and relevancy was fostered

[75] See Shrader, *Operations Research*, pp. 120-122. There were many factors culminating in the dissolution of the ORO, including instances where Johnson spoke to the press about non-classified issues, the leak of ORO documents to the press (though not by ORO members), which resulted in an increasing Army demand that ORO not undertake "controversial" studies. Johnson resented the Army's new attempts at control of the ORO agenda, and tensions eventually led to the formation of the Research Analysis Corporation (RAC), which was similar to ORO, but did not have Johnson at its head. The circumstances surrounding the closure of ORO led some to joke that RAC stood for "Relax And Cooperate"; Smith, *RAND Corporation*, p. 272. Also see Johns Hopkins University's internal assessment of the situation, memorandum from P. Stewart Macaulay, provost, to Milton Eisenhower, president, 11/17/1960, *JHU*, Papers of the Office of the Provost, Series I, Subseries 4, "Associated Universities/ORO Review of ORO-Army Relationships, 1960-1961" folder, Box 28. On Johnson's more general attitudes about the role of OR, see Ellis A. Johnson, "The Crisis in Science and Technology and Its Effect on Military Development," *Operations Research* **6**

through the establishment of a close intellectual relationship between operations researchers and military policymakers.

This intellectual relationship between OR and the military should not, however, be analyzed assuming a strict patron-expert dynamic, wherein scientists would analyze a problem and produce a scientific solution, which would then either be accepted or rejected by the military. In order to understand the intellectual status of postwar OR, it is crucial to understand how knowledge related to authority within the policymaking structure of the military. In general, the military was eager to keep authority well away from civilian scientists, who concurred with this policy. Because military doctrines and plans contain many arbitrary assumptions, it was important that the command structure retain the authority to command personnel on an arbitrary basis. Of course, the military was also eager to improve its policies whenever possible—issuing orders without merit invites defeat, lowers morale, and risks insubordination. However, the military took a conservative attitude toward doing so, out of tradition and inertia, certainly, but also doubtless because it was not willing to change policies based on a simple indication that an alternative policy *might* prove superior. Changing policies simply on the basis of new information would shift authority from the command structure to its heuristic organs, which was not permissible if commanders were not to be second-guessed on account of disputes over preferred doctrines.

I argue that operations research fit into this intellectual system and drew strength and independence from it by allowing itself to be explicitly *deprived* of authority. This argument runs strongly against the traditional historiographical tendency to link objective

(1958): 11-34; and Ellis A. Johnson, "The Long-Range Future of Operations Research," *Operations Research* **8** (1960): 1-23.

science specifically with authority, as though it were a judge in a courtroom. In the case of OR and the military, independence from authority allowed OR groups to maintain honesty about the definitiveness of their conclusions and, thereby, to promote an intellectual dialogue with military policymakers. Because no decision rested upon an OR result, operations researchers were free to identify areas where they deemed their own conclusions to be tentative. In fact, OR risked losing credibility if researchers boasted certainty on claims that were obviously speculative or that were later proven to be false. Their studies were useful to policymakers not because they told the military what to do, but because, like doctrine builders, they provided clear intellectual frameworks within which the military could dispute its internal differences and come to agreements when changes in policy were warranted.

To maintain this intellectual role with respect to the military policymakers, OR groups relied on a carefully orchestrated process of maintaining boundaries between knowledge and authority. Document distribution was probably the most important tool in this process. In order to secure a broad distribution, a document had to be thoroughly vetted, so as not to embarrass the military agency that produced it on account of a failure to answer criticisms leveled against it. Wide distribution was an honor usually reserved for official policies. OR groups distributed the results of their work on a narrower basis, and the intellectual status of the knowledge contained in them still had to be carefully defined. OR documents were divided into categories, usually: reports, technical memoranda, and working papers, indicating, in decreasing order, the strength and finality of the conclusions being presented. The documents themselves were filled with qualifying language, and assumptions narrowing the validity of the claims were explicitly stated, and often underlined.[76] No OR document was ever given official approval by any military agency, and documents produced by OR groups were usually marked with a disclaimer explaining as much. Working papers were not even given the official approval of the head of the OR group, and were distributed mainly to garner feedback and not to recommend any action.[77]

Typically, individuals on the initial distribution list for OR reports and memoranda would be asked to comment on the results and make recommendations for wider distribution if the document was deemed of high reference value. The comments received were usually quite well-informed, often offering line-specific critiques, singling out where the recipients concurred with the study results, where recommendations had already been implemented, where they concurred "in principle" but where results were likely to be tempered by circumstance, where they disagreed and why, where arbitrary assumptions limited the validity of the recommendations, where limited sampling threw results into questions, where additional study was most needed, and so forth. These discussions merged with other policy discussions and recommendations, which, if heeded, resulted in changes in policy that were agreed to adhere to the best available evidence and could be distributed broadly. By framing policies within an intellectual framework, OR

[76] A memorandum LeRoy Brothers wrote to the USAF's Assistant Chief of Staff for Operations about an Air University report on a militant stance against the Soviet Union demonstrates the point well. Brothers argued against distribution beyond the Air Force, even though he found the report compelling, in part because it rendered "strong conclusions allowing very little doubt," and he feared that unless the conclusions were backed up by additional research and represented as judgments and not facts, the report would "be subjected to very damaging criticism." Memorandum from LeRoy A. Brothers to Major General James E. Briggs, 6/16/1954, *NACP*, RG 341, Vice Chief of Staff; Operations Analysis, 1949-1961 [A1/1009], Box 3, "TS-1011" folder.

[77] These statements are based on the examination of a large sample of British and American postwar OR documents contained in the US and UK National Archives. For known locations of these documents, see Appendix B.

rendered this process more robust, but it rarely resulted directly in policy changes, so it is difficult to measure the actual influence of OR.[78]

Of course, the intellectual differences between policy and scientific research were not always clear-cut in an environment where the trading of opinions led to policy through rational, but nevertheless deeply political processes. In a 1951 letter to the Army's Chief of Psychological Warfare, Ellis Johnson discussed the status of OR as scientific work in response to a query as to whether ORO reports found to be in error should be recalled. When military doctrines and orders are updated, previous versions are typically recalled, but Johnson did not feel this process should apply to OR reports and memoranda, because OR was a "pioneering enterprise".[79] He pointed out, "The very greatest scientists have published papers which have... proved erroneous. Such papers are not withdrawn, they simply become known to those concerned with the subject as wrong or negligible." He recognized that the

> Army audience is not exactly the same as a scientific audience, and many of those who read our papers may not be regular readers of scientific work on the same subject, and there may not be the usual safeguards that exist in professional scientific work by which students are guided to ignore past papers which are either wrong or irrelevant.

Even still, he felt that OR inquiries should retain their histories, and that it was the responsibility of military planners to converse with OR work on scientific grounds, but he allowed that the Army had "a perfect right to control its distribution." Cognizant of the authoritative status of military doctrine, he pointed out that the army could recall

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[78] It is difficult to track reception of OR studies, however the Army kept significant files relating to commentary on OR studies, from which this discussion is largely drawn. See, for example, a memorandum from John G. Hill to Deputy Assistant Chief of Staff (G-3), re: ORO Final Report ORO-R-5, 2/27/1953, *NACP*, RG 319, [A1/137A], 1953: Box 20, (040, ORO). See Appendix B for a more general listing of these decimal files.
[79] This attitude was not uniform. Working papers issued by the USAF operations analysis team were issued with markings explaining that they could be withdrawn at any time. The ORO seems to have issued comparatively few working papers. See "Department of the Army Operations Research Office Publications," 1/3/1955, *NACP*, RG 319, [A1/137B], 1955: Box 22, (040, ORO).

distributed papers as deemed necessary when they "might lead to instances of confusion."[80] Military OR was science, but it was not public science.

The core threat to OR credibility was not controversy—controversy was inevitable in highly arbitrary policymaking processes. It was the failure to integrate OR effectively into the military policymaking apparatus. The generation of confusion was one possible sign of failure, but so was neglect. Neglect could result from a failure to keep studies timely. Because OR studies took some time to design, execute, and prepare for distribution, there was a real possibility that when the process was complete, the data used would be obsolete, the situation would have changed enough to invalidate results, or an irrevocable decision would have already been made with respect to the question at hand. Studies could also suffer neglect because they did not engage with appropriate military personnel. Typically, while policies were issued from high in the military chain of command, the recommendations that led to them originated at intermediate levels of the hierarchy. If studies did not reach the appropriate individuals, or did not address problems pertinent to their work, OR work was likely to languish. The worst problem, of course, was if reports were consistently judged to contain unrepresentative, misrepresented, or outright false information. Ensuring close contact with military experts could keep such mistakes out of studies. In 1955, the ORO set up a murder board to subject studies to an additional layer of withering criticism before distribution, and made repeated surveys of study production to determine how they were used, by whom, how useful they were supposed to be, and in what way.[81]

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[80] Letter from Ellis Johnson to Brigadier General Robert A. McClure, Chief of Psychological Warfare, 12/26/1951, *NACP*, RG 319, [A1/137A], 1950-51: Box 40, (020, ORO).
[81] See especially "The Professional Evaluation of ORO Publications," an ORO booklet, in *JHU*, 02.001, Papers of the Office of the President, Series I, Box 34, "Jan. 1956 – Dec 1956" folder. The booklet points

Because many of the intellectual foundations for military operations research were institutionalized during the war, on the whole military OR groups did not require a great deal of time in which to adapt to their new, slightly different but better institutionalized postwar role. The emphasis on intellectual engagement with military planners and independence from chains of authority, as well as the practice of restraint with respect to the strength of their claims they made, all had wartime antecedents. By contrast, even though it enjoyed more immediate freedom than the military OR groups to pursue "blue sky" work, the RAND Corporation had a more difficult time situating its own much more ambitious systems analysis program within the Air Force R&D policymaking machinery.[82]

RAND's Doorstep: Edwin Paxson in the Desert

The driving force behind systems analysis in the earliest years of the RAND Corporation was the mathematician Edwin Paxson, who, as we saw in the last chapter, had been an expert on the mathematics of aerial gunnery during the war. After the war, Paxson served with the United States Strategic Bombing Survey, and then became the co-head of the mathematics department at the Naval Ordnance Test Station (NOTS), which had been established in 1943 in the deserts of southeast California near the small town of

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out that the need for quality control is especially acute for classified research because peer criticism will not serve to regulate the evaluation of claims. See also L. D. Flory, "Analysis of the ORO Work Program with Respect to Timeliness," 11/1/1960, *JHU*, Papers of the Office of the Provost, Series I, Subseries 4, Box 28, "Associated Universities/ORO ORO Publications, 1960-1962" folder; Helen S. Milton, "ORO Publications: An Army-Wide Review, Summary of Questionnaire responses of 85 Army agencies," 11/21/1960, *JHU*, 03.001, Papers of the Office of the Provost, Series I, Subseries 4, Box 28, "Associated Universities/ORO, ORO Publications, 1960-1962" folder. Unfortunately, I have not found such extensive documentation of OR issues relating to the OR groups of other services, but it seems reasonable to presume that similar issues prevailed. Some discussion of ORO practices were published in Whitson, "Growth of the Operations Resarch Office".

82 "Blue sky" quote from Vaughn D. Bornet, "John Williams: A Personal Reminiscence (August, 1962)," 8/12/1969, RAND Document D-19036, p. 1.

Inyokern.[83] The activities taking place at NOTS were not particularly remarkable, involving many of the same problems in weapons and tactical design that were typical of equipment development and testing, but Paxson was determined to apply the lessons of the war to formulate more farsighted methods of tackling these problems. Because he was only at NOTS for a short while before moving to RAND, he apparently did not have an opportunity to push any major shifts in R&D practices personally, but one document he produced while there, Working Paper No. 10, "The Role of Mathematical Research in the Airborne Fire Control Development Program," provides an excellent view of what his most immediate postwar vision was, and how it owed a clear debt to his work with AMP.[84]

According to Paxson's paper, prior to World War II, equipment development programs often failed to take into account two points: first, what future strategic and tactical situations were likely to be faced, and, second, what technology was expected to be available at a future point in time. "Airborne fire control development of the past," he observed,

> has consistently violated both principles. In the first instance, design proceeded on the basis of naive assumptions of rectilinear target courses, and low angular rates; and, in the second instance, design presumed that any improvements in projectiles, in radar inputs, in barbettes, in speed and maneuverability of the parent aircraft, and stabilization or second order sight corrections could be simply adjoined to existing equipment.

Because tactics were embodied in any fire control system, it was better to understand the tactical and technical aspects of the problem at a fundamental level rather than make a comparatively arbitrary primary assumption about how equipment should be designed and then assume all necessary modifications were secondary corrections to the primary

[83] Paxson biography in RAND Corporate Archives, Box 13, Bob Specht's RAND biographies, M-R.
[84] NOTS Working Paper No. 10, Memorandum from E. W. Paxson to Dr. L. T. E. Thompson, "The Role of Mathematical Research in the Airborne Fire Control Development Program," 5/13/1949, Ed Paxson papers, Box 4, RAND Corporation archives, Santa Monica, California.

model. We may recall the experience of Blackett's Circus and the Admiralty Research Laboratory "amputating" the British Army Anti-Aircraft Command's Sperry predictors and giving new instructions to the operators of guns using Vickers control devices only to conditionally obey what the predictor told them to do. These were crude attempts to alter the primary logic of the predictors. Now that the war was over, Paxson, who had felt the proper role of AMP had been to address "stop-gap" problems, believed it was wasteful to use mathematicians primarily as "patch up" specialists who "had little contact with functional specifications and design." The time to work on intermediary technologies was passed. He understood that the need to use mathematicians in an *ad hoc* manner would always present itself as new situations arose, but knew that "time and money" could be saved through a thorough theoretical analysis that preceded "rigid commitment to a particular design" by minimizing the "real dangers of obsolescence and inadequacy". In this context it was more "the mathematician's feeling for generalization, structure, and alternative" than "his specific knowledge of differential equations, function theory, statistics, geometry, mathematical physics, and computing techniques" that made him such a valuable asset to the development and design process.[85]

Paxson's experience with L. B. C. Cunningham's combat theory was the crucial touchstone in his thinking on the matter. This theory could take known parameters of technical and tactical system components and help determine which configurations could be eliminated from the outset, which were promising, and which aspects of the problem required further technical research and component testing. Even better, in the years since Cunningham first developed his theory, it had gained a new ally in John von Neumann and Oskar Morgenstern's game theory, which they rigorously developed in their 1944

[85] *Ibid.*

tome, *The Theory of Games and Economic Behavior*.[86] We will recall from Cunningham's initial letters to the Royal Aircraft Establishment that his theory aimed to calculate the probability of outcomes of an aerial duel if the combatants "adhere to their tactical programmes". It did not, however, calculate the implications if a variety of tactics were to be used owing to the fact that no one tactic would prove consistently best. For instance, suppose a fighter could open fire on a bomber early or late in an approach, or somewhere in between, and that the fighter's armaments could be adjusted to maximize the fighter's effectiveness depending on how early it opened fire. If an armament scheme was designed to optimize attack based on the assumption that it was always best to attack late, a bomber's designers could, in turn, design a defensive scheme that would guard against such attacks. What an armament designer would really want to do is assume that fighters would use a mix of tactics from attack to attack so that the bomber would have to be prepared to defend against each. A rough approach to this problem might presume that fighters would attack from three different distances 1/3 of the time. However, if a late attack really did tend to be superior, it would make sense to attack late more often and weight the armament scheme accordingly. Just how often to attack late and how the armament scheme should therefore be weighted would call for the so-called "minimax" solution provided by game theory, which would maximize the payoff for such so-called "mixed strategies".[87]

In his time at Inyokern, Paxson became deeply engaged with the problems surrounding the application of combat theory and game theory to exactly such problems.

[86] John von Neumann and Oskar Morgenstern, *The Theory of Games and Economic Behavior* (Princeton: Princeton University Press, 1944).
[87] Early development of game theory at RAND dealt with exactly this class of problems, see Paul Erickson, "The Politics of Game Theory," chapter 2.

The results of various small duels, such as the one between a fighter and a bomber, could be combined to find an optimum armament for each. In a letter to von Neumann, Paxson described such encounters as "tactics in the small". Once these outcomes were understood, one could apply similar reasoning to the assignment of forces against various kinds of targets in a diverse melee—recall the Navy OR report from Chapter One on the aerial battle in the Leyte Gulf. These were "tactics in the large". In theory, then, the choice of tactics in the large could, in turn, be combined to develop overall war strategies. Following Warren Weaver's argument in "Comments", military commanders made these kinds of game theoretical decisions all the time, but based only on a vague understanding of the issues. The point of rendering them in mathematical format was not to go about the problem in a fundamentally new way, but to determine how much sense various tactical and strategic choices actually made, how self-consistent they were, and what fallacies they might perpetuate.[88]

At NOTS, these kinds of questions were of immediate concern in deciding between competing weapon research, development, and design programs. Certain kinds of weapons made more sense if one had a sense of to what use they were likely to be put. Paxson outlined four stages of studies in which mathematicians could play differing roles: long term, auxiliary, short term, and operational. These stages proceeded in the opposite order from the scheme he would outline to von Neumann a few months later. One first dealt with the armament problem "in the large" and then proceeded to the design problems "in the small". Since NOTS was not responsible for building war plans from

[88] See Jardini, "Blue Yonder," pp. 50-52; Jardini quotes a letter from Paxson to von Neumann, 10/6/1946, RAND D-63. According to Jardini, p. 50, "Paxson, in particular, believed that game theory might provide a vehicle for the mathematization of conflict." However, as we have seen, mathematical studies of conflict were already highly embedded in technology and tactics; later strategic studies would have different roots.

scratch, in order to decide what kinds of equipment needed to be designed, it was necessary to hold a fixed idea of what kinds of tactical situations certain kinds of equipment were likely to be used, which, in turn, would determine what combinations of tactics and technology would produce the best results for that fixed scenario. So, for instance, in order to design a gun sight, it was important either to know or to estimate, quantitatively, what "ranges, range rates, angles off, angular rates, target course curvature, tolerated sight settling time, [and] motion of gun platforms" were likely to be encountered in the future. These sorts of considerations could have enormous impacts on a research program. For instance, if bomber speeds were sufficiently high, it might be considered wise to place barbettes (protective armor) over the rear of engine nacelles to guard against a preponderance of rear attacks; but barbettes precluded the use of jet engines. If the defense of engines against fighters was more favorably impacted by barbettes than by additional gains in speed, it was one argument in favor of staying with propellers, though, of course, countless other factors had to be considered as well.[89]

Once promising configurations had been found for different armament configurations, to choose finally between them it was necessary to make "measurements" of expected kills and losses for each configuration. Paxson observed that in actual combat, operations analysis groups had been able to derive such measurements by parsing statistics from real combat. In absence of such direct means, one had to place "reasonable bounds" on these figures. To a certain extent, this process deployed highly arbitrary methods. As in previous design practice, for some aspects of a problem one had simply to set ranges and determine how sensitive the final result would be within those ranges. In some cases, one had no choice but to guess. The point was not that the

[89] Paxson, NOTS Working Paper No. 10.

process would arrive at a guaranteed best weapons system, but that the factors bearing on equipment design could be identified and explicitly incorporated rather than be overlooked entirely. The choice of a best weapons system would always be the result of guessing, but the guesses made would at least be less arbitrary than they would have been without the benefit of detailed mathematical analyses.[90]

Along the way to a complete analysis, auxiliary studies would also be necessary to clarify peripheral points bearing on the main analysis such as how weapon inaccuracies bore on kill probabilities, or how well a human and a machine, in tandem, could track a target. Then, once long term and auxiliary problems were solved, the problem "in the large" disappeared, and was replaced by the problem "in the small" where design engineers took over, while mathematicians stepped into the background. According to Paxson, the fire control engineer's job "is the rugged job of invention, design, construction, and extensive bench testing of pilot models"—the familiar work of past R&D and design, but now guided by the insights provided through analysis. Then, in operational contexts, the mathematician could reappear to serve as a consultant for the evaluation of airborne tests, to aid in gunnery training, and to "serve in an operations analysis section" *in times of war*. This last job meant offering advice on field modifications, devising "statistical theorems of performance," and extrapolating trends from operations that could be fed back into long term and auxiliary studies for future equipment development.[91] Paxson clearly saw OR through his own wartime experience—as something that mediated between technology and the surprises that real war inevitably brought.

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[90] *Ibid.*
[91] *Ibid.*

The Origins of Systems Analysis at RAND

In early 1946 AMP alumnus John Williams briefly became an employee under Edwin Paxson in the Mathematics Department at the Naval Ordnance Test Station. Finding that there was no housing available on the base near Inyokern, Williams arranged to work out of an auxiliary NOTS office in lush Pasadena instead. As Williams settled in there, in nearby Santa Monica, Project RAND was just coming into existence, and its director, Frank Collbohm, was looking for staff. Impressed by AMP's war work, Collbohm contacted Warren Weaver, who recommended that he hire Williams. According to Williams, Weaver had told Collbohm that Williams was "the laziest man that he had ever met" and "therefore could be relied upon to find an easy way to solve hard problems."[92] And RAND's problems were extremely hard. If the use of the B-29 had proved to be too open-ended for AMP to digest, then the question of how best to prosecute intercontinental warfare was more daunting still. Yet, as with the AC-92 study, it was an irresistible chance to go beyond the bounds of AMP's everyday studies, and to do the "things it was sensible to do," as Williams later recalled. More fundamental mathematical studies of the various kinds of problems AMP had encountered had always been desirable, but they had been put off on account of the urgency and quantity of the military's demands. Williams could not resist the opportunity to see what could be accomplished in a peacetime environment, and he sheepishly informed Paxson and the administrators of NOTS that he had accepted a job working on a project for their rivals in the Army Air Forces.[93]

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[92] Williams joined RAND in June 1946, and quickly diverted his fellow AMP alumnus, Olaf Helmer, from NOTS to RAND as well. See Bornet, "John Williams," pp. 1-3, 12, 15, quote on p. 3.
[93] *Ibid.*, p. 12, quote on p. 1.

Acquiring Williams was one of RAND's first steps in assembling a specialized civilian organization that could make a worthwhile contribution to the problems to be faced by military planners in the postwar period. Project RAND was divided into various sections (later called departments in the independent RAND Corporation), most of which were straightforwardly technical: nuclear energy, rockets, communications (including missile guidance), electronics, and airplanes, with others added later. The "Military Worth" section, headed by Williams, dealt with the problem of integrating studies into a comprehensive analysis. Military Worth soon became the "Evaluation" section. With the later addition of economics and social sciences departments, Evaluation simply became known as the Mathematics, but it still entailed the use of mathematics in the far-reaching way as Paxson had been pushing at NOTS.[94] Paxson himself left NOTS to join RAND in March 1947, where, independent of sectional affiliation, he took control of its first major research project, a study of how best to prosecute an intercontinental nuclear bombing campaign.

The study was to be the first "systems analysis", a term that soon came to identify what was considered to be RAND's foremost intellectual product.[95] Previous historians have observed that, in the beginning, systems analysis was RAND's attempt to construct a "science of warfare", but the meaning of the statement is obscure.[96] Much of the

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[94] Historians have been fascinated by the concept of military worth, and the creation of this section has been detailed several times, but see especially Collins, *Cold War Laboratory*, chapter 4.

[95] Paxson had no departmental affiliation, and became known as "The Systems Analyst"; see Fred Kaplan, *The Wizards of Armageddon* (Stanford: Stanford University Press, 1983), p. 87, Collins, *Cold War Laboratory*, p. 164.

[96] See Jardini, "Blue Yonder," chapter 2; according to Collins, *Cold War Laboratory*, p. 163: "RAND hoped that the authority of knowledge, legitimated through scientific method, might provide a common view of problems and solutions. Rational persuasion might be the means to overcome the pull and counterpull of American pluralism." This statement is not representative of the initial purpose of systems analysis. Collins' characterization of OR is equally off the mark. According to Collins, in OR problems "could [...] be formulated precisely and solved for reasonably clear answers, such as the best tactics of

connotation comes from certain common ideas about the role of quantification in providing science with authority, and much of it from certain ideas that have arisen about Paxson himself. As with Curtis LeMay, the journalist Fred Kaplan has provided us with some enduring images of this particular "wizard" of Armageddon: "Paxson was ingenious, rude, abrasive, a driven man, hardworking, hard-drinking, chain-smoking." At internal RAND briefings he was a brutal critic of others' work, driving one briefer into a faint. He was "the numbers-cruncher *par excellence*. He loved to devise and try to solve equations of gargantuan dimension, the more numbers and variables and mathematical complexities the better."[97] Paul Erickson echoes Kaplan in his dissertation on game theory, calling Paxson a "hard-boiled operations researcher *par excellence*" (though as we now know he never saw himself as an operations researcher at all).[98] The dust jacket of Martin Collins' book on RAND makes use of an all-too-clichéd image, showing us a picture of Paxson at a blackboard, tending to his equations. We are *supposed* to know who this man is. He does not get along well with humans—his comfort is in vice, and his trust is in numbers. Like Weaver's idealized computer, he is intent on reducing all the complicated realities of the world into measurable quantities, with the intent of producing a scientifically-legitimized "correct" answer to the Air Force's research and development

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avoiding, repulsing, or countering submarine attacks," p. 171. By comparison, systems analysis, informed by the "Weaver-Williams ideal of a science of warfare," was overambitious, and inevitably bound by "practical limitations," p. 172. As we know, very little military OR involved precise formulations, and the point of system analysis was to reduce arbitrariness, not transcend limitations.

[97] Fred Kaplan, *The Wizards of Armageddon*, pp. 86-87; Collins, *Cold War Laboratory* quotes Kaplan on p. 164.

[98] Erickson, "The Politics of Game Theory", p. 115. When I informed Erickson of the similarities of language, he was shocked to learn of it, because he had not read Kaplan in years (and Collins does not quote "*par excellence*"), which, to me, demonstrates either the power of evocative language to inform our impressions, or it is a remarkable coincidence.

quandaries. We are certainly not left with someone who thought deeply about the philosophy underlying his work.[99]

In reality, though, our knowledge of Paxson is scant. Unlike some other RAND affiliates he did not publish in the open literature, and most of his RAND publications are purely technical.[100] Even still, equipped with better knowledge of his background at AMP and NOTS, we can say a little more. Certainly, his thinking was far more nuanced than simply trusting to the virtues of quantification. Kaplan informs us that quantifying "every single factor of the strategic bombing campaign" was "his dream".[101] While it is certainly true that what became known as the Strategic Bombing Systems Analysis was an attempt to construct and solve an enormous interconnected set of equations, this work was far more grounded in Paxson's experience with weapons design than in his fancy. Certainly his work at RAND was a direct descendant of his work with Jam Handy, AMP's AC-92 work, as well as his budding ambitions for work on fire control at NOTS. It was probably no accident that just as AC-92 had been an attempt to compute the best armament configuration and tactical use of the B-29, the Strategic Bombing Systems Analysis quickly settled into the problem of selecting the characteristics of the Air

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[99] According to Paul Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, Mass.: The MIT Press, 1996), pp. 167-168, mathematics-oriented people do not like to think deeply about things: "The 'hard' master of computers is a subject whose major cognitive structures are preconceived plans, specific goals, formalisms, and abstractions, who has little use for spontaneity, trial-and-error, unplanned discovery, vaguely defined ends, or informality. This is also American culture's prevalent image of the scientists, generically portrayed as disciplined thinkers who deploy long chains of logical and mathematical reasoning to arrive at their subtle, powerful understanding of nature's ways." Edwards observes, "In practice, of course, the image is false." Nevertheless, the imagery is important because "contests for legitimacy" take place between those who think through "hard" and "soft" means. Apparently in the public imagination the means are incompatible; hence the common portrayal of Paxson and, by extension, RAND. But, see Sharon Ghamari-Tabrizi, *Herman Kahn*, where we find a RAND that embraces both sides of the coin.

[100] An exception—and an intriguing elixir to combat our vision of Paxson—is his snide review of Norbert Wiener's *Cybernetics*; E. W. Paxson, "Cybernetics," *Journal of the Operations Research Society of America*, **1** (1953): 252-253. Following the grain of the historiography, Paxson and Wiener should be intellectually kindred spirits.

[101] Kaplan, *Wizards*, p. 87.

Force's next generation bomber, the B-52. Boeing had already won the contract to design and construct the B-52, but the company's engineers were proving incapable of providing a bomber meeting the Air Force's requirements and were sent repeatedly back to the drawing board. For its part, the Air Force complicated the problem by continually changing its desired specifications for range, speed, flying altitude, and aircraft size.[102] These were *precisely* the kinds of issues that Paxson and, before him, Weaver felt could be averted by developing military requirements and design *in tandem* by employing mathematics in the early conceptual stages.

As a successful veteran of AMP, Paxson certainly understood full well the possibilities and limitations of applying mathematics to choices of this kind. He knew mathematics did not invariably lead to successful design, but he also did not consider himself to be in the business of constructing scientifically validated matters of fact. His aim was not to supercede military experts as an outside scientist, but to explore as extensively as possible the logical consequences that military experts' piecemeal studies had for the development of overall weapons systems before the necessity of making a near-term decision arose.

As with OR, the first step was to become a scholar of military plans and doctrines, and to determine what factors were likely to bear on the outcome of an operation. In analyzing the B-52's configuration, this process entailed obtaining knowledge of the enemy's expected capabilities, and the expected capabilities produced by American R&D, and how these factors might be expected to play out in combat. It entailed knowing what kind of budget could be expected, what targets would need to be hit and with how much

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[102] See Collins, *Cold War Laboratory*, chapter 5, esp. pp. 179-183, for a discussion of Paxson's systems analysis in light of continuing disputes over the design of the B-52.

force, and how many personnel could be trained. Once these factors had been outlined sufficiently well to begin—if need be, they could be modified later[103]—the next step was determining expectation values of combat results. A great deal of this problem was bibliographical. RAND assembled information from equipment manufacturers, military research and testing facilities, military schools, the military OR groups, and war records. RAND staffers made frequent visits to military facilities, military personnel made visits to RAND, and RAND sometimes organized colloquia to discuss certain kinds of tactical and technical problems.[104] Although RAND was not quite the sort of place where military, scientific and industrial experts could routinely come together, as in Edward Bowles' vision, these experts' intellectual efforts were still integrated, but by proxy, through the mediation of systems analysis.[105]

The greatest problem Paxson and the systems analysts faced was deciding how to model different aspects of an operation. There were no "correct" ways to model, only ways that incorporated the most available knowledge pertaining to how future combats would unfold. It was important not to try to model more precisely than facts warranted, but it was also important not to leave out important factors on account of a lack of precise data. For instance, it was particularly difficult to create sets of expectations for how the

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[103] Edward Quade, who later became one of the leading figures in the systems analysis community, worked under Paxson on the Strategic Bombing Systems Analysis, later recalled that he had worked on solving an equation based on Cunningham's combat theory detailing bomber-fighter machine gun duels (Paxson's wartime specialty) that resolved itself into a non-converging series. Quade grappled with this problem for quite some time before the issue was eventually thrown out of the systems analysis altogether, because it became presumed that machine guns would not enter into future bomber-fighter duels on account of improvements in air-to-air missile technology, which would restrict such duels to distances beyond a machine gun's effective range; see interview with Edward Quade (conducted by Martin Collins), 2/18/1988, *NASM*, RAND Oral History Project, pp. 9-10.
[104] See for example, E. W. Paxson, "Report on an Air Tactics Colloquium," RAD No. (L)255 (Draft), 4/9/1948, RAND Corporation library.
[105] Martin Collins, *Cold War Laboratory*, argues that RAND replaced Edward Bowles' "associationalist" vision for RAND (Collins' term, not Bowles') with a rationalist vision, but I would argue that the two visions are not as far apart as Collins implies.

technical capabilities of individual combatants impacted the outcomes of "mass action" problems comprised of multiple combatants. During the war operations research had been an excellent source of statistical information about how such combats tended to play out in practice. In planning for the future, it was only possible to make estimates, using wartime data, information on the capabilities of individual combatants, model set-ups such as those used at the Mt. Wilson Observatory in the AC-92 project, and mathematical tools such as the Lanchester Equations, which estimated battle casualties from opposing forces of differing size and fighting effectiveness.[106]

The best way Paxson and his analysts had of knowing how good their models were was if expectations accorded with diverse data sets, or with expectations derived by other means. For example, "first order" theories of bomber attrition simply involved making an estimate of what percentage of bombers were likely to survive the trip to a bombing run, and what percentage of those survivors were likely to make the trip back. A "second order" theory incorporated information from the potential "history of a mission" which had to "be given in idealized form (i.e., a mathematical model)." One could estimate how likely a bomber would be to encounter flak or fighter interceptors, and then estimate how vulnerable a bomber would be to such attacks.[107] One could scale down an analysis such as to the level of accuracy of enemy fire against vulnerable surface area of an aircraft, until one reached a level where one had to admit that flipping a coin would yield as good of a guess. If theoretical conclusions did not match tests or war data,

[106] See "Trip Report of E. W. Paxson, May 3 to 17, 1947," RAD-127, discussing collaboration with the Navy, and listing numerous "documents to be extrapolated" on mass action problems, including studies from Air University and RAF Fighter Command Research Branch. Also see Paxson's discussions of these sorts of problems in Working Paper No. 10, and E. W. Paxson, "Second Order Theory of Bomber Attrition," RAD-150, 7/3/1947, RAND Corporation library.
[107] Paxson, "Second Order Theory".

it might suggest the need for extraneous factors, such as a "degradation factor" accounting for human unpredictability, or the mysterious "Paxson Packing Factor," as it was dubbed by others.[108] Beyond a shadow of a doubt, there was a lot of cheating involved in systems analysis, but Paxson would have considered his cheats to be minor compared to the brazenly arbitrary and, worse, unacknowledged assumptions that went into prior equipment designs.

RAND and Military Policymaking

In the first years of the RAND Corporation, Edwin Paxson and other analysts did not seem to give very much thought to the fact that they were not actually equipment designers or military policymakers. By creating a complete mathematical model of a new weapons system and a set of expectations for its operational implementation, RAND effectively superceded the intellectual work of the entire military planning process. While it seems unlikely that the analysts at RAND actually expected the Air Force to take their results and implement them forthwith, it is difficult to imagine how else Paxson's behemoth synthetic effort might have been used. The limited analyses of military OR contributed to the piecemeal evolution of policy over time. By focusing on the design of intermediary technologies, AMP also managed to find a place within the cyclical process of equipment design and implementation. What Paxson's systems analysis seemed to do was redraw entire military policies from the ground up all at once. In at least one

[108] See Quade interview, p. 13; Collins, *Cold War Laboratory*, p. 193; on discussion of degradation factors, see Herbert Goldhamer, "Human Factors in Systems Analysis," RM-388, 4/15/1950, copies available beyond RAND, which states, p. 3: "It is characteristic of RAND systems analyses that they call for a degree of intrepidity above and beyond the ordinary call of scientific duty." It goes on to state: "...the value of D[egredation] is at present necessarily based on vague quasi-empirical intuitions." The sociologist Goldhamer sought to offer a more sophisticated analysis of human impact on systems; see below.

instance, Paxson contrasted his work with "operations analysis" by calling it "operations synthesis".[109] Some years later, RAND analyst Albert Wohlstetter felt that "systems design" was a more appropriate name for what RAND was doing.[110] Needless to say, RAND suffered strained relations with the Air Materiel Command.[111]

We can start to address the relationship between RAND work and the military policymaking by observing just how closely RAND depended on military knowledge. For instance, when Paxson was participating in a 1951 analysis of a potential Soviet offensive through West Germany called Exercise STYX, it was found necessary to estimate how many casualties could be expected, given certain characteristics about attacking and defending forces at the Rhine River. Paxson wanted to model the situation using casualty estimates given in military officers' planning manuals and Lanchester's differential equations, but he made a visit to Fort Leavenworth in Kansas to discuss the "physiology of ground operations" with military professionals first. There he was informed that the problem was not so simple, and that the manuals' figures were actually meant to be used for planning the logistics for a campaign, not for estimating the outcomes of any particular battle. When planning actual battles, they told him, it was necessary to consider "terrain, weather, the degree of organization or disorganization of the forces on a given side at a given stage of the battle, [...] the number of days of combat which the various forces have been submitted to and, most particularly [...] the quality of the replacements coming up." These factors could have a surprisingly radical

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109 Paxson, "Air Tactics Colloquium," p. 3.

110 See Albert Wohlstetter, "Analysis and Design of Conflict Systems," in *Analysis for Military Decisions*, edited by E. S. Quade, PAGES (Chicago: Rand McNally, 1964); see also Albert Wohlstetter, "Systems Analysis Versus Systems Design," P-1530, 10/29/1958, available online at: <http://www.rand.org/about/history/wohlstetter/P1530/P1530.html>.

111 Collins, *Cold War Laboratory* discusses RAND's relationship with the AMC extensively. According to Collins, the roots of this animosity were in Bowles' initial conception of RAND as a counterweight to AMC authority.

impact on modifying the estimates given in the official manuals as preparations were made for battle.[112]

Paxson was skeptical that officers actually made calculations anywhere near so intricate, "and still felt that there were a few magic little numbers which an officer estimating the situation was really using." To test his hypothesis, Paxson asked the officers to carry out a simple simulation of a battle on a map. After witnessing the way the battle unfolded—prompting Paxson to comment that a graphic model of force movements could be animated beautifully by Walt Disney[113]—he was persuaded of the officers' point and became "rather discouraged about the entire approach to the problem of ground operations through differential equations." Instead, he asked, "Why shouldn't we use professionals as professionals in connection with the estimation of the results of ground operations?" He proposed to prepare a matrix of special situations versus relative ground forces, and then bringing in a small panel of officers to fill in numbers of "approximate losses in personnel and materiel" for the various scenarios and then use their combined opinions to make plans.[114]

While Paxson and other systems analysts clearly saw military personnel as being able to provide the most reliable information on the component problems of policies and the nature of the relationship between components, they styled systems analysis as a newer, more reliable way of examining the broader *synthetic implications* of military

[112] E. W. Paxson, "Sixteen Hours of Discussion at Forth Leavenworth," D(L) 1115-PR, 12/15/1951, RAND Corporation library.
[113] Paxson's invocation of Disney in visualization presents an intriguing kernel for a potential study of the history of the role of visual modeling in science, particularly in the period preceding advanced computer graphics. Recall also Paxson's wartime employment with the Jam Handy Organization film production company.
[114] This technique was an suggestion for the use of the "Delphi Method", which was developed by RAND analysts (notably Olaf Helmer) to digest differing expert opinions into an analyzable format. This method remains an accepted means of analyzing expert opinion today; Paxson, "Sixteen Hours".

expectations. In one memorandum, Paxson urged that RAND foster solid contacts with the Air University, since, he wrote, "one may suppose that the major RAND function is to supply technical and scientific numerical implementation of the qualitative [Air War College] type thinking."[115] He probably did not mean to imply that the military's schools did not think quantitatively at all, but, rather, that their correlation of quantifiable and even non-quantifiable factors did not employ a level of rigor that could prevent the propagation of fallacies if the implications of military expectations and rationales were only hastily correlated as they so often were in the ordinary policymaking process. Deeper numerical analysis could expose contradictions and fallacies in the explicit or implicit rationales of military plans, and it could expose implausible or uncompetitive plans before they reached a well-developed stage of policymaking.

However, the great advantage held by the standard method of policymaking was that it balanced all sorts of factors that were difficult to fit into an analysis, even if in a very crude and possibly self-contradictory way. In order for a systems analysis to attain relevance with respect to it, there was an obligation for it to be *complete*, which meant that analysts had to take responsibility for producing relatively or even completely arbitrary plan elements when no expert opinion was available. RAND analysts thus felt they even had to quantify factors that resisted quantification such as morale, because of the fact that any decision that was actually made on account of these factors was made because, at least implicitly, it was expected to have some measurable impact on results. A military officer might make a decision that lowered technical efficacy but increased morale because it was ultimately expected to have a long-run beneficial effect on *some* criteria used to measure policy value. After all, keeping morale high was only a means to

[115] Paxson, "Trip Report," p. 7.

the end of winning wars. RAND analysts thus committed themselves to asking difficult and uncomfortable questions such as just how much morale one should buy through decreases in efficacy. If the military could make such decisions by using simple rules of thumb, then RAND was obligated to make those rules of thumb explicit, or devise ones that were at least as good. While RAND opened itself up to criticism by doing something as futile as quantifying the unquantifiable, it would have also opened itself up to criticism if it did not include those factors at all.

In accordance with the need to understand the less technical side of military planning better, John Williams pressed for the incorporation of social scientific and economic work at RAND. RAND's management was initially skeptical of the idea of incorporating less technical areas of research. As Williams later remembered it, the prevailing sentiment was:

> …well, we've had a little contact with economists, and we're not exactly transported by the experience. As for the rest of the people in the social sciences, we know nothing about them, but they seem to be at least an order of magnitude worse than the economists, so far as really having their fields in hand is concerned; we're skeptical, and it appears that if we go blundering into this business, as is suggested, it doesn't seem to us to be a very conservative thing to do.

Williams, however, insisted that, to be as complete as possible, RAND's analyses had to be "catholic". Using the usual measuring stick of scientific progress, he agreed that the social sciences were "in the fourteenth century" compared to the physical sciences and engineering, but, he explained, "I thought that was our situation: fourteenth century, tenth century, who cared what century; we need them to the extent that they can contribute. We can't do the job we want to do, leaving these factors out—these social, economic, and political factors."[116] In the ensuing years, RAND would become a remarkably influential organization in the history of the social sciences.[117]

However, if a complete study contained so many arbitrary assumptions that, as far as anyone really knew, an arbitrary choice of *overall* design was as likely to be as effective as any other, was it really worthwhile to do any analysis at all? RAND was effectively founded on the idea that it was. In a 1947 address given to a RAND-sponsored conference of social scientists in New York City, Warren Weaver explained as much: "I take it that every person in this room is fundamentally interested in and devoted to what you can just broadly call the rational life." He explained that someone devoted to the "rational life" was someone who "believes fundamentally that there is something to this business of having some knowledge, and some experience, and some insight, and some analysis of problems, rather than living in a state of ignorance, superstition, and drifting-into-whatever-may-come."[118] His repeated use of the word "some" indicated

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[116] Bornet, "John Williams," p. 21. There is some room for confusion as to what Williams and others envisioned as the role of social scientific research in RAND analysis. Essentially, it was research that would modify the results of a study by revising key assumptions in light of the role of humans in system functioning. Goldhamer, "Human Factors," p. 7, points out, "The desire for social science aid in [systems analysis] has not been born of an abstract conviction of the importance of human limitations and human variability in systems of military action or from a theoretic fondness for interdisciplinary formulations. The need for intensive consideration of human factors in systems analysis has directly emerged from current RAND work in offensive and defensive system analyses. These analyses incorporated certain human parameters which were given values made plausible by the experience of the last war and by current operational experience. Quite reasonable variations in these values and in the manner in which the parameters are incorporated into the payoff function produce radical differences in the payoff variables. It has been apparent, then, that such human parameters are not trivial additions intended to lend a supererogatory breadth and elegance to the functions of the systems."

[117] On the importance of RAND, see Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (New York: Cambridge University Press, 2002); S. M. Amadae, *Rationalizing Capitalist Democracy: The Cold War Origins of Rational Choice Liberalism* (Chicago: University of Chicago Press, 2003); the supporting role played by RAND in Hunter Crowther-Heyck, *Herbert A. Simon: The Bounds of Reason in Modern America* (Baltimore: The Johns Hopkins University Press, 2005); Robin, *Cold War Enemy*.

[118] Warren Weaver, "Opening Remarks, Plenary Session, RAND Conference," Conference of Social Scientists, September 14-19, 1947, *ELB*, Box 44, Folder 4.

that he did not call for the dominance of policymaking by quantitative models, but the commitment to the struggle against arbitrariness.[119] He warned:

> I rather carefully did not say the logical life, because I am not as exclusively strong for the logical life as I am for what I mean by the rational life. I think there are some things that we need to talk about that are not very logical, but which are still awfully important; and I would include an intelligent interest in alogical aspects within what I mean by an enthusiasm for the rational life.[120]

It is unclear what Weaver meant by "logical" and, even more intriguingly, "alogical", but given that the bulk of his talk was dedicated to a defense of imperfect analysis against critics who doubted the tractability of the problems to *any* analysis, it seems likely that the logical life meant a necessarily arbitrary analysis cloaked in a quantitative veneer. As in his earlier "Comments on a General Theory of Air Warfare," Weaver did not think problems could be solved scientifically. Rather, he thought scientific analysis could help policymakers think problems through as thoroughly as possible.

The trick was to figure out in what ways scientists could actually contribute to a rational approach *as scientists*. An internal memorandum proposing the establishment of an Evaluation Section at RAND, probably written by Williams, observed that "there is no particularly logical place at which to curtail the analysis, short of the limit imposed by inadequate information and understanding." Studies were supposed to be selected only on the basis of their "promise to increase information and understanding of the consequences of warfare operations." It was still wise to circumscribe the applicability to within the limits set by arbitrary assumptions "for the reason that it is easier to prognosticate the effect of an operation in a specific setting than it is to derive a

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[119] In *Wizards of Armageddon*, pp. 72-73, Kaplan uses these lines to presage how "some of those at RAND [...] would try to impose the order of the rational life on the almost unimaginably vast and hideous maelstrom of nuclear war." Kaplan uses ellipsis to cut out two of Weaver's uses of the word "some," and fails to acknowledge Weaver's caveat about the logic life, even though (given Weaver's previous experience) it seems to specifically warn against the imposition of logic against the "alogical".
[120] Weaver, "Opening Remarks".

universally applicable forecast."[121] Still, RAND was committed to coming as close to the universal as it was deemed possible. The task, however, turned out to be not so much one of setting an overall limit of analysis, but how far any given *style* of analysis could go. In attempting to present a unified, mathematical picture of a nuclear bombing campaign, Paxson's systems analysis managed to be simultaneously overambitious mathematically and too limited in analytical scope .

In early 1950, after three long years of study, the Strategic Bombing Systems Analysis was finally completed and briefed to military staff. The study's final recommendation for a turboprop bomber fell on deaf ears, and the B-52 was ultimately developed as a jet aircraft.[122] The failure of the military to adopt the study's strongest conclusion caused a combination of indignation and a loss of confidence at RAND. On the one hand, the management at RAND felt that the Air Force had been irrationally prejudiced in favor of using jet propulsion on the B-52 from the beginning, and that the analysis was merely a formality. High level military officers, on the other hand, not only felt that using economic analysis to measure such things as casualty expectations was distasteful (jet aircraft, though more expensive and thus fewer in number, stood a better chance of survival), but, more importantly, they felt that the assumptions made, such as only considering a single strike rather than repeated strikes against Soviet targets, invalidated the conclusions.[123]

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[121] J. Williams, "Program for the Evaluation Section of Project RAND," 1/20/1947, RAD-26.

[122] Albert Wohlstetter, one of the analysts most responsible for moving systems analysis away from Paxson's style, later gave him a great deal of credit, and pointed out that the Soviets actually did choose a turboprop airplane for their next bomber; interview with Albert Wohlstetter, 7/29/1987, *NASM*, RAND Oral History Project, p. 5.

[123] Collins, *Cold War Laboratory*, chapter 5, is by far the best source on the reception of the systems analysis, but also see Jardini, "Blue Yonder," pp. 56-64. According to Collins, pp. 200-201, when, after initial briefings, RAND was suddenly informed that Air Materiel Command had determined that turboprops were likely to be twice as expensive as thought, and jet engines half as expensive, RAND's

The study would be altered to incorporate multiple attacks, with an unchanged recommendation for turboprop technology, while the military would remain intransigent about the decision to use jet engine technology. A few standard slurs about the brainlessly political military and overly-theoretical scientists were exchanged, but, ultimately, a clear consensus on what *exactly* was wrong with the Paxson study or the military reception of it never emerged, and RAND came to chalk the study up as a sobering failure.[124] One 1950 memorandum pointed out that limiting assumptions on the study, such as the use of only North American bases, would severely constrain the applicability of the study, and so it "cannot properly be presented to ultimate consumers as a generally preferred bombing system."[125] In a 1953 memorandum, Edward Quade, a mathematician who had worked on the project, would argue that Paxson's analysis (and others) suffered from "an excessively narrow formulation of the problem or the omission from the criterion of factors the Air Force considers of overriding importance."[126] It is notable, though, that the mathematics of the study were rarely criticized.

The question becomes what *we* should make of Paxson's study. The historiography compels us to portray it as an instance of a scientific Icarus flying too close to the sun.[127] Martin Collins, who has given us the most insightful historical analyses of the events surrounding RAND's origins to date, concludes his chapter on Paxson's systems analysis the following way:

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management was livid, feeling that the Air Force was altering the figures arbitrarily to make the study come out the way they wanted. In any case, even with the new figure, the study still supported the turboprop.

[124] Aside from the citations in Collins and Jardini, see Stephen Enke, "Comments on Colonel R. R. Walker's Criticisms of RAND's First Strategic Bombing Systems Analysis," 6/7/1950, D(L)-709.

[125] Memorandum from J. Hirshleifer to C. J. Hitch, "Remarks on Bombing Systems Analysis," 6/15/1950, D-893-PR; RAND Corporation library.

[126] E. S. Quade, "The Proposed RAND Course in Systems Analysis," 12/15/1953, D-1991, Rand Corporation library, pp. 5-6; we will deal with the proposed course in more detail in Chapter 5.

[127] For instance, Hounshell, "RAND", p. 244, describes it as a "debacle".

> The Paxson study and the reactions to it set the tone for systems analysis at RAND through the 1950s. The heady ambitions and hopes that Warren Weaver and John Williams had for air warfare as science and that Ed Paxson and Frank Collbohm found in systems analysis subsided. The idea of using these tools to organize social resources and rationally plan for total war became *irrelevant* with the changing funding and planning environment brought on by the Korean War.[128]

I, however, would warn against the historiographical tendency to accept the clear shortcomings of the study as an invitation to proclaim the folly of RAND's attempt to create rationalized policy, because we then neglect to point out that creating a scientifically rationalized policy was never actually the point. We are only accustomed to thinking as much, because ultimately the study produced a single major recommendation that was ignored. Paxson's style of systems analysis was certainly not "irrelevant", though. The study was an extremely rich exploration of a large number of problems that were highly relevant to the problems faced by aircraft designers, and it was recognized as such.[129]

Returning to Quade's 1953 memorandum, we find a silver lining in his lament: "The impact of our full analyses has been disappointing; most of their effect has come from component studies and off-shoots from the main effort. Our major conclusions have often been received with hesitation."[130] In a later interview (conducted by Collins) he was able to see the positive side of things without the gloomy overcast, noting the enthusiasm the military had for the methods of Paxson's systems analysis: its attention to

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[128] Collins, *Cold War Laboratory*, p. 212; emphasis mine.

[129] Collins, *Cold War Laboratory*, gets at this point somewhat by describing the Acting Deputy Chief of Staff for Operations, Idwal Edwards' advocacy for RAND's *methods*, pp. 204-206. Unfortunately, the context of RAND's methods are never made clear, so it is not obvious what role they were seen as playing. Collins states, pp. 205-206: "A RAND report alone, intended as an exemplar of rational persuasion, could not work its magic of binding together disparate interests without advocates. RAND confronted the difficulty of maintaining a clear boundary between its proclaimed role as a voice of science and its role as a contributor to a process of policy and decision making in which it advocated the merit and validity of its research." I believe this passage is closer to the actual prevailing situation at the time than the overall chord Collins strikes, but I also believe that his description of RAND's "proclaimed role as a voice of science" is overstated.

[130] Quade, "Proposed RAND Course," p. 5.

technical detail, and its unique ability to correlate data from a strikingly wide variety of sources in a rigorous way. He remembered, "Everybody liked the method, really."[131] We must be absolutely clear that in rejecting the systems analysis, the Air Force was not rejecting rational or scientific thinking in favor of political dickering once more funds became available for the taking—it was rejecting the notion that RAND's conclusions represented the final word in policy. For its part, RAND knew the Air Force better than to think that it was irrational. Initial disillusionment there stemmed more from the fact that the Air Force seemed to be denying the conclusions that followed logically from its specialists' own rational presumptions.

The real problem with Paxson's systems analysis was that RAND did not understand how to integrate its broad-scale policy designs into the military policymaking infrastructure.[132] The effect seemed catastrophic because of the grand scale of the studies. If a military OR study was ignored, there was always the next study. If a RAND study was ignored, it represented a major blow to RAND's research program—but, as it so happens, the study was not actually ignored. It was simply received in ways not intended by RAND. As we will see in chapter five, RAND's brightest thinkers would spend a significant fraction of the 1950s ruminating about just how to capitalize on the strengths

[131] Quade interview, p. 15. Compare to Kaplan, *Wizards*, p. 89: "Air Force officers, almost all of whom were pilots, hated the study. They didn't care about systems analysis. They liked to fly airplanes. They wanted a bomber that could go highest, farthest, fastest." It is an attractive story: Kaplan is quoted in Jardini, "Blue Yonder," p. 63, and Hounshell, "RAND," p. 245. Collins, *Cold War Laboratory*, chapter 5, presents a more nuanced and credible view, but as we can see, still heavily influenced by the expert-patron historiographical dynamic. Quade observes that the airframe industry, in particular was extremely receptive to the study, for reasons to be further explored in chapter 5.

[132] Collins, *Cold War Laboratory*, p. 208 says as much: "A crucial weakness of the study… lay in its origins. The study had begun as an engineering exercise to identify ideal parameters in assessing the performance of various types of bombers. Paxson continually adapted the study to broaden its scope and application to model an attack on the Soviet Union. But neither Paxson nor RAND ever conceived or organized the study to address specific decisions confronting the Air Staff—the selection of the B-52 or a bombing campaign. Paxson's work only roughly and inadequately mapped onto these real-world problems."

of its studies—which were apparent from the start—while leaving the Air Force room to interpret them in ways that were amenable to their own planning work. This problem, however, was not unrelated to problems also being faced in the growing field of professional OR at the same time, and it will make sense to turn in their direction first.

Conclusion and Postscript: The American Military Policy Analysis Network

This chapter has described the roles that military operations research and early systems analysis played with respect to military policymaking. We have seen how wartime OR was translated into a stable postwar entity through its practitioners' efforts to integrate their analyses with peacetime planning efforts by taking a very modest attitude toward the epistemological authority of science vis-à-vis military planning. We have also seen how in America the rise of military OR was accompanied by a concurrent rise in independent R&D program analysis at WSEG, IDA, and especially the RAND Corporation. Finally, we have seen how RAND's systems analysis technique was initially conceived of as an extrapolation of existing weapons design techniques, notably those pioneered by L. B. C. Cunningham before and during the war; and how comprehensively integrated designs could not be integrated into existing military planning efforts as easily as more segmented OR studies could be integrated into military policies.

This historical analysis might imply that OR and systems analysis performed a rigidly defined function within a relatively formulaic policymaking process. Increasingly, especially in America, such was not the case. Throughout the 1950s, OR was steadily becoming one part of a vast infrastructure of knowledge and theory flowing relatively smoothly, if chaotically, between military policymakers, industrial laboratories, civilian contract research agencies, and the universities. Previously, this infrastructure has been studied mostly from the perspective of the universities, and mostly with an eye toward the growing anxiety over the role of the university in American Cold War society.[133] While it would be worthwhile to examine the workings of this society in greater detail, here, as a postscript to this chapter, we must limit ourselves to a very incomplete view of it with a bias toward those parts of it most closely connected to OR.

In the postwar era contract research became an increasingly prevalent aspect of not only military policymaking, but American society in general. The service OR groups, and especially the ORO, contracted research out to universities and independent research groups, such as the Battelle Memorial Institute and the Stanford Research Institute.[134] In the 1960s, RAND started accepting contracts from organizations other than the Air Force. The service OR groups themselves came to be increasingly seen as one of several breeds of contract research organizations, and the overburdened WSEG often contracted its work out to them.[135] Meanwhile, as the military continually developed its own planning capabilities, the OR groups provided more specialized assistance. For instance, when the U.S. Army began to broaden its "combat development" activities—the integration of

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133 Thomas P. Hughes, *Rescuing Prometheus* (New York: Vintage Books, 1998) is a good overview of the military systems world. David Edgerton, *Warfare State*, pp. 324-327, points out that the historiography of American science pays considerably more attention to the military than that of British science, but observes that it is still largely a university-centric view. The historiographical influence stemming from the disparity between British and American anxieties about, university-government relationships within respective declinist and militarist historiographical traditions is a rich, but unexplored topic. On the effects of the military on university science, see Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *HSPS* **18** (1987): 149-229; Stuart Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993); and Rebecca Lowen, *Creating the Cold War University: The Transformation of Stanford* (Berkeley: University of California Press, 1997).

134 See William J. Platt, "Industrial Economics and Operations Research at Stanford Research Institute," *Journal of the Operations Research Society of America* **2** (1954): 411-418.

135 Shrader, *Operations Research*, p. 62.

technologies into effective tactics—new OR offices, such as the Combat Operations Research Group (CORG) at the Continental Army Command at Fort McLean, Virginia, were created to assist, and new companies, such as the Cambridge, Massachusetts-based Technical Operations, Inc., were created to staff both the new OR assistant groups, and other new facilities such as the Combat Developments Experimentation Center in California.[136] The Air Force, meanwhile, kept operations analysis offices at its proving grounds and schools, though these offices only had a few employees. However, it is also important to remember that organizations identified with the military, such as the Air University established in 1946 at Maxwell Air Force Base in Alabama, were supposed to play similar roles.[137]

The military also became increasingly interested in integrating their long range strategic studies with long range R&D programming. Some RAND systems analysts, such as Albert Wohlstetter, became involved in the strategic studies communities, while other RAND staffers, such as Bernard Brodie, were trained specifically in strategy. They joined a separate emerging strategy elite that included such up-and-coming figures as Paul Nitze and Henry Kissinger.[138] As a complement to their efforts in strategic thinking, RAND and ORO also supported the rise of the related academic field of area studies.[139]

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[136] On these groups, see Shrader, *Army*, chapter 4. On the Combat Developments Experimentation Center, see also Franklin C. Brooks and Floyd I. Hill, "A Laboratory for Combat Operations Research," *Operations Research* 5 (1957): 741-749; the authors were employees of Technical Operations, Inc.

[137] See Robert Frank Futrell, *Ideas, Concepts, Doctrine: Basic Thinking in the United States Air Force*, Vol. 1: 1907-1960, especially chapter 7, entitled, "The Air Force Writes Its Doctrine, 1947-55".

[138] Fred Kaplan, *Wizards of Armageddon*, is quite good in following the history of strategic thinking. See also Andrew David May, "The RAND Corporation and the Dynamics of American Strategic Thought, 1946-1962," Ph.D. dissertation, Emory University, 1998. Kissinger, we might note, co-authored at least one report for the ORO during the Korean War: C. Darwin Stolzenbach and Henry A. Kissinger, "Civil Affairs in Korea, 1950-51," Technical Memorandum ORO-T-184, *NACP*, RG 319, [A1/137A], 1954: Box 21, (040, ORO). We will discuss the rise of strategic thinking in slightly more detail in chapter five.

[139] On area studies, see Bruce Cumings, "Boundary Displacement: Area Studies and International Studies During and After the Cold War," in *Universities and Empire: Money and Politics in the Social Sciences During the Cold War*, edited by Christopher Simpson (New York: The New Press, 1998), pp. 159-188. On

Meanwhile, though the Navy was never so ambitious, it, too, took steps to monitor its long term strategic viability. In 1955, it established a Long-Range Objectives Group, staffed by experienced military officers, which, as we have seen, employed an OEG-affiliated civilian arm, NAVWAG. A separate Long-Range Studies Project dealt with the selection of large R&D projects. This project soon became the Institute of Naval Studies (INS), which contracted a team of civilian analysts from the Institute for Defense Analyses. Another adjunct of the OEG, the Applied Science Division (ASD), concerned itself with more basic technology-related research.[140]

Of course, all of these OR-related developments should be seen as only a small part of a much broader expansion in the U.S. government's long-term policy analysis and R&D policymaking infrastructure that incorporated university-based defense laboratories such as Johns Hopkins' Applied Physics Laboratory and MIT's Lincoln Laboratory, new spin-off entities such as Lincoln Lab's MITRE Corporation[141] and RAND's System Development Corporation (initially a project to aid the Lincoln Lab),[142] new R&D contractors such as Ramo-Woolridge, new government defense laboratories such as Los Alamos and Livermore, new science and technology-oriented agencies such as the Atomic Energy Commission and the National Aeronautics and Space Administration, and organizations working on highly speculative technologies such as the aforementioned Advanced Research Projects Agency and IDA's Jason Division.[143] The involvement of

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the rise of Russian studies in particular, see David C. Engerman, "The Ironies of the Iron Curtain: The Cold War and the rise of Russian Studies in the United States," *Cahiers du Monde Russe* **45** (2004): 465-496.

[140] Tidman, *Operations Evaluation Group.*

[141] MITRE Corporation, *MITRE: The First Twenty Years: A History of The MITRE Corporation (1958-1978)* (Bedford: The MITRE Corporation, 1979).

[142] Claude Baum, *The System Builders: The Story of SDC* (Santa Monica: System Development Corporation, 1981).

[143] Paul Dickson, *Think Tanks* (New York: Atheneum, 1971) provides a sober and informative, but still opinionated overview of this system. It focuses on contemporary issues, so it fares less well as an historical

such OR-related groups as RAND, IDA, and, to a lesser extent, the ORO in the multiple frontiers of military research made military operations research into a poorly-defined entity in practice. To the untrained eye, OR was virtually indistinguishable from the rest of the great military-industrial complex.[144]

This broad, poorly defined system persists into the present, although individual organizations have waxed and waned in importance. For instance, RAND's pursuit of contracts beyond the Air Force coincided with a split between RAND research interests and Air Force policy interests.[145] The academic world was cut out of the military OR loop in the early 1960s. In 1961, amid turmoil between Ellis Johnson and the Army, the Army contract with Johns Hopkins University was not renewed, and the ORO was replaced by a new Research Analysis Corporation, which retained much of the ORO's staff—but not Johnson.[146] In 1962 MIT closed its contract for OEG, NAVWAG and the ASD, and these organizations combined with the INS to form a new Center for Naval Analyses, which was administered by the Franklin Institute in Philadelphia.[147]

These developments occurred just ahead of a broader schism between the military and the academic world in America. When Johns Hopkins began considering hosting the ORO in 1948, item number three on a four item list of arguments against the contract was the "wails from pacifists and appeasers," and, crossed out, "the campus Commies". Item

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resource. The Atomic Energy Commission, incidentally, had its own OR group, but I have been able to do little aside from learn of its existence. For a recent, serviceable overview, see Harvey M. Sapolsky, "The Science and Politics of Defense Analysis," in *The Social Sciences Go to Washington: The Politics of Knowledge in the Postmodern Age* (New Brunswick: Rutgers University Press, 2004).

[144] The blurriness of military OR was also reflected in Britain, although the confinement of OR to the more tightly structured military civil service doubtless kept it better bounded than did its American counterpart's closer association with the rapidly expanding universe of quasi-independent research organizations.

[145] See Jardini, "Blue Yonder". For the military's perspective on shifts in institutional prestige, see comments by Col. Francis X. Kane, in *Science, Technology, and Wafare: Proceedings of the Third Military History Symposium* (United States Air Force Academy, 1969), pp. 178-182, in which he also recoils from the historian's perspective.

[146] See note 75.

[147] Tidman, *Operations Evaluation Group*,

number one read: "The greatest single negative factor is the tremendous moral responsibility to perform effectively in a large field of effort."[148] As the Cold War arms race, the Vietnam War, and domestic unrest heated up in 1960s America, performance anxiety was overshadowed by a long simmering uneasiness about the relationship between the universities and the military, which soon boiled over, leading to a backlash on campuses against the university-military relationship. In many places this backlash led to university administrators permanently disentangling their institutions from the newly tainted military. Notably, IDA severed its ties with its university sponsors in the late 1960s as that organization became a specific target of the student body's ire.[149]

We must be careful not to read the decline of the involvement of *academic* science in military affairs to signal the decline of expert culture in general, however. In the era when OR represented a link between academic and military worlds, it was usually seen as counteracting the divide between these worlds. Whether this divide was a good thing or a bad thing depended upon one's point of view. In America, of course, the restoration of the divide came to be seen as a salvation for academic integrity (at least until the commercial world moved in soon after), but in Britain American science's ability to transcend military-industrial-academic boundaries had always provoked more of a sense of envy than pity. The British use of a scientific civil service was viewed as an inferior arrangement, because their supposed separation from university life was seen as separating them from original thought. In reality, neither the American nor the British

[148] Memorandum from Arthur E. Ruark [head of Johns Hopkins University Institute for Cooperative Research] to [Isaiah] Bowman [JHU president] and [P. Stewart] Macaulay [JHU provost] , "Check Sheet for discussion May 6. Operational Studies for Army and for Navy," 5/5/1948, *JHU*, 02.001, Papers of the Office of the President, Series I, Box 33, "May to Dec. 1948" folder. Compare this situation to that presented in Patrick McCray, "Project Vista, Caltech, and the Dilemmas of Lee DuBridge," *HSPS* **34** (2004): 339-370.

[149] Dickson, *Think Tanks*, pp. 146-147.

view deserves too much credence. The academic world was always simply a useful adjunct to a much, much larger world that was profoundly military-industrial—*and* genuinely scientific, even if it did not produce many treatises on quarks or the structure of DNA. As for the academic world, it had already developed its own very distinct notion of OR during the fifteen years following the war, and it was this notion, not the continuing military one, that came to dominate the identity of the postwar OR profession.

CHAPTER FOUR

Problems and Models: The Merger of Mathematical Modeling and OR

On the somewhat infrequent occasions when a mathematician looks at the practical problems of operations research, his first impression is apt to be that only known and elementary parts of mathematics are needed. But if he dwells with the subject longer, his reaction tends to be quite the opposite: he begins to realize that, when rightly conceived and formulated, these practical problems often turn on mathematical questions deep enough to go beyond existing knowledge and to require for their answer research into new mathematical fields.

The practical worker in operations research is apt to have the counterpart of this experience; at first he seems to be able to do what he needs to do with only the simple mathematics that is familiar to him. But with increasing experience he sees that some of his most important problems require powerful and sophisticated mathematical instrumentation.

Bernard Koopman, "New Mathematical Methods in Operations Research," 1952[1]

When the Columbia University mathematician Bernard Koopman spoke about "new mathematical methods in operations research" at the first full meeting of the Operations Research Society of America (ORSA) in 1952, he was quite conscious of the fact that most of those who identified their work with OR at that time did not necessarily consider it to be a heavily mathematical activity. We will recall that during World War II Koopman had become involved with the war effort by working on the mathematical theory of antisubmarine search for the Applied Mathematics Group at Columbia, and that he later became a member of Philip Morse's Operations Research Group in the Navy. As long as seven years after the end of the war, however, search theory was not seen as a typical example of OR practice. According to Koopman, the idea that OR *might* be mathematical seemed to be a state of mind arrived at only through a process of gradual

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[1] Bernard O. Koopman, "New Mathematical Methods in Operations Research," *Journal of the Operations Research Society of America* **1** (1952): 3-9.

revelation. Three years later, however, speaking to the seventh national meeting of ORSA, Philip Morse himself recognized and lauded the large number of theoretical techniques being worked on under the rubric of OR, but lamented that by comparison "the development of experimental techniques seems to be almost totally neglected," and felt obliged to point out that OR was "not an exercise in pure logic."[2]

In the three years between Koopman's and Morse's talks the practice of OR had not changed appreciably, but the dominant *notion* of OR had shifted from its role as an aid to military planning to the growing mathematical canon it was beginning to collect. The object of this chapter is to understand just how and why OR came to be identified as a mathematical discipline at that time. I will argue that self-identified OR practitioners did not develop their own set of theories, so much as they welcomed their acquaintances in applied mathematics, statistics, and econometrics who had come to identify their work with the idea of OR. But this intellectual merger was not planned. The available evidence suggests that it took place gradually as policy-oriented OR proponents and mathematical modelers increasingly began to see their intellectual programs as

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[2] Philip M. Morse, "Where is the New Blood?" *Journal of the Operations Research Society of America* **3** (1955): 383-387, pp. 384-385. He insisted, "Those of us who worked on operations research during the war should know this." It would be far from the last time that the wizened sages of OR pointed back to the good old days as a time when their trade was simpler, more flexible, and more broadly applicable. The lament was common among early military operations researchers. Hugh Miser, who had been a member of Air Force operations analysis since the war, wrote on the subject until his death in the late 1990s. See, for example, Hugh J. Miser, "The Easy Chair: What OR/MS Workers Should Know About the Early Formative Years of Their Profession," *Interfaces* **30** (2000): 99-111. Even after the passing of the wartime generation, some subsequent practitioners have seen World War II OR as a more pure form of scientific practice. MIT professor Richard Larson, who served as president of the Institute for Operations Research and the Management Sciences (the current American OR professional society), frequently referred to the free-form scientific inquiry familiar to the first generation of OR practitioners. In an interview conducted by *OR/MS Today* editor Peter Horner, he claimed, "Operations research cuts horizontally [...], so we're not constrained into some silo of a traditional engineering situation. We're only constrained into a silo of scientific method. Phil Morse basically said operations research is the application of scientific method to operational problems of systems. We need to be elevated to the level at least of those different disciplines and say, 'Hey, if you have operational problems that combine all these other things, call an operations researcher.'" Peter Horner, "The Science of Better Synergy," *OR/MS Today*, **31**/6 (2004): 36-43, p. 43.

fundamentally related. As Koopman's speech indicates, the compatibility between mathematics and policy was not exactly obvious, but it was real enough that a two-way dialogue between mathematical theory and policy was made possible.

Of course, mathematicians and policymakers did not communicate with each other directly. Rather, the idea of operations research grew in importance as it became a professional and conceptual conduit through which *insights* could pass between policymaking and mathematical worlds. Policymaking practices provided intriguing problems for mathematicians who were interested in defining what entailed an optimal or a rational response to a well-defined situation. OR gave them a ready source of such problems. The definitions that the mathematicians produced could, in turn, be compared with actual policies by mathematically skilled OR practitioners, and used to improve the logic of managers' policies. It was often not so much a question of modeling real policies and applying models directly to them, as it was seeing something in one place that was lacking in another, and translating knowledge from one intellectual forum to another in order to fill these gaps. Of course, this professional relationship was never actually spelled contractually. What really needs to be understood is the ways in which various individuals came to select OR as a preferred professional venue for insight exchange, because it was in no way predetermined that it *would* be a site for such exchanges.[3] To understand why this selection happened, it will be important to look to

[3] As I explain in the next chapter, the consciously invented term "management science" became another serious contender, and the two ideas have successfully, if rather awkwardly, shared the stage in America for over half a century. The econometrics profession, which was then in a state of transition, was another possibility, but an emphasis on pure economic theory tended to preclude the development of more applied problems with a limited amount of journal space available. See Melvin E. Salveson, "The Institute of Management Sciences: A Prehistory and Commentary on the Occasion of TIMS' 40th Anniversary," *Interfaces* **27** (1997): 74-85. Salveson was a student of Tjalling Koopmans, who was one of the most important forces behind the transformations in econometrics. For another perspective, see C. West Churchman, "Management Science: Science of Managing and Managing of Science," *Interfaces* **24** (1994):

the influence of specific individuals and institutions, but also to the ways in which seemingly discordant intellectual programs came to be seen as compatible.

In this chapter I will describe the intellectual merger between individuals interested in mathematical modeling and those interested in driving the operations research profession forward in several stages. In the first stage, I look at early efforts to expand the field of OR outside of the military as set against the background of a strong management consulting profession. OR's proponents, I believe, had a strong incentive to adopt a set of mathematical techniques as a public representation of OR. The simple idea of making policies explicit and subjecting them to rigorous scrutiny would not necessarily have been compelling to business managers. These managers had for decades structured their business activities so as to make them easily governable, and they regularly hired quantitatively literate management consultants to critique their practices in much the same way OR scientists had critiqued military planners during the war. OR needed to find a way to stand out against this background.[4] In the second stage, I show

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99-110. We will meet Churchman in the next chapter; as the first editor of *Management Science*, and a proponent of an expansive definition of "management science," he was dismayed at the flow of articles coming into the journal on account of the fact that they could not be published in *Econometrica.*

[4] An article in *Fortune* addressed the issue directly, suggesting that OR could actually be a threat to "old line" management consulting. See Herbert Solow, "Operations Research," *Fortune* **43** (April 1951): 105-122. I tend to believe that consulting was far more of a threat to the expansion of OR. In such a professional environment, it was beneficial to be able to point to a body of theory and call it the methodology of OR, which was clearly different from other activities. Indeed, in 1953, Philip Morse, as the retiring first president of the ORSA, declared, "We can already say: *Operations research is the activity carried on by members of the Operations Research Society; its methods are those reported in our JOURNAL.* Very soon this definition will be more instructive and convincing than any number of special explanatory articles and talks." See Philip M. Morse, "Trends in Operations Research," *Journal of the Operations Research Society of America* **1** (1953): 159-165, p. 159, emphasis in original. It was, however, by no means clear at that point that the journal would become largely dominated by theory. It is important to remember that no wartime operations researcher wanted OR to be defined through a specific set of techniques. Philip Morse and his second wartime research director George Kimball's widely-read postwar report, "Methods of Operations Research," took great pains to point out that the real worth of OR was in the spirit of scientific inquiry, not in specific methods. Nevertheless, it was hard to ignore the fact that the bulk of that same report was devoted to explaining the various statistical and probabilistic methods that had been used in the ORG's work over the course of the war. When this report was declassified in 1947, it became a sort of informal OR textbook, until 1951, when it was published and became one formally. See

how the idea of using mathematical modeling to explore policies would not have been totally foreign to those whose wartime experiences had shaped their ideas about OR. Search theory had been used during the war to help operations researchers understand and evaluate naval tactics in ways not unlike those that employed far simpler quantitative arguments. These first two stage, however, simply portray OR as a venue amenable to methodological innovation, but do not suggest any reason why this innovation actually took place.

In the third stage, I shift to the perspective of the mathematician, and show how applied mathematicians took up problems as much for their formal suggestiveness as for the utility a solution to them could be expected to bring, and I show how many mathematicians proved adept at moving between abstract and applied realms. I concentrate particularly on two areas of mathematical innovation: the wartime genesis of the fundamentally significant technique of sequential analysis in quality control testing, and the intertwining of the analysis of Air Force logistics planning, the more trivial development of the Traveling Salesman Problem, and the linear programming calculative language. In the fourth stage, I show how the operations research and mathematical modeling worlds came together in venues such as the RAND Corporation, and around specific problems in industrial operations, most notably inventory and production modeling. The inventory and production problem is notable not only for providing OR with a triumph spanning the theoretical and practical realms, but for drawing the attentions of budding theorists of no less stature than Kenneth Arrow and Herbert Simon. Arrow and Simon, of course, are best known for their formal explorations of the

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Philip M. Morse and George E. Kimball, *Methods of Operations Research* (New York: The Technology Press of MIT and John Wiley & Sons, 1951). The original report was Operations Evaluation Group Report No. 54 and it has been made available online at <http://www.cna.org/documents/1100005400.pdf>.

fundamental nature of rationality, and, accordingly, I end the chapter with a short, speculative discussion of why two individuals who were helping to revolutionize the social sciences were also interested in the comparatively mundane problems of industrial operations.

Management Consultants, Mathematicians, and Operations Researchers

If OR had managed to create a reputation for itself during World War II by performing generalized studies of military plans and doctrine and selecting component problems for elucidation, this reputation was bound to be difficult to translate into industry. In America, the growing demand for generalized corporate research had already begun to be filled by groups of independent, quantitatively-literate advisors who had arisen out of the cost accounting profession. As Christopher McKenna has shown in his informative study of the history of management consulting, in the early years of the twentieth century, corporate managers had to negotiate intricate problems arising from overhead allocation, volatile demand, and depreciation of final and intermediary products. To solve these problems, McKenna informs us, "engineers in tandem with accountants created costing systems that not only distinguished between fixed and variable costs, depreciation, seasonal fluctuations, and variable labor rates, but also offered a way to compare costs between manufacturers within a single industry."[5] Even as early as the 1890s it had become difficult to discern certain branches of the engineering and accounting professions from each other. When Frederick Taylor began hocking his program of scientific management, cost accounting was a component of it, but it was

[5] Christopher D. McKenna, *The World's Newest Profession: Management Consulting in the Twentieth Century* (New York: Cambridge University Press, 2006), chapter 2, quote on p. 39. See also S. Paul Garner, *The Evolution of Cost Accounting to 1925* (Tuscaloosa: University of Alabama Press, 1954).

overshadowed by Taylor's famous time and motion studies. Later, when Taylorism collapsed in America amid a public relations debacle after the First World War, cost accounting was picked up by professional accountants such as Arthur Andersen, James McKinsey, Charles Bedaux, Charles Stevenson, Carle Bigelow, and J. P. Jordan, whose names would later be most associated with the consulting profession. These individuals had no commitment to the shop floor efficiency problems of Taylorism. Instead they concentrated their efforts on the bottom line problems of the boardroom. Their ongoing ties to the engineering profession, however, alienated them from mainstream accounting, and they established their own National Association of Cost Accountants in 1919.[6]

During the 1920s cost accounting professionalized into the new field called management engineering, which specialized in "financial investigations". At the heart of every financial investigation was the cost accountants' tool-of-the-trade, the "general survey," which examined the overall efficiency of a company's production and the soundness of its assets. Independent management engineering firms flourished in Chicago by investigating potential investments in the Midwest for the big banks back east. As cost accounting became increasingly an in-house activity, and executive concern shifted away from productivity issues and toward administrative organization and legal matters, management engineers proved willing to bend with the winds, and the field morphed into the more general format of management consulting. The profession was given a boost, when, following the anti-monopoly Glass-Steagall Act of 1933, investment services and banking were no longer permitted within the same corporate entity. As a consequence, the well-established Chicago consulting firms flourished, despite the Great Depression, by providing investment assessment services in all parts of the country on

[6] McKenna, *Newest Profession*, chapter 2.

behalf of the banks. They also began to help firms shore up their own strategic shortcomings. The profession grew precipitously, and by 1950 there were over one thousand management consulting firms in existence in America.[7]

Management consultants' financial investigations are comparable, in a broad sense, to wartime operations researchers' explorations of military practice and doctrine. When operations researchers decided that they could make a contribution to the problems of peacetime industry, they assumed that, taken into the confidence of industrial executives, they could find problems to investigate as easily as they had for the military. The non-scientist management consultants did not seem to enter their thinking. There were those, however, who at least foresaw general difficulties existing practices would present to any move toward industry. For example, when the Massachusetts Institute of Technology began considering establishing a program in OR in 1947, its administrators invited Patrick Blackett over from England to speak with them about it. Blackett had no illusions about any unique perspective that could be offered by OR, as he understood it. In his notes from the meeting are phrases such as "less application where trial and error has been in operation for a long time" and "good firms have always done it".[8] After returning to England Blackett wrote to Karl Compton, the president of MIT, that he found the discussion "interesting" and that he hoped "that the conclusions, though perhaps rather negative in character, were satisfactory" to them.[9] We will discuss MIT's further experience with OR more in the next chapter. For the moment suffice it to say that

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[7] *Ibid.*, chapters 2 and 3.

[8] "Notes, M. I. T. Discussion," January 1948, *PMSB*, PB/4/7/2/4/4 [formerly D.100].

[9] Blackett to Compton, 2/6/1948, *MIT*, AC 4, Box 32. Blackett had earlier written to Compton that he had opted out of efforts in the UK to extend OR into the postwar era, but that he was "of course very interested in a general way, in applying the lessons we learned during the War, to the social problems of peace."; Blackett to Compton, 12/11/1947, AC 4, Box 32. We will examine the tension between his opinion of OR and his interest in applications of the lessons of the war in chapter six.

Blackett was correct in suspecting that the kind of broad investigations that still defined OR for him already had a strong foothold in industry, but it is also true that OR's boosters were intent on pushing ahead.

In Great Britain in 1948, a number of OR enthusiasts established an Operational Research Club with restricted membership, led by its "convenor", Sir Charles Goodeve, who had been Deputy Controller of Research and Development at the Admiralty before becoming the director of the new British Iron and Steel Research Association after the war, and who was not an operational researcher in his own right. Although a few of the members of Britain's academic scientific elite who had been associated with wartime OR were members of the club, they were certainly not a driving force in postwar British OR. Rather, the OR Club was largely comprised of supporters of industrial modernization who had strong ties to the scientific and mathematical communities. They saw OR as an umbrella term encompassing fields such as quality control, market research, efficiency engineering, and, indeed, cost accounting, which were not covered under the traditional rubric of research and development.[10] In 1950, the club began publishing a journal called *Operational Research Quarterly* (*ORQ*), which initially concentrated on gathering examples of what the journal's contributors considered to be OR from scattered scientific and trade publications, and publishing the club's deliberations on such issues. In 1953, the club rechristened itself the Operational Research Society and opened its doors to a wider audience.

[10] See Maurice Kirby, *Operational Research in War and Peace: The British Experience from the 1930s to 1970* (London: Imperial College Press, 2003), chapter 11. For good examples of the perceived function of the journal, see "Marshalling and Queueing: Report of a Meeting of the Operational Research Club on March 17th, 1952," *Operational Research Quarterly* 3 (1952): 4-16, and also the wide variety of abstracts deemed pertinent to the club's business in every issue. See also Charles Goodeve, "Operational Research," *Nature* 161 (1948) 377-384.

In America, probably the most important torch bearer for OR during the immediate postwar years was the Princeton mathematical statistician Samuel Wilks. We will recall that in 1942 Wilks had been a part time member of the Navy's Anti-Submarine Warfare Operations Research Group before joining Warren Weaver's Applied Mathematics Panel, where he became an important liaison between the two groups. In 1947 Wilks assembled a pair of sessions on operations research at the year-end joint meeting of the American Statistical Association and the Institute of Mathematical Statistics. These sessions' speakers, however, were a heterogeneous selection of scientific individuals who had been involved in the war effort in general, not just in OR. The first session, chaired by Edward Bowles, featured talks by Jacinto Steinhardt of the Operations Evaluation Group and LeRoy Brothers of the Air Force's Operations Analysis Division.[11] The second session was chaired by the mathematician Merrill Flood, who was then Assistant Deputy Director of Research and Development on the Army's General Staff. It featured talks by former operations analysts Roger Wilkerson of Bell Telephone Laboratories and A. E. Brandt who was technical director of the Naval Ordnance Laboratory, E. S. Lamar of Steinhardt's OEG, and George Dantzig, who had been in the Combat Analysis Branch of the Army Air Forces' Statistical Control Section and was then working as a mathematical consultant for the Air Force Comptroller.[12]

The motley contributions to Wilks' sessions foreshadowed the interweaving of professional interests that would soon do so much to shape the future of the notion of OR, but at that moment that path was still not foreordained. In March 1948, cognizant of the

[11] The session also featured discussion by Arthur F. Brown of the OEG, Thomas I. Edwards of the OAD, W. J. Youden of Douglas Aircraft (formerly an operations analyst), and G. Baley Price of the University of Kansas (formerly an operations analyst).
[12] See materials in *ELB*, Box 52, Folder 4, especially a letter from S. S. Wilks to Bowles, 11/24/1947.

inability of OR to attain spontaneous coherence outside of its military context, Wilks circulated a draft of a proposal (presumably written by him) for a small conference to be convened under the auspices of the National Research Council's Division for Mathematical and Physical Sciences to discuss the meaning and significance of OR. The proposal observed that in its original military context the term (and its variants) referred "to the scientific study [of] the performance of men, equipment and weapons under combat conditions, with a view toward the improvement of their combined effectiveness against the enemy." It went on:

> More recently there has been some controversy in England and in the United States as to exactly what this work is and what are its implications for peace time application. Are its peace time applications in industry and business essentially any different from activity carried out by industrial and business management under different names? If so, what are the differences?

He noted that there had been more public discussion of OR's significance in England than in the United States (a subject to which we return in chapter six), and that it was important to work these issues out.[13]

This first NRC meeting was held in May 1948, and a second exploratory meeting was held in June 1949 (after the foundation of the Operations Research Office and the Weapons Systems Evaluation Group) to discuss what was to be done. I have found no record of the 1948 meeting,[14] but the second meeting was attended by Wilks; Philip Morse, then head of the National Military Establishment's WSEG; Steinhardt; Ellis Johnson of the Army ORO; the University of Chicago mathematician Marshall Stone; Alan Waterman, the research director at the Office of Naval Research and a wartime head

[13] Letter from S. S. Wilks to Edward L. Bowles, 3/15/1948, and attached proposal, *ELB*, Box 35, Folder 8.
[14] The record is not available in the National Research Council's archives, although a letter from R. C. Gibbs, the Chairman of the Division of Mathematical and Physical Sciences, to Detlev Bronk, then head of the NRC, 10/17/1949, refers to "two exploratory conferences" indicating that the 1948 meeting did take place; *NRC*, "PS: Com on Operations Research, 1949-1949, Beginning of Program" folder.

of the Office of Field Services; John Coleman, who was a member of the NRC's Committee on Undersea Warfare; and R. C. Gibbs, the head of the Mathematics and Physical Sciences Division.[15] The meeting also included Arthur A. Brown and Horace Levinson.[16] Levinson, who was retired and living on a farm in Maine, had received a Ph.D. in mathematical astronomy from the University of Chicago in 1922 and taught mathematics at Ohio State University before heading research departments at Bamberger's and Macy's department stores. He had written two books: one on general relativity metrics, and the other on applications of probability.[17] Brown, who was the deputy director of the OEG, had received his Ph.D. in mathematics from Princeton in 1940 and had worked at Bamberger's with Levinson before becoming a member of Morse's ASWORG. He was presumably responsible for bringing Levinson on board.[18]

Discussion in the second meeting varied widely over the scope of what OR was, whether it had applications in industry, how this activity could be initiated, where personnel would come from, and whether a pamphlet should be published to define what OR was. Among the participants, Wilks appeared the most concerned about existing analogs. However, the ORO's Ellis Johnson was unimpressed by what he viewed as industry's piecemeal advances. To him, OR was something that by definition permeated

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[15] It is unclear if Coleman had a more important connection to OR. Waterman had taken over the OFS after Karl Compton became the head of the abortive Pacific Branch of the OSRD.

[16] There were two men named Arthur Brown in the ORG. Arthur W. Brown was an actuary.

[17] Horace C. Levinson and Ernest Bloomfeld Zeisler, *The Law of Gravitation in Relativity* (Chicago: University of Chicago Press, 1931); and Horace C. Levinson, *Your Chance to Win: The Laws of Chance and Probability* (New York: Farrar & Rinehart, 1939), which was soon to be revised and expanded as Horace C. Levinson, *The Science of Chance, from Probability to Statistics* (New York: Rinehart, 1950). Levinson was also employed by a mail-order house in Chicago. On some research problems from his career, see Horace C. Levinson, "Experiences in Commercial Operations Research," *Journal of the Operations Research Society of America* **1** (1953): 220-239.

[18] Biographical details from letter from Gibbs to Bronk, 10/17/1949. According to John Magee of Arthur D. Little, Brown had been employed by the University of Maine where Levinson's brother was a professor of philosophy. It was he who had introduced Brown to Levinson; interview conducted by William Thomas, 4/11/2005, Concord, Massachusetts.

an organization's decision making. He gave credit to some organizations such as Bell Telephone, but was dismissive of a general culture that made decisions "by having a conference and then making a decision without much investigation – an arbitrary handling of a situation."[19] Levinson seemed to agree with Johnson, stating: "There is no broad planning. It is generally thought enough to have an accountant's picture of the business. In ordinary business, figures are accumulated over a period of a year and that picture satisfies a man in charge." Nevertheless, he did take an accountant's perspective, observing, "The present day businessman is caught in a squeeze between the pricing problem and mounting costs. One solution is a more efficient operation of business. Labor waste can be as high as 30%." The participants seemed to settle on an agreement that OR might serve as a sort of a rallying flag to unite scattered efforts in a drive toward the modernization of business decision making through in-depth analysis. At the end of the meeting, Levinson, who had the only real corporate experience among the participants, was given the chair of a new NRC Committee to promote OR to industry.[20] This committee was a progenitor of ORSA, which would be founded a few years later.[21]

The NRC Committee, being heavily populated by representatives from military OR and veterans of the wartime effort, remained committed to promoting the very general view of OR familiar from the wartime experience, which they struggled to define in terms of the use of scientific method and the position of an OR team within the

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[19] Johnson was even more dismissive of the British who he felt had "just discovered time and motion studies."

[20] "Minutes of the Meeting on Operations Research held at the National Research Council, June 22, 1949," *PMM*, Box 9, "National Research Council" folder.

[21] The NRC committee had appointed a subcommittee consisting of Philip Morse and Jacinto Steinhardt, and steps were taken outside of NRC auspices to establish ORSA; see letter from Horace C. Levinson to R. C. Gibbs, 5/6/1953, *NRC*, "PS: Com on Operations Research 1950-1954, General" folder.

executive structure of an organization.[22] However, the links between these people and major figures in the applied mathematical communities at places such as Wilks' high-powered mathematics department at Princeton University, and the cauldron of theoreticians and analysts at the RAND Corporation—not to mention the many informal connections fostered by the late war—created an environment ripe for the interweaving of research commitments. When ORSA began publishing its journal late in 1952, it was a site fit for the dissemination of mathematical work that did not quite fit the more obviously abstract character of the problems treated in statistical and economics journals. Before the ORSA journal could begin playing this role, however, mathematicians first had to identify their intellectual agenda with that of OR.

The Wartime Precedent of Search Theory

In order to understand the compatibility of the tradition of wartime operations research with mathematical modeling, it is first crucial to grasp that there were strong similarities in their intellectual projects that would have been familiar from their work prior to the time when their alliance began to redefine OR in the early 1950s. As we have seen in the first three chapters, operations research sought to improve military planning and technological development by making the rationale behind planning, doctrine and technique explicit, and then clarifying areas where decisions seemed to be based on arbitrary assumptions, and correcting areas where the rationale proved incorrect in the face of collected evidence. This process bears a certain resemblance to the project of

[22] A transcript of a 1951 conference on OR at the University of Illinois demonstrates the painful contortions operations researchers were obliged to perform to attempt to make what they did sound coherent outside of its wartime context; see "Proceedings of the Conference on Operations Research held at the University of Illinois," 9/27/1951, *NRC*, "PS: Com on Operations Research 1951; Conference on Operations Research Sep Proceedings" folder.

mathematical model building, which makes the logical consequences of sets of stated axioms explicit. Theoreticians hoped that if models could be made sophisticated enough, they could be manipulated and used to derive better strategies than those achieved by more codified doctrinal approaches.

As we have seen, mathematical modeling was used constantly in non-OR environments, such as in equipment design workshops, and in mathematical service groups such as the Applied Mathematics Panel and the British Air Warfare Analysis Section for just such a purpose. By comparison, it was used only rarely within actual OR groups, but the development of search theory in the Navy's ORG was a clear exception to this rule. In its essence, search theory was an attempt by mathematically-trained individuals to make the rationale implicit in Navy search tactics explicit. The first official document ever produced by ASWORG in May 1942 stated outright that the purpose of developing a mathematical theory of search was "to clarify the minds of the Operational Research Group on this problem." It went on:

> The only way a theoretical subject of this sort can be attacked is to set down a series of preliminary hypotheses, which can then serve as a basis for discussion and modification. It is to be hoped that if the comments are read by the various Naval A/S/W [Antisubmarine Warfare] Units they will be considered in this light. Suggestions, particularly contradictory opinions, by members of these A/S/W Units, will be particularly valuable. Only in this manner can the O/R/G modify its hypotheses to come closer to actual cases.[23]

Once the theory had explicitly replicated the wisdom arrived at through more traditional military heuristics, it might prove of some use in actual tactical development.

By the summer of 1943, ASWORG's own heuristic process had developed the theory of search considerably. On August 10th of that year, William Shockley,

[23] "Preliminary Report on the Submarine Search Problem by the A/S/W Operations Research Group, Section C-4, N.D.R.C.," 5/1/1942, *NACP*, RG 38, Records of the Anti-Submarine Measures Division, Tenth Fleet [A1/349], Administrative Files, Box 3.

ASWORG's first research director, gave a presentation to one of the regular conferences held on antisubmarine warfare entitled "Simulated Hunts for U-boats". Its subject was the use of radar to "exhaust" a U-boat. By using enough aircraft to patrol an area in which a previously-seen U-boat was certain to be continually, hunters could guarantee that a U-boat would be spotted by radar when it resurfaced. If it was not attacked, it would at least be forced to submerge again. This process could, in theory, be continued until the U-boat's batteries and air supply were exhausted from underwater travel and it was forced to the surface where it could be easily killed. The trouble was that the number of aircraft supposedly needed over the course of the hunt to guarantee a sighting at the time of resurfacing was about 15, and, if the estimate of the initial position of the U-boat was off by as little as 25 miles, many more aircraft would be needed.[24] Typically, though, only as many as three aircraft were available for such a hunt, which was clearly insufficient to guarantee success. However, for less than a guaranteed contact, the group discovered it was not possible to calculate the probability of spotting a U-boat given search theory alone. It was necessary to take into account different kinds of tactics employed by U-boats and their hunters. "After all," Shockley observed, "a U-boat hunt is not a geometrical problem, but a sort of game, with opposed intelligences on either side."[25]

[24] Assume a U-boat can travel at 3 knots submerged for 25 hours and that a search plane can travel at 120 knots with a search radius of 5 miles, leading to a sweep rate of 1200 miles^2/hour. Assuming an efficient sweep, it would be necessary to log 125 flying hours in the area, and toward the end of the hunt 15 aircraft would be required to patrol at once to make certain the whole area was swept once per hour. If the initial position estimate was off by 25 miles, 290 flying hours would be required, with 27 aircraft required near the end of the hunt.
[25] "Digest of minutes, conference on anti-submarine warfare, held at the Headquarters of the Commander in Chief, United States Fleet," 8/10/1943, Enclosure C: Dr. Shockley, "Simulated Hunts for U-boats," *NACP*, RG 38, Records of the Anti-Submarine Warfare Analysis and Statistical Section, Tenth Fleet [A1/350], Series II, Administrative Files, Box 47, "ASM Conferences (2)" folder. See also a brief discussion in Morse, "Trends," pp. 163-164.

While two members of ASWORG had been in England, they had had the opportunity to interrogate three British submarine captains, two of whom had commanded the captured U-boat rechristened *H.M.S. Graph.*

> A highly idealized exhaustion hunt plan was laid out on paper and each submarine commander was asked to describe his tactics subsequent to a sighting. On the basis of the tactics initially adopted, the observations of aircraft which the submarine might make were worked out from the flight plan and were told to the officers. On the basis of this information they then revised their plans. This procedure was continued until the period of time allotted to the hunt was covered.

Even though the search was designed to sweep the necessary area twice per hour (supposedly guaranteeing contact), the U-boat still had a good chance of escaping, because, knowing they were being hunted, "the regularity of the plan [...] permitted the U-boat to surface and dive on schedule between aircraft transits." On the basis of what was learned from these crude simulations, new formulations of the problem were worked out back in Washington. "These were to be made as realistic as possible." Taking into account the fact that U-boats can spot an aircraft and submerge without notice about twice as often as aircraft spot U-boats first, a new simulation was set up:

> The U-boat commander plots on tracing paper his position at the end of his first 30 minutes of travel and at the end of the first hour. Similarly the A/C [aircraft] squadron commander plots the position of each A/C at the end of each 10 minute interval, during the first hour. These two charts are turned over to a referee, who compares them to see whether or not a sighting is possible. If one is possible he decides whether or not it occurred by doing the equivalent of flipping a coin. If, during the hour, either commander made a sighting, he is so notified by the referee and permitted to alter his plans if he so desires. This procedure is repeated for each hour of the hunt.

These kinds of simulations actually took an amount of time comparable to a real hunt, and it was decided to carry out the investigation "theoretically along mathematical lines, based on the results of the few hunts actually run."[26]

One promising result of this investigation was the development of a tactic called the "continuous gambit" in which aircraft would patrol just *outside* the area where a U-

[26] Shockley, "Simulated Hunts for U-boats".

boat could possibly be. Not sighting an aircraft through a periscope, the U-boat would be tempted to surface and run with greater speed, but quite possibly into the patrol area. It was decided to expedite the simulation of such hunts using a device that ASWORG designed (and constructed by the Special Devices Section of the Bureau of Aeronautics) that "dispenses with the referee and automatically introduces the probabilities of sighting by the aircraft and the U-boat" and that let the other side see what the other was doing only in the event of a sighting. "The games," Shockley pointed out,

> should prove of value in other respects than testing hunt plans. In particular, other general tactical conclusions, like that embodied in the continuous gambit, may be discovered; hunts in special areas having restricted waters and shallow bottoms may be studied; and the effect of navigational errors and hence the value of navigational aids may be investigated.

He concluded by observing that the continuous gambit was already employed by the Fourth Fleet using only a few planes, and that it had obtained some successes in reacquiring contacts.[27]

In Shockley's presentation, the tactics already derived by military officers served as a criterion against which the contributions of ASWORG could be measured. Officers knew that simple geometric models did not take into account the intelligent behavior of an enemy that could anticipate and avoid a search constructed only around geometric principles. It is possible to recognize the rudiments of John von Neumann and Oskar Morgenstern's game theory—which would not be published in its full form for another year—in the insights gained from communication with the officers. Any minimax mixed strategy solution to a game theoretical problem must eschew regularity. (Distributing your strategy evenly in a game of paper, rock, scissors, for instance, will do you no good if you always choose among them in a fixed order.) It is also possible to recognize the

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[27] Ibid.

use of war games featuring mathematical rules to test how principles offering no clear solution can arrive at workable strategies by studying patterns created through mathematical *design* rather than calculation. In this case, the "continuous gambit" demands that U-boat captains risk moving on the surface because, if they understand the tactics being used against them, even though they know they might be spotted, they also know that they will *never* sight an aircraft regardless of whether or not they are safe in moving forward, thus forcing them to take the risk. In this way, mathematical exercises could both validate and improve the rationale behind tactics arrived at through more intuitive means. The formulation of the mathematical exercise itself, however, was usually dependent on guidance from those familiar with the intricacies of the problem.

Calculations and Contexts; Solutions and Efficient Solutions

The compatibility of doctrine and mathematical modeling stems ultimately from judgments of the efficiency of choice. Because every non-arbitrary choice must have a rationale, whether implicit or explicit, it behooves the modeler to understand the rationale behind existing policies as well as behind their own models so as to understand why those policies might actually be superior to the carefully considered model. The difficulty arises when one attempts to use rationality to discover more effective ways of making a choice. Experience allows one to root out ineffective solutions, and to obtain better ones systematically. The notion of attacking a problem systematically, however, is not clear cut. A systematic attack might rely on highly intuitive means, or it might rely on highly formalized generalizations of the problem. However, intuition and formalization are not mutually exclusive approaches. Intuitions frequently disguise some unarticulated rationale, while formalized treatments are still only rules of thumb that are more or less rigorous. Thus, the act of representation of a problem in an abstract domain, such as mathematics, is by necessity, and despite reputation, a craft.

The practice of the mathematical craft depends on the mathematician's appreciation of the requirements of the problem. Only very simple or well-controlled problems readily admit rigorous solutions. Solving problems of any real complexity must entail radical simplifications of the problem, and invoke what we might call an economics of calculative effort. If a rigorous solution to a problem might be expected to yield substantial improvements over more arbitrary solutions, it will be worthwhile to make the rigorous calculation. On the other hand, if calculation can only be expected to offer marginally better solutions, simplified or even intuitive solutions might be allowed to suffice. Mathematicians also must give some thought to whether or not they themselves are finding the least laborious way to calculate a solution of sufficient rigor, or whether better craft might yield more efficient means of calculation. Of course, the perceived importance of the problem as well as the degree of improvement expected through calculation weigh on the nature of the calculation to be performed, and appraisals of the expected benefit of calculation, in the end, rely on the experience and intuition of the mathematician. Such *meta-calculative* concerns were a mark of postwar mathematics, statistics and especially mathematical OR, and were clearly shaped by the context in which the mathematical effort was performed.[28]

[28] Lambert Williams, "Cardano and the gambler's habitus," *Studies in the History and Philosophy of Science* 36 (2005) 23-41, points out that what I am calling meta-calculative concerns were implicit in Girolamo Cardano's sixteenth century treatments of probability calculations in his *Book on Games of Chance*. Williams argues that the book should not be read as an imperfect version of later theories of probability, such as those put forward by Blaise Pascal, but as an actual practical discussion of gambling where rough calculations must be made in short time spans and under heavy psychological pressure. The move from Cardano's approach to more fundamentalist approaches to probability theory, and back to meta-

Mathematicians can achieve economies of calculative effort within a context through some combination of making their own simplifying assumptions with respect to the demands of the context, and using analysis to improve the efficacy of the simple calculations made by non-mathematicians within that context. The context of World War II shined an especially bright spotlight on the economy of calculative effort. In previous chapters, we have seen how mathematicians often employed OR as a bridge between their work and the context of their work to improve the impact of the work they did. We have also seen how mathematicians produced intermediary technologies to help non-mathematicians make sophisticated calculations with a minimum of actual calculative effort, either by honing their instincts through improved training regimens, or by providing them with calculating devices such as slide rules and tables.[29] In this chapter, we are mainly discussing abstract classes of mathematical problems adhering to a related notion of economy of calculative effort: exploring what *defined* an efficient design of certain routine calculative activities, such as devising search patterns for U-boats.

As a simple concrete illustration of this concept of efficient calculative design, consider the following problem worked on during lunch breaks at the Statistical Research Group (SRG) at Columbia University:

> Given 12 coins, all of the same weight except one, and using only a two-pan balance, find the odd coin and determine whether it is heavier or lighter than the others, *making only three weighings*.

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calculative issues, such as those present in sequential analysis (to be discussed presently), deserves additional study. Samuel Wilks, incidentally, wrote the foreword to the 1961 edition of Cardano's book: Samuel S. Wilks, "Foreword," in *Book on Games of Chance (Liber de ludo aleae)*, by Girolamo Cardano, translated by Sydney Henry Gould (New York: Holt, Rinehart, and Winston, 1961), pp. iii-iv.

[29] To avoid inappropriate reifications of choice in a training regimen, calculating device, or rule of thumb, mathematicians, as the individuals most familiar with the simplifications that have been made, must also provide the users of calculating devices instruction in the circumstances for which the device's calculations will remain useful, and when more intuitive solutions might provide a better guide.

One could easily find and determine the weight of the odd coin using more than three measurements, but if it is important to find an efficient solution, such as if one has to find so many odd-weighted coins in one day, one must pay much more attention to how much information can be obtained from each measurement, including information that can be gleaned about coins not even in the balance, and information that is contingent on the results of future measurements.[30] The SRG mathematicians would go a step further and try to understand the principles underlying the efficient solution. This particular problem occupied some of the most brilliant mathematical minds in the country for several weeks' worth of lunch breaks before one of them arrived at a generalized solution to the class of problem represented.[31]

This trivial problem has a certain similarity to what is considered to be the greatest mathematical triumph of the war, the development of sequential analysis by the economist Abraham Wald, who was working at the SRG at Columbia.[32] Sequential analysis was developed to make quality control inspections more efficient. If one wants to know the quality of a production lot, one has to develop certain standards of acceptability. Then one tests a certain sample from a lot and records what percentage of the sample meets the standard. If this percentage exceeds a certain predetermined percentage, the lot is retained, and if it does not, the lot is scrapped. According to statistical theory before sequential analysis, one would only need to continue testing until one had determined that the lot had either passed or failed the test. As an illustration, if

[30] Actually trying the problem instills an appreciation in the non-mathematician for economy of calculative effort. I profitably spent the bulk of a train ride from Washington, DC to Charlottesville, Virginia working out the solution to the particularized problem. Obviously, generalization was beyond my ability.
[31] W. Allen Wallis, "The Statistical Research Group, 1942-1945," *Journal of the American Statistical Association* **75** (1980): 320-330, p. 329, emphasis added in problem description.
[32] The work was initially begun by the economists Allen Wallis and Milton Friedman, but was turned over to Wald when it became clear that the statistical theory would prove too formidable for them to handle.

80 out of 100 shells of ammunition chosen at random from a lot needed to pass inspection for the lot to be accepted, then as soon as 20 shells did not pass inspection, one could end the test without testing all 100 and scrap the lot accordingly. However, what if one had tested 83 shells and 79 had so far proven acceptable? Given the results of the first 83 tests, what would be the odds that, of the seventeen shells remaining, all seventeen would prove defective? Did one really need to proceed any further? Quality inspectors understood intuitively that they could stop testing the lot well before that 83rd shell, but, rationally, given certain well-defined tolerances of error, at what point could one actually stop? Economy of action was crucial during the war when many lots had to be inspected at minimal cost in spent ammunition and time, and the need to design efficient tests that still ensured quality prompted the need to find an answer to this question for the first time in history.[33]

The impact of war and other external contexts on the development of more academic mathematical topics, such as sequential analysis, has received increasing attention from historians of mathematics,[34] but I would like to press a somewhat stronger

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[33] The formulation of this problem is simplified considerably here for easy digestion. Selection of the sample size from lot size in sequential analysis was determined as one continued a sequence of tests. When Allen Wallis and Milton Friedman (who would go on to become one of the most influential economists of the twentieth century) first attacked the problem, they were not convinced that a sequential test would not actually result in *larger* sample sizes. Sequential analysis proved to be an extremely suggestive field of study: Abraham Wald's 1947 book on the subject was over 200 pages long, and it was far from the final word on the topic; see Abraham Wald, *Sequential Analysis* (New York: John Wiley and Sons, 1947). On the history of sequential analysis, see Wallis, "Statistical Research Group," pp. 325-328, and especially Judy L. Klein, "Economics for a Client: The Case of Statistical Quality Control and Sequential Analysis," in *Toward a History of Applied Economics*, edited by Roger E. Backhouse and Jeff Biddle (Durham: Duke University Press, 2000), pp. 27-69.

[34] In addition to Klein, "Economics for a Client," see Tinne Hoff Kjeldsen, Stig Andur Pedersen and Lise Mariane Sonne-Hansen, "Introduction," in *New Trends in the History and Philosophy of Mathematics*, edited by Tinne Hoff Kjeldsen, Stig Andur Pedersen and Lise Mariane Sonne-Hansen (Odense: University Press of Southern Denmark, 2004). They cite several pertinent sources: Amy Dahan-Dalmedico, "L'essor des mathématiques appliquées aux Etats-Unis: L'impact de la seconde guerre mondiale," *Revue d'histoire des mathématiques* **2** (1996): 149-213; Moritz Epple, "Topology, Matter, and Space, I: Topological Notions in 19th-Century Natural Philosophy," *Archive for History of Exact Sciences* **52** (1998): 297-392; Moritz Epple, "Genies, Ideen, Institutionen, mathematische Werkstätten: Formen der Mathematikgeschichte,"

point, which is that the war was one of *many* contexts working *simultaneously* on the development of a network of interrelated mathematical issues. Mathematicians were not simply spurred into new fields by encounters with particular classes of practical problems such as the quality control of ammunition. They could sometimes find fundamental insights into problems bearing on a wide variety of contexts, some of which could be quite abstract, and some of which could seem to be quite mundane. The ability of mathematicians to shuttle rapidly between contexts does not mean that mathematical work can transcend context, exactly, but rather that problems can take on new hues when seen in different lights. What might seem a very arcane issue can offer insight into the meta-calculative issues surrounding a wide variety of issues of relevance to mathematicians and non-mathematicians alike, depending on the mathematical tools already available and the range of problems to which mathematicians have been exposed.[35]

To demonstrate this point, I would like to continue this exploration of the nature of problems and efficient solutions by examining the relationship between a fairly frivolous problem developed prior to the war called the Traveling Salesman Problem (TSP) and the clearly utilitarian development of linear programming just after the war.

Mathematishe Semesterberichte **47** (2000): 131-163; and Tinne Hoff Kjeldsen, "A Contextualized Historical Analysis of the Kuhn-Tucker Theorem in Nonlinear Programming: The Impact of World War II," *Historia Mathematica* **27** (2000): 331-361. See also Amy Dahan and Dominique Pestre, "Transferring Formal and Mathematical Tools from War Management to Political, Technological, and Social Intervention (1940-1960)," in *Technological Concepts and Mathematical Models in the Evolution of Modern Engineering Systems: Controlling, Managing, Organizing,* edited by Mario Lucertini, Ana Millán Gasca and Fenando Nicolò (Boston: Birkhäuser Verlag, 2004). Judy Klein's forthcoming book, *Protocols of War: The Mathematical Nexus of Economics, Statistics and Control Engineering,* is sure to be an essential resource, and closely related to some of the material presented here.

[35] This point is not unlike that pressed in Peter Galison, *Einstein's Clocks, Poincaré's Maps: Empires of Time* (New York: W. W. Norton & Company, 2003). Galison uses an analogy to "critical opalescence" to describe the simultaneous formulation of abstract concepts such as Einstein's special relativity and thinking about problems such as clock coordination. I hesitate to use the term here because we are not dealing with the creation of ways of thinking about the world, but the special role played by mathematics in translating between contexts.

Simply stated, the TSP is as follows: a traveling salesman must visit a certain set of cities, never visiting the same city twice, and returning, ultimately, back to where he began. Find the shortest possible route satisfying these criteria (Figure 4.1). Formally articulated, one must find a permutation $P=(1\ i_2\ i_3\ \ldots\ i_n)$ of the integers from 1 through n that minimizes the quantity

$$a_{1i_2} + a_{i_2i_3} + a_{i_3i_4} + \ldots + a_{i_n1},$$

where the $a_{\alpha\beta}$ are a given set of real numbers. "More accurately," the mathematician Merrill Flood wrote in 1956, "since there are *only* $(n$-1)! possibilities to consider, the

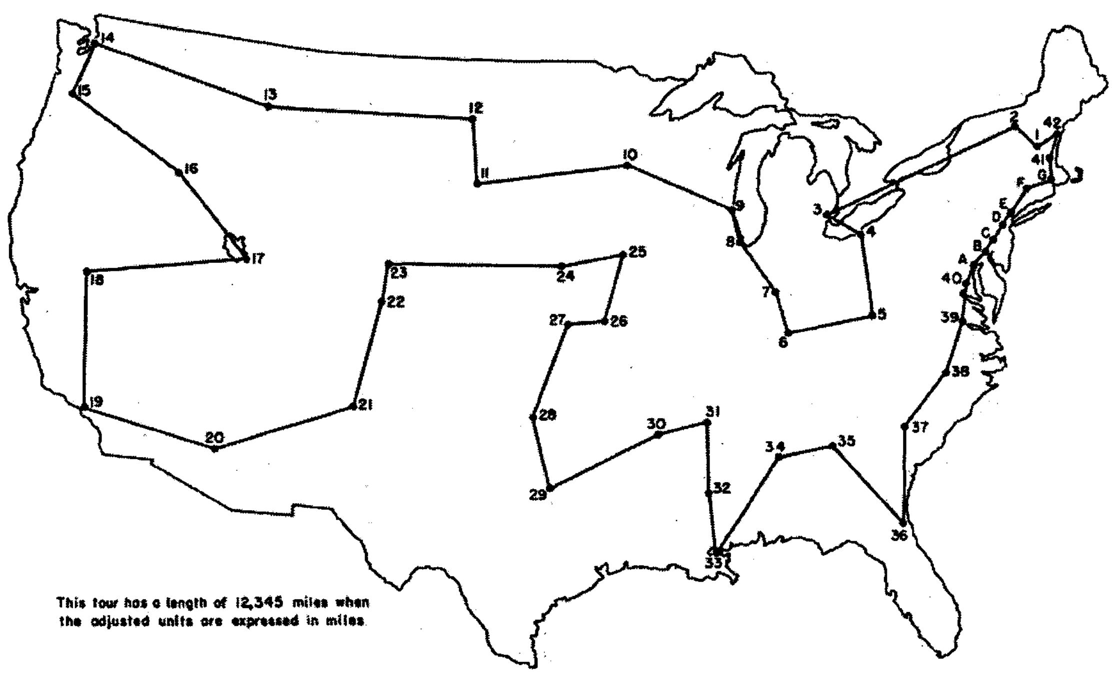

FIG. 16. The optimal tour of 49 cities.

Figure 4.1. An optimal solution to a Traveling Salesman Problem. Reprinted by permission, G. Dantzig, R. Fulkerson, and S. Johnson, "Solution of a Large-Scale Traveling Salesman Problem," *Journal of the Operations Research Society of America* **2** (1954): 393-410, p. 406. ©The Institute for Operations Research and the Management Sciences, 7240 Parkway Drive, Suite 310, Hanover, Maryland 21076, USA

problem is to find an *efficient method* for choosing a minimizing permutation."[36] One has not only to find an optimally efficient solution to the problem, but to do so via an efficient algorithm since the calculative effort was likely to be great. Meta-calculative issues abounded.

Flood had originally heard about the problem in the mid-1930s while he was a professor at Princeton University, and found himself ensnared by it when he was working on school bus routing problems in 1937.[37] While the TSP managed to garner only a little attention in the prewar period, in 1949 Flood moved to the RAND Corporation, where, apparently at the instigation of John Williams, he began soliciting real interest in the problem when the development of the optimization technique of linear programming began to make the problem more tractable.[38] Thus, in the early 1950s, the TSP became grouped with a number of other problems that had also become associated with the startling growth of linear programming. Notably, the "transportation" problem entailed finding the most economical transportation schedule of distributing goods from various points of supply to various points of consumption (Figure 4.2). This problem was mathematically related to the "personnel assignment" problem, which entailed assigning a given number of personnel with certain known skill sets to the same number of jobs in

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[36] Merrill M. Flood, "The Traveling-Salesman Problem," *Operations Research* **4** (1956): 61-75, p. 61; emphasis added—the sheer impossibility of calculating and comparing all possible permutations in a situation of any complexity was the very reason the problem was so vexing and required an efficient solution. Actually there are half this number, since backward routes are equivalent to forward routes.

[37] The exact origins of the problem are not deep, but they are obscure. According to Flood, both he and fellow Princeton mathematician Albert Tucker attributed their knowledge of the problem to a 1934 seminar given by Hassler Whitney, who subsequently did not recall knowing anything about it. See Flood, "The Traveling-Salesman Problem"; G. Dantzig, R. Fulkerson, and S. Johnson, "Solution of a Large-Scale Traveling-Salesman Problem," *Journal of the Operations Research Society of America* **2** (1954): 393-410, p. 410. A more comprehensive history is available in A. J. Hoffman and P. Wolfe, "History," in *The Traveling Salesman Problem: A Guided Tour of Combinatorial Optimization*, edited by E. L. Lawler, J. K. Lenstra, A. H. G. Rinnooy Kan, and D. B. Shmoys (New York: John Wiley & Sons, 1985). Hoffman and Wolfe's essay offers other historical wrinkles that do not concern us here.

[38] Williams, it is reported, wanted to expand RAND's theoretical work beyond game theory, for which RAND was already a major center of work; Hoffman and Wolfe, "History," p. 6.

such a way that uses their talents most efficiently; and the "nutrition" problem (posited by the economist George Stigler in 1945), which demanded that one find the least expensive means of satisfying one's nutritional requirements given the price and nutritional content of certain foodstuffs.[39]

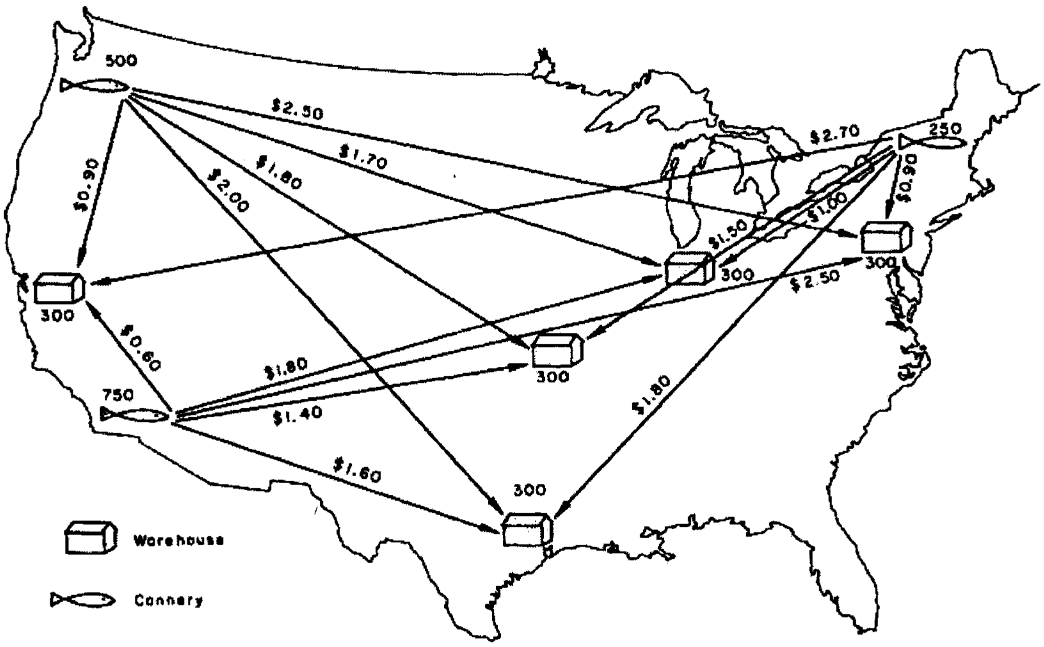

Figure 1-2-I. The Problem: Find a least cost plan of shipping from canneries to warehouses (the costs per case, availabilities and requirements are as indicated).

Figure 4.2. One example of the transportation problem. Source: George B. Dantzig, *Linear Programming and Extensions* (Princeton: Princeton University Press, 1963). © The RAND Corporation, reproduced by permission of Princeton University Press.

Linear programming itself originated in the postwar work of the mathematician George Dantzig on the logistical problems surrounding the organization of Air Force mobilization plans called "programs". While Dantzig had been the head of the Combat Analysis Branch of the Army Air Forces' Statistical Control Unit, he gathered and

[39] The nutrition problem (also known as the "housewife's" problem) differed in that there were no limits to the amounts on each kind of food that could be incorporated into the cheapest diet. In the assignment problem, exactly one person had to be assigned to each job; see George B. Dantzig, *Linear Programming and Extensions* (Princeton: Princeton University Press, 1963), especially chapter 1.

processed tremendous amounts of data relating to the sorties flown, targets attacked, bombs dropped, aircraft lost, and so forth, and he became quite familiar with the vast clerical infrastructure that the Air Forces had established to coordinate the even vaster logistical infrastructure behind bombing operations.[40] He had been privy to the 1943 creation of a "program planning" function in the Air Force under Edward Learned, a professor at the Harvard Business School, which aimed to make separate planning functions such as procurement, supply, training and deployment into a coordinated and self-consistent system under an overall war plan.[41] After the war ended, the Air Forces consolidated statistical control, program monitoring, and budgeting in the office of the Comptroller, and Dantzig was made a mathematical consultant to the Comptroller and tasked with systematizing program planning, and mechanizing the process using a digital computer. Dantzig, working under his fellow statistical control alumnus Marshall Wood, gathered a group (dubbed Project SCOOP[42] in 1948) to attempt to work out ways not only to formulate coherent programs—a difficult enough task—but also to compare any of the countless different possible configurations of programs to find more efficient ones. Taking a cue from Harvard economist Wassily Leontief's prewar "input-output" model of the American economy, Dantzig and his group at Project SCOOP worked out a matrix

[40] Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (New York: Cambridge University Press, 2002), p. 256, improperly identifies Dantzig as "an operations researcher for the Army Air Forces".
[41] This system, alternatively known as "program control", "program planning" and "program monitoring" is briefly described by Dantzig in *Linear Programming and Extensions*, p. 14. It is covered in much greater detail in David Hay, "Bomber Businessmen: The Army Air Forces and the rise of statistical control, 1940-1945," Ph.D. dissertation, University of Notre Dame, 1994, pp. 99-112.
[42] SCOOP stood for "Scientific Computation of Optimal Programs".

formulation for combining staged program elements of an overall war plan (a World War II-era program is shown in Figure 4.3) as a system of linear equations and inequalities.[43]

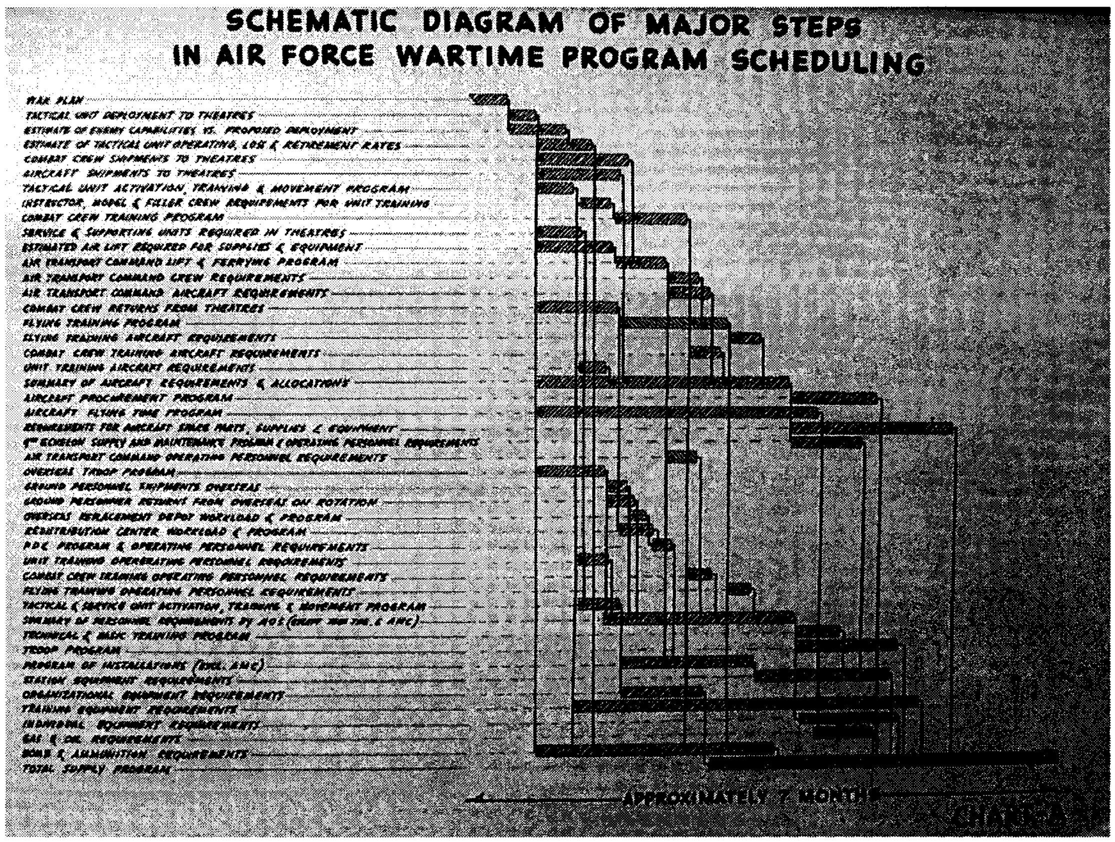

Figure 4.3. A wartime program showing the interrelation of different steps in the fulfillment of an overall plan. Source: Project SCOOP, Report 4; obtained by courtesy of Saul Gass. This figure is also reproduced in Marshall K. Wood and Murray A. Geisler, "Development of Dynamic Models for Program Planning," in *Activity Analysis of Production and Allocation*, edited by Tjalling C. Koopmans, 7th printing (New Haven: Yale University Press, 1971 [1951]), pp.189-215.

Once satisfied that programming activities could be adequately formulated as a series of linear equations and inequalities, Dantzig set out to find a way of solving the system efficiently, which led him to visit the economist Tjalling Koopmans at the Cowles

[43] On the history of linear programming, see Dantzig, *Linear Programming*, pp. 12-29; George B. Dantzig, "Reminiscences about the Origins of Linear Programming," *Operational Research Letters* **1** (1982): 43-48; an interview with Dantzig by Donald Albers, "George B. Dantzig," in *More Mathematical People: Contemporary Conversations*, edited by Donald J. Albers, Gerald L. Alexanderson, Constance Reid (San Diego: Academic Press, 1990). See also Tinne Hoff Kjeldsen, "Different Motivations and Goals in the Historical Development of the Theory of Systems of Linear Equalities," *Archive for History of Exact Sciences* **56** (2002): 469-538, pp. 522-530; and Mirowski, *Machine Dreams*, pp. 256-259.

Commission for Research in Economics in Chicago in the summer of 1947. Koopmans had formulated a version of the transportation problem while working on shipping schedules for the British-American Combined Shipping Adjustment Board and the British Merchant Shipping Mission in Washington during the war, but had since turned toward more fundamental research in economics.[44] Recognizing his wartime formulation of the static transportation problem in the formal structure of Dantzig's time staged (i.e. dynamic) linear model, Koopmans became deeply interested in the problem as a means of reformulating recent advances in economic theory. However, he did not have the answer to Dantzig's immediate problem. So Dantzig employed his own knowledge of convex functions to work out a means of arriving at a solution on his own that soon became known as the simplex algorithm.[45]

In the wake of the simplex algorithm, theoretical work on what had become known as linear programming expanded rapidly and into a remarkable range of application, from very mundane problems such as gasoline blending to abstract research in the properties of convex sets and neoclassical economic theory. Project SCOOP, of

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[44] Koopmans had been a student of physics under Hans Kramers, but was largely advised by Jan Tinbergen after switching his emphasis to economics. He emigrated to the United States in 1940 with the aid of Merrill Flood—who also got him interested in the TSP. On the relationship between Koopmans, Flood and the TSP, see interview with Merrill Flood, online at: <http://libweb.princeton.edu/libraries/firestone/rbsc/finding_aids/mathoral/ pmc11.htm>, also cited and quoted in Mirowski, *Machine Dreams*, pp. 257n35, 262-263n39. See Tjalling C. Koopmans, "Exchange Ratios between Cargoes on Various Routes (Non-Refrigerating Dry Cargoes)," in *Scientific Papers of Tjalling C. Koopmans* (New York: Springer-Verlag, 1970), pp. 77-86. The problem was developed independently by Frank Hitchcock, and was widely known as the Hitchcock-Koopmans Transporation Problem. On his personal history with the transportation problem, see Tjalling C. Koopmans and Stanley Reiter, "A Model of Transportation" in *Activity Analysis of Production and Allocation: Proceedings of a Conference*, edited by Tjalling C. Koopmans, 7th printing, (New Haven: Yale University Press, 1956 [1949]), pp. 222-259.

[45] Before Dantzig and his group understood the algoritm's remarkable efficiency, they searched the literature for a problem on which to test it, and found Stigler's 1945 article on the (non-dynamic) nutrition problem in the *Journal of Farm Economics*; see George J. Stigler, "The Cost of Subsistence," *Journal of Farm Economics* **27** (1945): 303-314. Over the course of 120 man-days worth of number crunching on hand-operated mechanical calculators in the fall of 1947, Dantzig's group found the optimal solution.

course, continued to work on finding better ways to program Air Force logistical activities.[46] RAND became a major supporter of work on linear programming, hiring Dantzig away from the Air Force in 1952. Meanwhile, the mathematician John von Neumann immediately recognized the mathematical equivalence of linear programming with two-person game theory when Dantzig presented the problem to him, but neither published anything on the subject. Soon enough, however, the Princeton mathematician, Albert Tucker, and his students, Harold Kuhn and David Gale, reproduced the result independently, published it, and opened up important new areas of mathematical research in "duality" and "nonlinear programming" based upon this insight. Tjalling Koopmans' use of linear programming to develop fundamental neoclassical economic theories would earn Nobel Prizes for him and others who followed him.[47]

In the early 1950s, Koopmans, and Julia Robinson of the RAND Corporation, began formulating linear programming approaches to the TSP, not because armies of traveling salesmen (or even fleets of school buses) were demanding optimized circuits, but because the constraints that the problem demanded made its formulation and solution particularly challenging and, therefore, enticing. Not only did all cities have to be visited, but they had to be visited in a cyclical order, and this cycle could not be comprised of sub-cycles or disconnected cycles, which, expressed formally, were feasible solutions to related problems such as the assignment problem. In 1954 Dantzig, working in John Williams' mathematics department at RAND, co-authored a seminal paper successfully applying linear programming to the solution of the TSP, which appeared in the second

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[46] See Saul I. Gass, "The First Linear Programming Shoppe," *Operations Research* **50** (2002): 61-68.
[47] See Hay, "Bomber Businessmen," pp. 305-314; Mirowski, *Machine Dreams*, chapter 5 on Koopmans' development of linear programming within a Walrasian economic framework; Kjeldsen, "Kuhn-Tucker Theorem"; and Kjeldsen, "Systems of Linear Inequalities".

volume of the *Journal of the Operations Research Society of America* (helping to cement the status of the journal as a forum for path-breaking mathematical research on problems of more interest for their theoretical than for their practical significance).[48] When Merrill Flood, now working at Columbia University after his stint at RAND, published a review of work done to-date on the TSP in 1956, it was also in the journal, which by then was simply—and significantly—called *Operations Research*.[49]

We will turn to the sudden and surprising appearance of OR in this story presently, but for the moment, I would like to remain with the question of context. The mathematician Tinne Kjeldsen has examined the origins of linear programming within a much broader history of the mathematical development of systems of linear inequalities, and has pointed out the sheer variety of contexts in which this retrospectively constructed history has taken place. Drawing on Henk Bos' discussions on the history and philosophy of mathematics, she contrasts the wildly differing motivations driving mathematicians undertaking various "tasks," some of which dealt with a direct interest in highly theoretical subject matter, some of which were "imposed" by outside agencies who had hired the mathematicians' services. In fact, Kjeldsen argues, it was the military utility of the problem that finally served to focus work on abstract systems in linear inequalities (which had until then been taking place in mathematical communities scattered through space and time) into a coherent research program.[50]

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[48] Dantzig, Fulkerson and Johnson, "Traveling-Salesman Problem".
[49] Flood, "Traveling-Salesman Problem".
[50] See Kjeldsen, "Systems of Linear Inequalities", pp. 530-532; and Henk J. M. Bos, "Philosophical Challenges from the History of Mathematics," in *New Trends in the History and Philosophy of Mathematics*, edited by T. H. Kjeldsen, A. S. Pedersen, and L. Sonne-Hansen, Odense: University Press of Southern Denmark, 2004, 51-66.

Kjeldsen is surely on the right track in pointing out the variability of motivation behind mathematical work in different mathematical communities working on conceptually related topics. I would only hasten to emphasize how mathematicians'

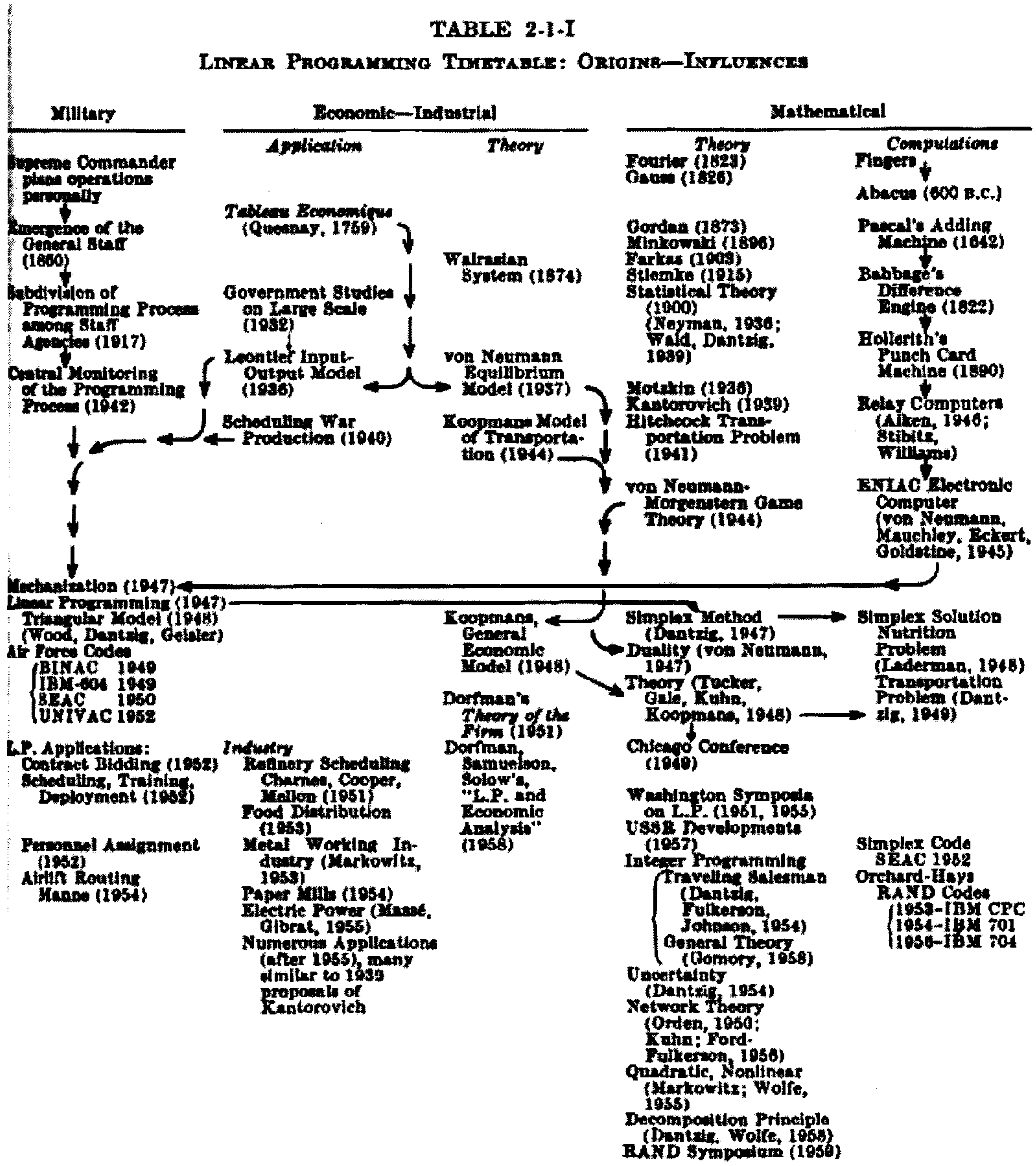

Figure 4.4. Dantzig's diagram of the history of linear programming. Notice the sudden and temporary disruption across categories that takes place immediately following the war. From George B. Dantzig, *Linear Programming and Extensions*. © The RAND Corporation, reproduced by permission of Princeton University Press.

motivations can vary markedly *within* mathematical communities and *within* very short time frames. Consider the history of linear programming offered by George Dantzig himself in his 1963 textbook on the subject. His account interwove several distinct strands of thought in the history of computation, mathematical theory, economic theory, economic analysis, and military organization that met at the moment of the creation of linear programming in the late 1940s.[51] The formal language of linear programming acted as a corridor to move with striking quickness between different contexts. Redrawing Dantzig's chart slightly, in the story told here, we move suddenly and chaotically between the development of shipping schedules, speculations about inexpensive diets in agricultural journals, high economic theory, the mathematical theories of linear inequalities and convex sets, and meditative objects situated between the practical and theoretical such as the Traveling Salesman Problem. It was precisely such translatability between contexts that explains why the OR community was able to develop an abstractly theoretical wing alongside its traditionally practical side, and why certain independently-generated mathematics, such as linear programming and the Traveling Salesman Problem, could soon be found in OR's mathematical canon.

The Development of an OR Theory Community

The simplest answer to why American mathematical theoreticians who worked on logistics techniques such as linear programming became integrated into the operations research rubric would be that a shared wartime experience between loosely related academic communities created a sense that peacetime applications of their general scientific skills should proceed under a united banner. Whereas in the military logistics

[51] Dantzig, *Linear Progamming*, chapter 2.

and operations are two clearly separate things; in industry, for all intents and purposes, logistics *are* the operations, meaning that, outside the military, mathematical logistics studies could be more easily integrated within the OR rubric.[52] Even so, this convergence of objects of study provided no clear impetus actually driving the two fields together. In Britain, for instance, the publication format of *ORQ* essentially reported on techniques that were recognized as belonging to fields such as applied statistics, but that were sure to prove pertinent to anyone with an interest in OR, that is, anyone with an interest in industrial modernization who happened, for one reason or another, to identify with OR. OR was not, however, seen as an intellectual rubric within which such techniques would be developed.

In America, the situation was initially regarded in a similar light. At the first full meeting of ORSA, Bernard Koopman was *reporting* on new mathematical methods pertinent to OR, but, at the same time, he was to an extent actually *equating* studies of operational policies with mathematics. He observed that "practical problems often *turn* on mathematical questions deep enough to go beyond existing knowledge and to require for their answer research into new mathematical fields." The problems that operations researchers so blithely studied by collecting data and organizing it according to an existing doctrinal framework could sometimes yield *entirely different* solutions if doctrines were translated into mathematical equivalents and subjected to a rigorous analysis. Here was the same principle that had begun to occupy the minds of equipment designers and mathematicians such as L. B. C. Cunningham, Warren Weaver, John Williams and Edwin Paxson during the war. We will recall that they distinguished the

[52] As observed in the last chapter, in the postwar Royal Air Force, logistics studies, such as the wartime planned flying and planned maintenance program were grouped under an entirely new category of "administration research".

analytical response to this problem, what they called combat theory or warfare analysis, from OR, which they saw as keeping analysis connected to pressing military needs. We will, however, also recall that warfare analysis and OR tended to spill into each other fairly easily on account of the mobile boundary between technology and tactics, and between the laboratory and the field. Koopman himself, of course, had worked on search theory under Weaver's umbrella before jumping under Philip Morse and George Kimball's at the Navy ORG at the beginning of 1944.

The foggy relationship between mathematical analysis and OR was renewed in postwar institutional arrangements. The RAND Corporation's primary project was to extend the techniques of equipment design into overall logistical *and* operational planning in order to select preferable weapons systems from a service-wide perspective. As we saw in the last chapter—even though RAND was not created with OR specifically in mind, and failed to integrate itself into the intellectual framework of military policymaking on its first attempt—its role as a civilian analysis organization, its frequent reliance on military OR for up-to-date military policy knowledge, not to mention the acquaintances with OR scientists made during the war, all served to associate RAND with the American military OR circle. As R&D policymaking and operational planning began to become more thoroughly integrated; operational, logistical and R&D planning only became more intertwined. Thus, RAND began to hire individuals such as Merrill Flood and George Dantzig to further its research in logistical design.[53] And thus, when members of the military OR community and veterans of the wartime OR groups began to move into the (logistics-laden) problems of industry in alliance with prominent mathematicians such as Samuel Wilks (who had worked with Flood at Princeton), it

[53] In 1953, the RAND Corporation would establish its own logistics department.

should come as no great surprise that individuals such as Flood and Dantzig joined Koopman among the ranks of those who saw the ORSA journal as a site for publishing path-breaking mathematical research.[54]

Once OR began to attract some abstract mathematical work in its journal, it only required a quorum of mathematicians to agree that OR would serve as a disciplinary identification for their work in order for the relationship to become perpetuated. I would argue that the members of this quorum decided to identify themselves with OR for two reasons. First, it offered a convenient gathering place for certain branches of the mathematics, statistics, and, increasingly, the engineering and economics professions that dealt with problems of localized economic efficiency. Second—and probably more important—close connections with industry promised a ready source of new problems and variations on old ones. As in their service to the military's logistical branches, incorporating new techniques into industrial operations not only gave theoreticians a satisfying sense that their work had use; crucially, the various situations faced by industries gave them an endless source of problem complexity on which to test the robustness and power of their modeling methods. Yet, theoreticians were never committed to relying *exclusively* on industrial contacts to supply them with grist for their

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[54] When ORSA was established in 1952, RAND did not see itself as in the operations research business. No RAND members were a part of the effort to establish ORSA, and only two RAND employees, H. I. Ansoff and Lloyd Young were founding members of ORSA—a group otherwise dominated by the military OR community. Of the 73 founding members, 33 were members of military OR groups, a number that did include WSEG members, but did not include the RAND members, or former members of military OR such as Morse and Kimball (who would have expanded this number significantly), or H. K. Weiss, who worked for the Aberdeen Proving Ground. This view would change quickly as both systems analysts such as Edwin Paxson, and mathematicians such as Dantzig joined on. By 1953, ORSA's initial membership expanded rapidly to include over 500 members, a number that would rise rapidly throughout the 1950s. See "Members Attending the Founding Meeting," *Journal of the Operations Research Society of America* **1** (1952): 26-27; and "Members of the Society, July 1, 1953," *Journal of the Operations Research Society of America* **1** (1953): 8-16. Mirowski, *Machine Dreams*, p. 178 relays further membership figures: over 5,000 members within its first decade. When ORSA joined with the Institute for Management Science in 1995 the new Institute for Operations Research and the Management Sciences boasted more than 11,700 members.

theoretical mills. Often they simply imagined their own variations on problems, much to the chagrin of those who still felt the entire point of OR was to solve *industry's* problems, not problems that sounded industrial.

The need to *apply* mathematical theories was stressed constantly within a contingent of the OR community who fretted (with reason) about the field's relationship with its often finicky clients and employers in industry, but their frequently-articulated concerns assumed that theoreticians shared their conviction that every abstraction should eventually lead around to new applications.[55] This stress on applicability, I would argue, misread the nature of theoreticians' relationship with mathematical problems, which actually entailed the exploration of theoretical implications of the mathematical classes of problem being studied. To those not engaged with theory development and its ability to leap between contexts, this exploration could appear tantalizingly concrete in one instance but forbiddingly abstract in the next.[56] Yet, to expect that theoretical work was done with at least one eye toward application was bound to lead to disappointment, because ultimately, for theoreticians, theory *stimulation* was a far more important benefit of their association with OR than was finding a practical outlet for their wares. Practice-oriented OR communities had to tolerate committed theoreticians' mathematical musings on issues that seemed to have neither fundamental nor applicative importance, not to

[55] See, for instance, Morse, "Where is the New Blood?"; Omond Solandt, "Observation, Experiment, and Measurement in Operations Research," *Journal of the Operations Research Society of America* **3** (1955): 1-14; and John F. Magee and Martin L. Ernst, "Progress in Operations Research: The Challenge of the Future," in *Progress in Operations Research*, Vol. I, edited by Russell L. Ackoff (New York: John Wiley and Sons, 1961), 465-491.

[56] Here was, perhaps, a difference between physicists and mathematicians. Physicists, who tend to correlate theory formation with immediately testable predictions, formed a large portion of the military OR community, whereas those attracted to the new realm of OR theory tended to come more from mathematics and economics, who proved more content to theorize free of ontological obligations. This tension, however, is currently causing some angst in the physics community in the debate over the value of string theory.

mention abstract theory's eventual dominance of their journals.[57] However, on the balance it proved beneficial to the more practical practitioners as well, because they *were* able to extract a steady supply of formulations and skills they could adapt to make inroads into management consultants' territory, especially their most ancient and quantitative outpost, cost accounting.[58]

It was not, however, the Traveling Salesman Problem that sealed the relationship between mathematicians and OR practitioners, nor even really the revolutionary optimization tool of linear programming, which had been created and had thrived outside of the OR rubric. If any one body of scholarship can be said to deserve the honor, it is surely inventory theory.[59] The problem of inventory is deceptively simple on its surface. Storing an inventory entails taking on certain costs that might have great importance to managers seeking to maximize their competitiveness. The cost of purchasing and managing storage space is, of course, only the most obvious. Storing goods too long risks costs associated with depreciation, obsolescence and spoilage. An outdated car model cannot be sold for as much money as a new model. Summer clothes will not sell in August and September without a steep discount. Spare parts might be rendered useless if a new machine is bought. Rotten fruit must be thrown away. There are also opportunity costs associated with holding goods in a space that might be devoted to other more profitable goods. However, the costs of *not* keeping an inventory can be very steep. If a merchant runs out of goods and cannot sell to customers on the spot or if the

[57] The journal *Interfaces* was ultimately established in 1970 to publish applications in order to address just this problem.
[58] The primary challenge to the OR community was to keep pure theoreticians out of executives' offices. There are articles to cite on this point, including those mentioned in note 55, but we will reserve the discussion of such professional concerns for the next chapter.
[59] Queuing theory could also make a strong claim, but it had strong enough ties to engineering problems that the more economically inclined inventory theory makes the stronger case as a problem of managerial concern.

anticipated delivery time for ordered goods is too long, customers might decide to make not only that purchase, but future purchases from another merchant. The question to merchants, then, is how much inventory should one keep? Depending on assumptions made about the inventory model, these calculations can be quite difficult to formulate and even more difficult to solve, and, as such, they were irresistible to some of the greatest mathematical and statistical theoreticians of the postwar era.

Before inventory could become a mathematical topic, however, it first had to become a scholarly topic. The foundations for scholarship in inventory had been laid in two major places prior to World War II: business cycle economics and the literature on management. At the dawn of organized inventory theory in the early 1950s, the Princeton economist Thomson Whitin discussed the historical factors giving rise to the need for rigorous inventory theories in his book, *The Theory of Inventory Management*. According to bibliographical research he had conducted under the sponsorship of the new Office of Naval Research (ONR), during the nineteenth century inventory had moved from being seen as an unquestioned asset and as a sign of opulence to being seen as a liability. This shift in attitudes had been brought about through changes in both the goods being produced and in the manner of production. With the coming of industrial modernization, production outputs increased, leading to the possibility of truly massive (and thus expensive) inventories of salable goods; goods could be transported to distant locations more easily, meaning that inventories could be disposed of in alternative markets; and profit margins shrank because of increasingly fierce competition making marginal increases of efficiency more important. Inventories thus became at once a more prevalent, more burdensome, but also a more controllable aspect of economic life.[60]

However, inventory size could not always be controlled. The inability to rid oneself of excess stock had also become associated with broader economic downturns, especially in the aftermath of the Great Depression. Among some business managers, inventories had taken on the aura of a bad omen,[61] but there were those who were thinking more deeply about the relationship between inventories and national economy. Notably, the chain leading from large inventories to reduced factory orders to unemployment to reduced consumer demand and, thus, more undiminished inventories had been recognized by the British economist John Maynard Keynes, among others, as one of the fundamental drivers of business cycles. Keynes analyzed inventories in terms of "working capital," which denoted all goods in production, transport, and being held as a hedge against interruptions of service, and "liquid capital," which denoted surplus stocks.[62] Whitin himself preferred the alternative terminology of "planned" and "unplanned" inventories, which had been introduced by other authors, and he felt it "would be extremely useful" for understanding its relationship to business cycles. However, he admitted, "To estimate the volume of planned and unplanned inventories [...] is no simple matter because of the lack of any objective standard for indicating what

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[60] Thomson M. Whitin, *The Theory of Inventory Management* (Princeton: Princeton University Press, 1953), chapter 1.

[61] Among a raft of articles appearing in newspapers and business journals, Whitin cited G. T. Trundle, Jr., "Your Inventory a Graveyard?" *Factory Management and Maintenance* 94 (1936): 45, as a notable example.

[62] Whitin, *Inventory Management*, pp. 110-114. See John M. Keynes, *A Treatise on Money*, Vol. II, (New York: Harcourt Brace & Co., 1930), Book VI, p. 116. Whitin observed that surprisingly few empirical studies had been made of the relationship between inventories and business cycles; with exceptions in the work of Ralph Blodgett and Moses Abramovitz; see Ralph H. Blodgett, *Cyclical Fluctuations in Commodity Stocks* (Philadelphia: University of Pennsylvania Press, 1935); and Moses Abramovitz, *Inventories and Business Cycles* (New York: National Bureau of Economic Research, 1950).

plans have been."[63] In other words, before one could determine when one's inventories had deviated from what they were supposed to be, one first had to develop some sort of rational expectation of just how much inventory one was supposed to have on hand. Until that point, however, no rigorous inventory standards had been developed by business managers or within economic theories of the firm,[64] and, thus, it was still difficult to relate the experiences of individual firms to the experiences of the overall economy.

Whitin, however, felt that situation was changing because he believed that there was a trend moving away from "key decisions being made largely on the basis of intuition" and toward a "transition to 'scientific' inventory control." He cited the aforementioned large-scale industries and reduced profit margins, the increasing sophistication of business training, and the increasing role of engineers and, yes, cost accounting in entrepreneurial practice as factors contributing toward the building of better theory.[65] His book was a testament to this trend,[66] chronicling a rash of postwar treatments of inventory issues in economic theories, and, just before it went to print, Whitin inserted references to two insightful new papers appearing in *Econometrica* into his preface.[67] Whitin could not have known that these papers signaled the beginning of a newly focused field of inventory theory in which he would play a role. Nor could he

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[63] Whitin, *Inventory Management*, p. 115.

[64] The theory of the firm relates to the function of the actions of a directed enterprise within the context of a free market economy; this theory begins at the level where individuals collaborate toward a common goal rather than compete against each other, and is a subset of microeconomics.

[65] Whitin, *Inventory Management*, p. 5.

[66] Another testament was the appearance of new inventory control schemes for sale to companies, such as the "Wilson Inventory Management Plan," marketed by R. H. Wilson, and employed by Westinghouse Electric and General Foods; Whitin, *Inventory Management*, p. 208.

[67] Whitin, *Inventory Management*, p. v. These were: Kenneth J. Arrow, Theodore E. Harris, and Jacob Marschak, "Optimal inventory policy," *Econometrica* **19** (1951): 250-272; and A. Dvoretzky, J. Kiefer, and J. Wolowitz, "The Inventory Problem: I. Case of Known Distributions of Demand," *Econometrica* **20** (1952): 187-222.

have known that this research would find a frequent home in a field that until that point had been most associated with studying the planning of military combat operations. Thus the book had made no mention of OR. Nevertheless, within a year of its 1953 publication, Whitin would become a member of the newly-established ORSA and move to the equally new School of Industrial Management at MIT, where he would join the institute's even newer interdisciplinary Operations Research Committee, which had been founded to foster graduate student work on OR-related topics, which included inventory control.[68]

Of course, the publication of Whitin's book, the publication of the *Econometrica* papers, and the incorporation of inventory theory into OR were not unrelated events. The ONR project that had sponsored Whitin's bibliographical research was actually part of a much larger project that had begun in the late 1940s at George Washington University (GWU) and Cornell University to study logistical problems in general. In the summer of 1950 this program had attracted the attention of a newly-minted PhD from Columbia University's economics department named Kenneth Arrow, who had recently obtained a position at Stanford University.[69] While hunting around for a dissertation topic in the late 1940s (eventually landing on the "impossibility theorem" in social choice, which would become the cornerstone of postwar rational choice theory[70]) Arrow had joined up with

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[68] The School of Industrial Management is now called the Sloan School of Management. The book was reviewed by an employee of the Operations Research Office in the *Journal of the Operations Research Society of America*, wherein the first five chapters were deemed especially relevant to those in OR; the chapters after those included discussions of applications of game theory as well as military logistics projects, including the work of Dantzig at Project SCOOP; see Alvin Karchere, review of Thomson M. Whitin, *The Theory of Inventory Management*, *Journal of the Operations Research Society of America* 1 (1953): 314-316. Whitin's name appeared occasionally on meetings of the MIT Committee on OR; for minutes see Morse papers, Box 2, "Committee on OR Minutes" folder; his name appears in "Members of the Society, July 1, 1953".

[69] Arrow had gone to Columbia before the war with an interest in mathematical statistics, but the subject was not taught in the mathematics department, and so he ended up as a student of the economist Harold Hotelling.

[70] See S. M. Amadae, *Rationalizing Capitalist Democracy: The Cold War Origins of Rational Choice Liberalism* (Chicago: University of Chicago Press, 2003), chapter 2.

the aforementioned Chicago-based Cowles Commission for Research in Economics, and was spending summers at the then-new RAND Corporation. In the summer of 1949, his exposure during the war to the pioneering statistical work in sequential analysis, described briefly above, led him (with RAND statistician Meyer Girshick and another summer visitor, David Blackwell of Howard University[71]) to reformulate sequential analysis from a formal decision-theoretic viewpoint, supposing that the precise definition of an optimal decision is reformulated in stages as a result of random events occurring at each stage.[72] After that summer, Arrow left Cowles for Stanford, but continued to spend some time at RAND, and the next summer his former Cowles colleague Jacob Marschak invited him to an ONR-sponsored conference on inventory problems being held there. Hectographed copies of ONR-sponsored bibliographic research by Whitin, and Louise Haack of GWU, and the general sway of discussion at the conference led Arrow and Marschak to address what they saw as the clearest logical problems affecting inventory policies.[73]

[71] Blackwell was among the very few African-American professional mathematicians of that era, and would shortly become an important figure in game theory development.
[72] K. J. Arrow, D. Blackwell, M. A. Girshick, "Bayes and Minimax Solutions of Sequential Decision Problems," *Econometrica* **17** (1949): 213-244. It was this same summer that Arrow wrote his dissertation on social choice. As he later recalled, that summer was "very fruitful" in his research, Arrow, "The Genesis of 'Optimal Inventory Policy,'" *Operations Research* **50** (2002): 1-2. Note that Arrow's brief recollections of this paper constitute the first pages of a lengthy 50th anniversary issue of the journal.
[73] Arrow, "Genesis", p. 2; the bibliographers are identified in the references to Arrow, Harris and Marschak, "Optimal Inventory Policy".

Early Forays into Inventory and Production Control Theory

Before we move into the arguments in Arrow and Marschak's 1951 paper, "Optimal Inventory Policy,"[74] it will be helpful to pause and discuss the argumentative strategies employed by Arrow and his colleagues in the mathematical social sciences. Above all else, mathematical theoreticians prize logical consistency, which is why, in the social sciences, they encapsulate vague and variable concepts into the sharp language of mathematics. They want to understand not what is claimed to have been done—which they believe is often stated in vague or misleading terms—but the actual rationale for what is done. Of course most of them understand that no one actually thinks in strict, rational terms, and that no one makes decisions in a recognizably consistent way, but by giving a firm definition to what *would* constitute an optimally rational decision given certain acknowledged conditions, it becomes possible to understand what decisions people strive to make on the basis of their knowledge of their conditions, or what decisions are made because the fierce demands of competition for ever-increasing efficiency *must* be answered through some combination of rationality and trial-and-error lest the decision maker be run out of business.[75]

Very often, however, theoretical papers treat problems that have been so simplified that they seem inapplicable to all but the least complex and best-defined situations, or the complications that are introduced seem to appear more because they demonstrate a clever mathematical trick than because they are pertinent to real policy decisions. We need to understand why such strategies were accepted as legitimate. I

[74] This paper will be treated in additional detail in Judy Klein's forthcoming book, *Protocols of War*, alongside a similar important work, Pierre Massé, *Les Réserves et la regulation de l'avenir dans la vie économique* (Paris: Hermann, 1946).
[75] This notion is not unlike the idea of "revealed preferences" postulated by the economist Paul Samuelson in 1947; for an insightful discussion see Amadae, *Rationalizing Capitalist Democracy*, pp. 231-234.

would argue it is because of a perceived need to develop, simultaneously, an understanding of complicated problems and the mathematics necessary to navigate the implications of such problems in a rigorous way.[76] Developing simple theories both helps one to gain a foothold on more advanced cases, and also to gain an appreciation of how mathematical tools might be employed in practice. We will examine the practical side of this relationship in more detail in the next chapter. For the moment, we will remain with those leaning more toward theory development.

Until his participation in the ONR program, Arrow, at least, had not harbored any interest in inventories. We do not currently know how Arrow and Marschak developed their own understanding of inventory policies, nor is it especially important to us here, so we will limit our examination to how they built their theory in publication format. They framed their published inquiry, which was limited to the variability of demand and control over order size, as a "workable first approximation" of a more advanced theory. They also felt their approximation was adequately justified by the fact that these were the major questions handled in the extant professional literature on inventories. They began building their theory by handling an extremely simple "case of certainty", which meant that demand was actually known ahead of time, meaning that one could order exactly enough stock to last until the arrival of the next shipment, thus minimizing carrying costs. Arrow and Marschak used this simple case to prove the existence of an optimal maximum level of stock (S^*) and an optimal interval between reorder periods (θ^*), which would preclude variability of desired initial stock and reordering periods given the constancy of all costs and the sale price of the good (Figure 4.5)—a point, they

[76] Of course, the habits of theory development become self-justifying within communities that strictly identify themselves with theory—in the university, one *needs* to demonstrate cleverness to get published and thus paid.

parenthetically remarked, that was "usually accepted intuitively". They then showed how this level and interval would be calculated given these costs and prices.[77]

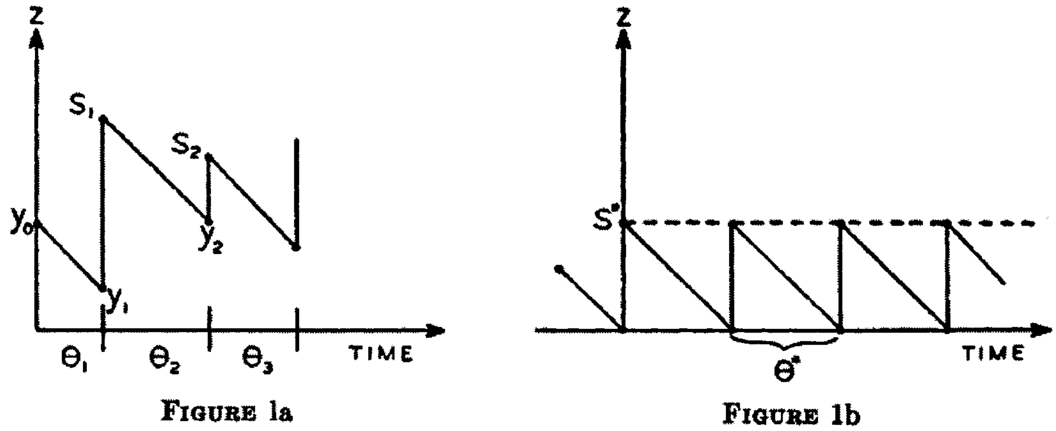

Figure 4.5. Figures 1a and 1b from "Optimal Inventory Policy" demonstrating constant demand (slope of the line denoting decreasing inventory, Z), and the variability of reordering level and interval versus the case of an optimized reordering system. Given constant demand, prices and costs, there exists a single optimum inventory level (S^*) and a single optimal interval between orders (θ^*) that should be adhered to at all times as diagrammed in Figure 1b. Source: Kenneth J. Arrow, Theodore E. Harris, and Jacob Marschak, "Optimal inventory policy," *Econometrica* **19** (1951): 250-272. © The Econometric Society. Reproduced with Permission.

Next, Arrow and Marschak considered a case taking place only within one interval, but with variable demand. In this case, because there was a finite chance that demand would be unusually high during the interval, one had to make a decision about how much carrying cost one wanted to absorb to avoid the costs associated with running out of stock (the paper quoted the line from Shakespeare's Richard III, "A horse, a horse, my kingdom for a horse," to illustrate the potential importance of such costs). Expectation values became key. One behaved during the interval as though one were going to pay a fraction of the costs of running out of stock every time one reordered based on what fraction of the time one expected to run out of inventory. Thus one needed to estimate the statistical distribution of demand, one needed to at least guess at how important it was (in dollar terms) that one not run out of stock, and, of course, one needed

[77] Arrow, Harris and Marschak, "Optimal Inventory Policy," quotes on pp. 252, 255.

to know the associated cost of holding an inventory of a certain size and the sale price of the good in order to calculate how often one was willing to deplete one's inventory in balance with how much inventory one was willing to hold.[78]

Both the multi-staged model with constant demand, and the single-staged model with uncertain demand represented the extent of rigorously-formulated inventory theory at that time. Arrow later remembered that he and Marschak "quickly saw that the interesting problem was the combination of the two models. That is, the realistically important model was one in which inventory is durable, so what is not used in one period has value in meeting future demands, but in which the demands are random."[79] Here the problem became much more complex. One not only had to calculate the frequency with which one could tolerate running out of inventory in any given interval, but one had to calculate optimal inventory levels that would incorporate the fact that future intervals made use of the final amount of goods in previous intervals. To attack this problem Arrow and Marschak decided to make use of a commonly used inventory management strategy: the "two-bin" or "(s,S)" system. Effectively, in this system, one did not calculate a constant interval of reordering *a priori*; rather one made a decision whether or not to refill inventory to its maximum level, S, at regular intervals based on whether or not the level had fallen below a fixed level, s (Figure 4.6).[80]

The real trick was calculating rationally-determined values for S and s. To approach this problem, Arrow and Marschak made any number of simplifying assumptions such as neglecting the time lag in reordering, keeping the cost of restocking

[78] *Ibid.*, quote on p. 257.
[79] Arrow, "Genesis," p. 2.
[80] Arrow, Harris and Marschak, "Optimal Inventory Policy".

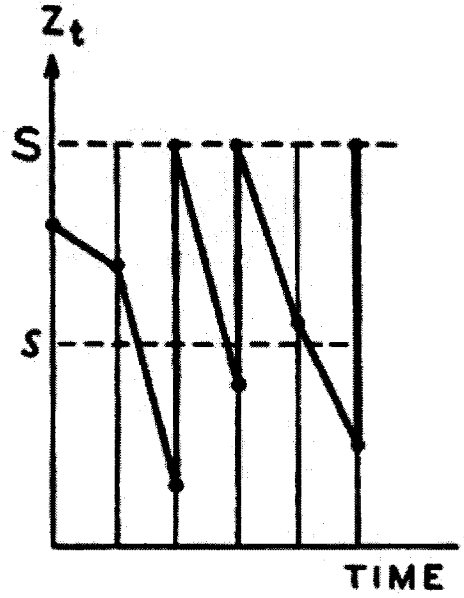

FIGURE 3

Figure 4.6. The reordering procedure in an (s,S) or "two-bin" inventory system wherein one reorders to a maximum level (S) at the end of a given period only if inventory (Z_t) has fallen below a certain level (s). Vertical lines indicate the instantaneous restocking of inventory inherent in the simplified model, the slope of the lines representing inventory depletion represent the rate of demand during each interval, which is determined randomly from a probabilistic distribution. Source: Kenneth J. Arrow, Theodore E. Harris, and Jacob Marschak, "Optimal inventory policy," *Econometrica* **19** (1951): 250-272. © The Econometric Society. Reproduced with Permission.

to level S constant with respect to the number of units required, neglecting any cost of depletion with respect to the duration of depletion, and so forth. Needless to say, there would be few instances wherein such a model could be directly applied and expect to have the levels S and s actually live up to any claims of optimality. Immediate application, however, was not Arrow and Marschak's goal. *No one* up to that point had ever made *any* sort of calculation that eschewed arbitrariness to the extent that their calculation did. Instead, they used the calculation to demonstrate the principle that inventory policies could be improved by taking into account the fact that preferred values for fixed inventory levels would be affected by the contingencies represented by the probabilities that one re-order or run out of stock at the end of any given interval. Multi-stage inventories could be treated as a Markov process, which meant that a decision at any given reordering opportunity would depend only on the state of inventory at that opportunity and not on past history, but it also meant that to calculate optimal levels ahead of time, it would be necessary to develop expectation values given all possible outcomes of all possible decisions taken at future reordering opportunities. One had, for instance, to calculate, based on the probability distribution of demand, the probability that one would run out of inventory in interval 2, given the probabilities that one had reordered at the end of interval 1, *and*, if one had *not* reordered, the probability of depletion given the probable inventory levels at the beginning of interval 2. Of course, these considerations were on top of the expected costs associated with the possibility one would not only reorder, but would have actually run out of inventory during interval 1. This process, in theory, extended for an infinite number of future intervals, although the costs incurred in distant intervals were discounted through a factor, $\alpha < 1$, which compounded exponentially with each new interval, thus ensuring a convergence at fixed values.[81]

The mathematics needed to determine the optimal levels resulting from this branching process, even with simplifying assumptions, were not only beyond the abilities (not to mention the calculative needs) of the average shopkeeper, they were beyond the abilities of Arrow and Marschak as well. They recruited a third author to their effort, RAND mathematician Theodore Harris, who had written a dissertation on such branching processes at Princeton.[82] Their final paper, including solutions to the expected costs involved with the branching process and sample applications, appeared in *Econometrica* in 1951. It was, of course, only the first word on the subject. Arrow, Harris, and

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[81] *Ibid.*
[82] Arrow, "Genesis," p. 2. Theodore Harris had been a student of William Feller who had worked out some of the mathematics of such processes in relation to population dynamics.

Marschak themselves pointed out factors at the end of their paper that could be investigated that would alter the results based on the assumptions of their models, including the possibility that the principles that Arrow had earlier worked out with Girshick and Blackwell could be used to modify the fixed demand distribution upon reassessment at each new decision point, and thereby vary the levels S and s accordingly. One problem that the theoreticians considered pressing was Arrow, Marschak and Harris' arbitrary use of the two-bin model. In a 1953 model, theorists Aryeh Dvoretzky, Jack Kiefer and Jacob Wolfowitz, working at Cornell University, showed that there were loss functions and distributions for which other models were optimal, but in 1960, Arrow's colleague, Herbert Scarf, proved that it was, in fact, the optimal strategy for a large set of assumptions.[83]

The other *Econometrica* paper cited by Whitin in the preface to his book was actually Dvoretzky, Kiefer and Wolfowitz's first foray into inventory theory the year before they demonstrated that the two-bin model was not universally optimal. Like Arrow, Harris and Marschak, they were working under the sponsorship of the ONR's logistics research program. We will not discuss this paper in any detail here on account of its stylistic similarities to Arrow, Harris and Marschak's paper. Our aim is to describe what sorts of things theoreticians of inventory set out to accomplish rather than to offer any sort of broad historical narrative of the development of inventory theory as a body of knowledge in the 1950s and beyond. Suffice it to say that Dvoretzky, Kiefer and Wolfowitz generalized certain aspects of the prior model, most notably not confining

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[83] Arrow, "Genesis," p. 2; see A. Dvoretzky, J. Kiefer, J. Wolfowitz, "On the Optimal Character of the (s,S) Policy in Inventory Theory," *Econometrica* **21** (1953): 586-596; and Hebert E. Scarf, "The optimality of (S,s) policies in the dynamic inventory problem," in *Mathematical Methods in the Social Sciences, 1959*, edited by Kenneth J. Arrow, Samuel Karlin, and Patrick Suppes (Stanford: Stanford University Press, 1960).

their analysis to the (s,S) formulation for cases involving planning over numerous future reordering intervals. A sequel to this paper in the following issue handled the further case of dealing with an unknown demand function, drawing extensively on Abraham Wald's work in sequential decision making that had sprung from his wartime formulation of sequential analysis.[84]

There were other aspects to the inventory problem as well. For instance, in the face of shifting demand, an inventory manager can, in certain situations, choose whether to absorb that shift by accepting a temporary reduction or increase in one's inventory, or to pass those shifts in demand along to a factory by increasing or decreasing one's orders in an attempt to keep the inventory at a level deemed optimal. The choice between these options is affected by costs. Factories run most cost efficiently when they maintain a constant level of production. If a factory is forced to ramp up and then decrease production, or vice versa, it incurs certain costs. On the other hand, if one absorbs shifts in demand through inventory, one incurs increased carrying costs or the costs associated with the risk of inventory depletion. In these situations, both flexibility and inflexibility in inventory policy generates certain costs, and so the object is to find some measured balance between them.

This problem of production control bears a striking resemblance to the fire control problems discussed in chapter two. If in aiming a gun one attempted to respond to every change in the course of a target—whether a result of target weaving or instabilities in tracking—one was apt to miss on account of the inability of the gun's aiming mechanism to keep up with such changes. If, on the other hand, one responded too slowly, one's aim

[84] Dvoretzky, Kiefer, and Wolfowitz, "The Inventory Problem: I"; and A. Dvoretzky, J. Kiefer, and J. Wolfowitz, "The Inventory Problem: II, Case of Unknown Distributions of Demand," *Econometrica* **20** (1952): 450-466.

was apt to wander too far off target so as eliminate the possibility of scoring any hits. The trick was to find a level of flexibility appropriate to the gun's aiming and firing capabilities. The mathematics of this process were encapsulated within servomechanism theory, which was the subject of a number of publications following the war, including Norbert Wiener's influential volume, *Cybernetics*. Herbert Simon, a polymath working at the Carnegie Institute of Technology's Graduate School of Industrial Administration, recognized the potential for the analytical tools of servomechanism theory to be of service in treating questions of production and control, and published a paper—aptly titled, "On the Application of Servomechanism Theory in the Study of Production Control"—in the same issue of *Econometrica* as Dvoretzky, Kiefer and Wolfowitz's first foray into inventory theory.[85]

The question driving Simon's work was to what extent it was appropriate to use servomechanism mathematics in the field of production control. He did not suggest this approach was a natural analytical method, but, by applying these mathematics to simple models, he hoped to gauge the "depth or superficiality" of the analogies he had drawn. Unlike in the case of Arrow, Harris and Marschak, in Simon's paper an optimal result is not obtained by setting up an equation and then solving it for minimized costs. Instead he emphasized the *design* of a *decision rule*, an equation that tells the policymaker what response to a given measurement will incur a set of desired results at the least cost. The key to designing such equations was to use the Laplace transform, which (like a Fourier integral) transforms time-domain inputs into the frequency domain, and plays a central

[85] Herbert A. Simon, "On the Application of Servomechanism Theory in the Study of Production Control," *Econometrica* **20** (1952): 247-268. On Simon, see Hunter Crowther-Heyck, *Herbert A. Simon: The Bounds of Reason in Modern America* (Baltimore: Johns Hopkins University Press, 2005).

role in servomechanism theory.[86] Such techniques had already been explored by some economists and engineers to suggest possible analytical tools for broader shifts in business cycles,[87] but Simon hoped that they might be *prescriptively* useful at the level of the firm, by allowing one to derive rules that would let inventories absorb high frequency shifts, which were more expensive for production units, but that would also pass longer term shifts, which were more burdensome on inventories, along to those units.[88]

Essentially, Simon's method entailed using a series of equations to describe a feedback loop between detected disparities in demand and production and the consequent adjustment in production orders on the basis of a decision rule that remained undefined. These equations, transformed into the frequency domain, could then be manipulated in such a way as to derive an expression relating the flow of goods to costs that varied with the nature of the decision rule to be employed and the characteristics of demand. In most of the examples Simon used, he characterized demand as a sinusoidal function, which, in principle, could be expanded to include linear combinations of sinusoidal functions.[89] Meanwhile, a certain level of familiarity with the properties of relevant classes of functions was required to choose a decision function and parameters within that function that would induce the inversely transformed equations to demonstrate a certain desired

[86] Guns can be told to respond to changes in the track of the target corresponding only to those characteristic of expected target movement capabilities and the aiming capabilities of the gun.
[87] Simon offered some examples in his bibliography; see also William Thomas and Lambert Williams, "Cultures of Computer Modeling, I: Modeling practice in Industrial Dynamics," manuscript in progress, for a brief description of some theoretical and physical servo-based economic models.
[88] Simon, "Servomechanism Theory," "depth or superficiality" quote on p. 247.
[89] Simon noted that an Air Force project then being undertaken at Carnegie Institute of Technology was attempting to reformulate the problem in "stochastic terms" wherein demand would be an autocorrelated function rather than the sum of superimposed sinusoidal functions, "Servomechanism Theory," p. 258-259n8.

behavior, i.e., minimizing production oscillations, provided costs of varying production were large compared to inventory costs.[90]

Despite the fact that in the more complicated models Simon developed the decision rules became somewhat formidable, the results achieved tended not to reveal any major discrepancies with policies that might be derived on a more intuitive basis. He described the correspondence as "reassuring", which makes sense in light of the fact that he was testing a new analytical method rather than attempting to revolutionize inventory policy in one instant. As he pointed out in the conclusion to the paper, even though most of the immediate conclusions could have been reached, "at least in a qualitative sense," by other means, "even here, intuition has been aided by the frame of reference that servomechanism theory provides. Moreover," he went on,

> the more exact procedures permit statement of our results with a degree of precision that could not be attained without them. Even in this very early stage the theory permits actual numbers to be inserted for the construction of specific decision rules that would apply ,with a considerable degree of realism, to actual situations.

By Simon's reckoning, inventory scholarship and mathematical analysis were complementary procedures wherein one could enlighten the other. This spirit was exactly what allowed advanced mathematics to find a professional home in OR.[91]

While all of the earliest work in inventory theory was published in *Econometrica* by individuals who did not consider themselves to be operations researchers, by the mid-1950s, the ORSA journal was publishing two or three articles on production and inventory theory per year while publications on the topic in the former journal waned. In 1953, meanwhile, the Institute of Management Sciences (TIMS) was established, sharing many of its members with ORSA, and its journal, *Management Science*, while smaller,

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[90] Simon, "Servomechanism Theory".
[91] *Ibid.*, p. 267.

became easily the most prolific publisher of articles on production and inventory problems in the 1950s (for reasons to be explained in the next chapter). Conferences and monographs dedicated to exploring the mathematical problems of production and inventory control provided additional outlets for inventory theory, and the ONR continued to fund research. In fact, another journal, *Naval Research Logistics Quarterly*, also began publishing in the mid-1950s and attracted high level work. Meanwhile, the concomitant integration of linear programming, queuing theory, the continuing development of search theory, game theory and other decision theoretical methods found followings within the self-identified OR community, and the establishment of a raft of university programs in OR all served to grant the new profession what it had lacked during the war: a unifying body of intellectual subject matter. Of course, OR did not maintain exclusive rights to this subject matter. Systems engineers, decision theorists, and other niche groups employed a similar set of mathematical and statistical tools. Nevertheless, while OR maintained its initial status within the military, certainly by the 1960s, if not earlier, the appropriation of statistical and economic theories into the OR canon began to dominate all of the professional trappings of OR: journals, training programs, conference proceedings, and so forth. We will examine how the old and the new faces of OR resolved their distinct intellectual missions in the next chapter. We will conclude this chapter with a brief sketch of how the intellectual mission of OR fit into what was widely understood to be a postwar revolution in the social sciences.

Mathematical Operations Research and the Revolution in the Social Sciences

Operations research was only one of many foci in which a revolution of statistical and mathematical methods in the social sciences took place during the decade and a half following World War II. It is important to understand that each one of these foci harbored its own culture, its own controversies and professional commitments, and that these cultures leaned upon each other—and, crucially, on the successes of actual policies based on their insights—to grant the entire revolution legitimacy.[92] Decision theory, for instance, dealt strictly with abstract questions relating to the logical structure of decisions: what does it mean to make a rational choice when trying to anticipate and outguess an opponent, or to incorporate new knowledge from observations taken at intervals into an evolving plan? Individuals working on such theory could work in academic domains safe in the knowledge that their formulations were not about to be taken up and directly implemented in complex policymaking environments. At the same time, there were those in the operations research communities who *were* eager to take up the logical insights of abstract theories of decision and find ways to integrate them into actual policy choices that had been handled in a largely arbitrary manner.[93] However, there was no linear model linking theory and application: it was not simply a case of *a priori* insights flowing downhill. It was only through the study of actual decisions that mathematicians, statisticians and economists began to understand how real decision making processes exploited hidden logical nuance not then available to more explicit theories. Both

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[92] In general, see Mark Solovey, "Project Camelot and the 1960s Epistemological Revolution: Rethinking the Politics-Patronage-Social Science Nexus," *Social Studies of Science* **31** (2001): 171-206; Judy L. Klein, *Protocols of War*; S. M. Amadae, *Rationalizing Capitalist Democracy*.

[93] The relatively well-known Project Camelot (see previous note) was a product of the American University-based Special Operations Research Office, which worked for the army. It was a part of the loose collection of contract research organizations serving the U. S. Army, as mentioned at the end of the last chapter. Largely, however, I mean industrial and management policy, which has not been written about extensively by historians.

operations researchers and decision theorists felt that by stating theories explicitly, whether in codified or mathematical form, they could explore the nature of such nuance, and, in the end, find ways to improve upon it in practice.

Before World War II, theories of decision already had a limited following in probability theory, and especially those (few) scholars who studied the eighteenth century work of the minister and mathematician Thomas Bayes. Bayes had written a famous paper, published posthumously, that mathematically explored how confidence in propositions is revised through the accumulation of new information. However, it was Abraham Wald's sequential analysis and John von Neumann and Oskar Morgenstern's game theory that lent new energy to the analysis of decision making methods.[94] The RAND Corporation mathematician Richard Bellman, who will not be treated as extensively as he deserves here, reached further into the exploration of time-phased decisions in his formulation of dynamic programming in the early 1950s.[95] All of these fields of analysis took very general approaches to problems such as what it means to make a rational decision in the face of expectations about the likelihood and uncertainty of future scenarios, and how one learns about and adapts to new information obtained with respect to those scenarios

Mathematical studies of specific cases were complementary to more abstract investigations. Inventory and production control were countenanced on the same

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[94] Decision theory has only begun to find its historians. On game theory specifically, see Paul Erickson, "The Politics of Game Theory: Mathematics and Cold War Culture," Ph.D. Dissertation, University of Wisconson-Madison, 2006. The importance of Wald has only begun to be recognized by historians; see Klein, "Economics for a Client," and Klein, *Protocols of War*; his influence is prevalent throughout early papers on learning processes. See also Kenneth J. Arrow, "Decision Theory and Operations Research," *Operations Research* **5** (1957): 765-774.

[95] This work was summarized in Richard Bellman, *Dynamic Programming* (Princeton: Princeton University Press, 1953); Klein, *Protocols of War* will discuss Bellman in more depth as well. He also wrote an unenlightening autobiography: Richard Bellman, *Eye of the Hurricane: An Autobiography* (Singapore: World Scientific, 1984).

epistemological basis as early systems analysis: *everyone* makes certain assumptions when they develop plans for future events no matter how much or how little one actually thinks about it. The object was to understand what elements of their decisions constituted a rational response to the problem given their own stated goals, regardless of whether decision makers thought they were behaving rationally or not. Dvoretzky, Kiefer, and Wolfowitz were adamant about this point in their first inventory theory paper in discussing the need to assign an explicit function to the variable, *W*, representing the expected losses given certain initial inventory conditions. In the middle of a highly technical discourse, they diverted a paragraph to the defense of encapsulating such a hazy notion in an explicit expression:

> It may be objected that our method requires one to specify the function *W* and that this function may be unknown or difficult to give. We wish to emphasize that the need for a function *W* is inevitable in the sense that any method which does not explicitly use a function *W* simply uses one implicitly. Thus one who selects a method of solving the inventory problem which ostensibly has the advantage of not requiring the specification of *W* is simply relinquishing control of *W*, and may be implicitly using a *W* of which he would disapprove (if he knew it). It is difficult to see what advantages can accrue to the ordering agency from deliberately burying its intellectual head in the sand. Even if the function *W* is very difficult to obtain it seems preferable to make some attempt at an intelligent decision about it. A rough approximation of greatly simplified version of the underlying *W* may be preferable to completely ignoring this fundamental datum of the problem.[96]

Their critique was the exact same one Weaver was making in his "Comments on a General Theory of Air Warfare". The Tactical-Strategic Computer was *always* running whether one acknowledged it or not: one was best served to face up to the logical intricacies of decision problems and explore them as far as possible, despite the impossibility of ever understanding their mechanisms in full.

By making a simple problem explicit and solving it, one gained insight into more difficult ones. Many OR problems in industry were attractive as intellectual footholds for

[96] Dvoretzky, Kiefer and Wolfowitz, "The Inventory Problem, I," p. 190.

decision theorists, because they represented a case of decisions made on the basis of a very clear criterion: minimizing costs measured in dollars. In a 1956 speech to ORSA, Kenneth Arrow framed the way that OR took problems and made them explicit, thereby permitting further modification of knowledge about the problem (as opposed to a rote optimization valid for all time on the basis of the explicit formulation) as a key facet of OR's relationship to burgeoning decision theory. "I would like to put forth the thesis," he remarked,

> that the open tentative character of operations research, and of all scientific analysis, can be itself discussed in terms of decision theory. More specifically, I believe that the notions of decision analysis in sequential situations, sometimes called dynamic programming, can, if properly understood, be very revealing of the tentative and approximate nature of any particular solution to an operations-research problem. This shows that, on the one hand, solutions to particular problems are less definitive than their formal statement suggests, and, on the other , formal over-simplified models which are not yet capable of being quantified may nevertheless be of great practical value.

He went on to describe a problem in inventory theory in mathematical detail, and suggested that it was a "a paradigm of most operations-research problems." The real point of an analysis, he argued, was not necessarily to arrive at the simple optimized result. Indeed, he pointed out, improvements in operations through the advanced understanding of a problem brought about through modeling it "improves the value of the decision as much or more than the exact attainment of the optimum solution of the problem, once properly formulated."[97] In making a model of a problem, ballpark estimates and educated guesses began to unravel into their constituent arbitrary and rational components. Once one understood what parts of decisions were made based on good information, and which parts were made based on wild guesses, one could begin to identify and combat arbitrariness while enhancing rationality as much as possible given constraints on time, information, control, and calculative capability.

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[97] Arrow, "Decision Theory and Operations Research," pp. 766-767, 769, 772

There was, of course, more than one way to use a theory to explore the divide between rationality and arbitrariness. In a review of a 1958 book on inventory and production control theory co-written by Arrow (with Samuel Karlin and Herbert Scarf), Richard Bellman lent some further insight into how theory development bore on inventory and production control scholarship, which he viewed as among "the most interesting classes of dynamic programming processes". Following the "classic paper of Arrow, Harris and Marschak," he suggested that theory development had forked. One trend, pursued by Dvoretzky, Kiefer and Wolfowitz, was attempting to answer "essential questions pertaining to the existence and uniqueness of the nonlinear functional equations" the settlement of which allowed one to "be sure of a one-to-one correspondence between the optimal policies of the original process and the solutions of the defining equations." If, for instance, one discussed the optimal solution to a two-bin formulation of inventory control, one would want to establish whether one was safe in bracketing out entire classes of solutions that might prove more optimal. Another trend was to study "sub-optimal policies" which meant finding methods of choosing between policies deemed feasible—the approach followed by Arrow, Harris and Marschak. Each of these trends pointed to the development of the logic by which decisions on inventory theory could be claimed to be more or less rational than others, once again, on the basis of the practices and values worked out in actual inventory control situations.[98]

Moving to a more abstract realm, the aim of decision theory, as opposed to OR, was to find a more generalized logical framework in which decision making processes

[98] Richard Bellman, review of Kenneth Arrow, Samuel Karlin and Herbert Scarf, *Studies in the Mathematical Theory of Inventory and Production*, *Management Science* **5** (1958): 139-141; Bellman also suggested a third fork in which "the functional equation can be utilized to determine the structure of optimal policies as a function of the structure of the cost and penalty functions, and also of the probability distribution occurring." We did not deal with this particular use of theory here.

implicitly made by all decision makers took place. In order to do this, it was still important to separate out what was rational from what was arbitrary, which, in this case, meant developing much more general definitions of what things were kept arbitrary because they were set in stone by a decision maker's private values, what things were arbitrary because of a lack of sufficient information and the desire or ability to calculate, and, thus, what remained that constituted rational behavior. This meant developing an analytical framework in which to discuss the relationship between arbitrariness and rationality, with respect to values and plans. Herbert Simon's 1950 book *Administrative Behavior* was dedicated to working out (non-mathematically) how these sorts of problems were handled within a large organization, and was a reaction to his "conviction that we do not yet have, in [the field of public administration], adequate linguistic and conceptual tools for realistically and significantly describing even a simple administrative organization..." By describing how administrative organizations set goals, mediated between their goals, and coordinated their employees' efforts toward achieving them by commanding loyalty and offering incentives, Simon hoped to elucidate how it was that organizations went about "getting things done".[99] Kenneth Arrow, in his (mathematical) 1951 book, *Social Choice and Individual Values*, took another tack in questioning the very logical coherence of trying to describe a group's priorities (its "social welfare function") as an amalgamation of individual priorities (or "preferences") set by individual

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[99] Herbert A. Simon, *Administrative Behavior: A Study of Decision-Making Processes in Administrative Organization* (New York: The Macmillan Company, 1950), quotes on pp. xiii, 1. The project was much older; his thesis of the same title was produced in 1945. See Crowther-Heyck, *Herbert A. Simon*, chapter 5 for additional information.

values. His work was a challenge to political theorists to make a new effort to clarify what they meant when they discussed the good of a democratic society.[100]

Of course, no theorist imagined that they had *the* answer or even *the* framework to problems of decision. The question of how to develop a theoretical approach to such problems rested largely on what theoreticians hoped their work would accomplish. Simon was mostly concerned with describing rational behavior, which meant paying attention to the ways in which calculative efficiency was achieved with respect to goals by arbitrarily circumscribing rational behavior to a limited set of possible decisions, what he called "bounded rationality". In his career, which stretched between administrative theory, OR, computer science and cognitive psychology, he sought out means of describing how people and organizations restricted their decision making in ways that achieved satisfactory but not optimal results—that "satisficed"—with respect to their goals.[101] This approach to theory was not dissimilar to the one expressed by Arrow in his 1956 speech to ORSA in which he pointed out that in inventory theory as in operations research, the object of making a model was to make more models, leading to a more nuanced understanding of the nature of a problem. "But," he pointed out, "there is always a point where we must stop." He, too, recognized that decisions had to be made using strongly arbitrary assumptions. Nevertheless, while Simon dedicated much of his career to *describing* behavior, Arrow was more concerned with *theoretical* economy. "Once a satisfactory [optimization] method has been found," he pointed out, "there is always a tendency to try to improve it. Presumably this process will continue until it

[100] Kenneth J. Arrow, *Social Choice and Individual Values* (New York: Wiley, 1951). See Amadae, *Rationalizing Capitalist Democracy,* chapter 2 for additional information.
[101] Again, Crowther-Heyck, *Herbert A. Simon* is an invaluable guide to Simon's work. See also Simon's autobiography: Herbert A. Simon, *Models of My Life* (New York: Basic Books, 1991).

converges to an optimum. Like all methods of success of approximations, the convergence will take infinitely long, if indeed it converges at all."[102] Attempting to understand this process of convergence was one of the hallmarks of Arrow's career in the economics profession.

Unfortunately, no history has yet managed to mine very far into the depths of the rich vein of intellectual thought driving individuals such as Arrow and Simon.[103] While some indication is given here of the context in which they viewed their work, many additional problems need to be addressed before we can claim to have an accurate and nuanced understanding of the thinkers in the postwar revolution of the social sciences. However, hopefully this dissertation will help to dispel the common view that these theorists genuinely believed that they could somehow capture the process of human, or at least government, decision making in a few well-executed equations and computer programs and that these equations and programs could be used to create strictly rational policies, free of politically arbitrary interference.[104] These theories, I argue, had independent appeal because they were more precise, but also more rich and descriptive

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[102] Arrow, "Decision Theory and Operations Research," p. 773.

[103] S. M. Amadae, *Rationalizing Capitalist Democracy* is an excellent beginning; but it seems to be weighed down by the need to connect the theories with the Cold War context in which it originated. The Cold War context is, of course, crucial in understanding the conditions of its origins, but, at the same time, one should not underestimate the ability of theory to move between contexts. Philip Mirowski, *Machine Dreams*, as well as Mirowski's other work, is extremely valuable, but requires a great deal of background knowledge, and must be read with great caution, both because of Mirowski's self-professed stake in contemporary economics, and his tendency to move recklessly between contexts and portray them as closely related events. Hunter Crowther-Heyck, *Herbert A. Simon*, is an excellent discussion of the intellectual project of the revolutionized social sciences from Simon's perspective, and provides an excellent model for further work. On game theory, see Paul Erickson, "The Politics of Game Theory".

[104] Amadae, *Rationalizing Capitalist Democracy*, chapter 1 quite expressly takes such a position thereby clearly identifying rational choice liberalism as the product of a Cold War milieu. Mirowski takes a very similar view with respect to neoclassical economics theory, and especially the "paranoid" worldview of John Nash (*Machine Dreams*, pp. 331-349). Paul Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America*, Cambridge, Mass.: The MIT Press, 1996, is an influential and generalized examination of the way driving scientific metaphors in the cognitive sciences corresponded to the way America chose to defend itself. He pays very little attention to the intellectual and institutional mechanisms that translated insights between contexts that we are exploring in this dissertation.

than theories that had come before, not because they promised a level of objective authority only quantitative science could provide, or because they could be plugged into a logic circuit that ran an automated military task.

At the time, the work of decision theorists absorbed and deposited insights across the social sciences, thanks in large part to a funding structure controlled by a social sciences elite (that included Simon and Merrill Flood), but since then these disciplines have fractured and these methods have been developed more independently of each other.[105] As a consequence, we are now in a position where it has become difficult to understand the important relationship between theory-building and policymaking that prevailed in the 1950s. Exploring the way fields such as decision theory, OR, and real policymaking ran *parallel* to each other and reinforced each other without directly feeding into each other one way or the other, and the way some individuals jumped so freely between them, will give us a better understanding of why their approaches to the social sciences and policymaking were deemed not only legitimate, but fruitful, and why these approaches to the social sciences still thrive, not as relics of the Cold War, but as genuinely appealing fields of inquiry.

Conclusion

Epistemologically, after four chapters, we have not come very far from Curtis LeMay's use of doctrine as a heuristic device, despite the fact that we have shifted radically from the practical planning of war to the abstract notation of mathematics. We will recall that for LeMay doctrine was something to be followed under all circumstances

[105] See Hunter Crowther-Heyck, "Patrons of the Revolution: Ideas and Institutions in Postwar Behavioral Science," *Isis* **97** (2006): 420-446.

at the moment of decision because it represented a summation of all expectations about what would happen on a mission, as well as the rationally considered responses to those expected events. Once the moment of decision was over, the plan could be evaluated and reconsidered for when the next moment of decision arrived. Operations research began its existence as a supplement to this process, by gathering and analyzing data to test the validity of aspects of military doctrine and practice. Search theory had taken the process a step further by actually rendering aspects of tactical doctrine in mathematical language and then testing the efficacy of search procedures against their theoretical limits, but it was still using an explicit formulation of a problem *based on the knowledge of experienced personnel* in order to establish the logical parameters of the problem and then making a decision that made the best use of available data and analytical techniques.

The idea of making a calculated choice based on extant information resonated strongly both with those interested in improving planning and logistics, and with those interested in understanding the logical problems of decision. Sequential analysis, game theory, linear programming, and, before long, dynamic programming, were all methods of trying to find out exactly what constituted a rational approach to a certain kind of problem. Through some combination of rationality and arbitrariness, anybody could come up with *a* solution to the Traveling Salesman Problem, the transportation problem, or any other problem, so long as it remained relatively small-scale, but these solutions would not be optimal. Analyzing them mathematically defined for what sort of solution the more instinctual decision maker was striving. However, exploring the various mathematical dimensions and these problems and the hidden logical relationships between them meant seeking out new problems on which to test the robustness of prior mathematical formulations. Meanwhile, almost coincidentally, another group of scientists and mathematicians began advocating for the establishment of a new profession to aid in industrial planning. These two groups came together in such places as the RAND Corporation, and around certain problems such as setting efficient inventory policies. Thus, no one really noticed when systems analysis and OR began being discussed as complex and simple varieties of the same concept, even though systems analysis had always been more akin to mathematical decision theory than OR. Nor did many people seem to mind when the investigative studies of the war were retrospectively recast as component optimization problems, even though most had not been.[106]

Ultimately, it was the epistemological compatibility of mathematical and doctrinal ways of viewing the world that allowed the new interrelationship between mathematical theory and policy research studies to persist, even though the meanings of words began to change. Operations research no longer necessarily entailed a bridge between technology design methods and the needs of users; it could also mean a set of mathematical techniques that were quite similar to the actual methods used in technology design.[107] As mathematical techniques proved useful, OR became associated less with its inevitable attachment to the venue of the field, and more with the duality of theory and application. In the brief and silent moment circa 1952 when OR's non-military identity was up for grabs between those who wanted to replicate the wartime experience in industry, and those who wanted to develop mathematical theory, the theorists won out. In the end, OR

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[106] See, for instance, Patrick Blackett's reaction to the recasting of the Admiralty's study of convoy size as an optimization problem in chapter six. It was, however, Philip Morse and George Kimball—two devotees of the practical side of OR—who framed the problem as an optimization problem in *Methods of Operations Research*, pp. 46-48.

[107] Operations research and systems engineering became related disciplines; for instance, see Charles D. Flagle, William H. Huggins, and Robert H. Roy, eds., *Operations Research and Systems Engineering* (Baltimore: The Johns Hopkins University Press, 1960).

did not become the new management consulting; instead consulting agencies hired operations researchers.

This end was perhaps inevitable, and there was never really a struggle or even a major controversy about it. The wartime veterans faced a steep, uphill battle to establish OR in a crowded field outside the military, and they could not deny that theoreticians' formulations gave them a fighting chance to survive in that environment. Besides, it had never been as though the very soul of operations research was at stake. Even if the profession became defined by its theoretical content, the role those techniques played at the level of OR practice could still be negotiated. The danger was that mathematical models would be applied too arbitrarily to actual policies, and thus become discredited along with the idea of OR itself. In the next chapter we will see how the tensions between the promises and the pitfalls of using abstract theories were handled in different venues.

CHAPTER FIVE

Promise and Pitfalls: The Stabilization of Operations Research in America

A Tale of Two Societies

During World War II, operations research had been a branch of the overall scientific effort associated, above all else, with the military experience of the field. As we saw in chapter one, wartime OR scientists helped military officers to understand their men's own field experience better, and to craft more robust plans in view of changing circumstances. As we saw in chapter two, it also provided an important bridge between the theoretical problems of equipment design and the tactics developed for use in the field. After the war, within the military, OR continued to serve as an aid to planning, but with the move to non-military environments and the establishment of a body of mathematical theory that constituted a new OR methodology, OR ceased to be defined as study taking place in the locale of the field operation, and instead came to be defined more around the duality of abstract OR theory and concrete OR practice. Nevertheless, even if the translation of insights between theory and practice branches of a new profession promised to lend it special vitality—and, crucially, an advantage in a world of professional managers, cost accountants, and management consultants—the translation process promised to be far from unproblematic. Suddenly, keeping abstract theory and practical application in their separate realms had moved from being a relatively minor problem, regulated by the divide between OR and laboratory design work, to being a major one. It would endanger the credibility of OR if overly simplistic mathematical policy models were applied to real problems.

The concern that theoretical work would be applied arbitrarily to the task at hand was considered by the wartime OR veterans and those who remained in military OR as an issue requiring immediate action. When the Operations Research Society of America (ORSA) was founded in 1952, maintaining the credibility of OR was as high a priority as promoting the new profession. As Philip Morse, the first president of ORSA, put it in his memoirs, "As soon as it began to look as though O/R would become popular, a few quacks began using its name to sell their magic."[1] Aside from malicious opportunism, however, there was also a real fear that demand for OR practitioners would outstrip the number of individuals whose work would live up to the name that OR had made for itself during the war. Accordingly, ORSA divided its membership into three categories: fellows, members, and associate members. Fellows were basically the self-identified OR elite, while associate membership essentially denoted a status of novice or well-wisher who was not necessarily representative of the OR profession.[2] There were, however, a number of individuals who were dissatisfied with ORSA, because they variously felt it was more geared toward military problems than problems of industrial management, because they felt it did not encourage the development of more abstract topics such as linear programming, and because they objected on principle to the stratification of its membership. In 1953, many of these individuals decided to form another professional

[1] Philip M. Morse, *In at the Beginnings: A Physicists' Life* (Cambridge, Mass.: The MIT Press, 1977), p. 291.

[2] Andrew Vazsonyi, who stumbled upon ORSA as an engineer interested in problems of managerial decision and was active in the foundation of TIMS, later recalled the issue somewhat differently: "Full members were only those theorists and mathematicians certified by the core group. Associate members were riff-raff from the real world—guinea pigs from business who could try out the full members' theories." Andrew Vazsonyi, *Which Door has the Cadillac: Adventures of a Real-Life Mathematician* (New York: Writers Club Press, 2002), p. 132.

society, the Institute of Management Sciences (TIMS), which began publishing its own journal, *Management Science*, the following year.[3]

To a certain extent, we can understand the split between ORSA and TIMS as arising from a split in understandings of the relationship of the abstract concept of science to OR and "management science". During the war, as I argued in chapter one, the idea that OR was a scientific activity was closely related to the idea that scientific epistemology constituted attaining scholarship over a body of policy knowledge, and then testing unclear or unverified aspects of that knowledge to determine whether it was valid. Not only the success, but the very legitimacy of this activity was dependent on practitioners' understanding the rationales behind actual plans and bodies of doctrine. At the end of the introductory chapter of their book *Methods of Operations Research*, Morse and George Kimball made a special point of repeating what they believed was

> the obvious fact that operations research is fruitful *only* when it studies *actual operations* and that a partnership between administrator and scientist, which is fundamental in the process, requires an administrator *with authority* for the scientist to work with. Operations research done separately from an administrator in charge of operations becomes an empty exercise. To be valuable it must be toughened by the repeated impact of hard operational facts and pressing day-by-day demands, and its scale of values must be repeatedly tested in the acid of use. Otherwise it may be philosophy, but it is hardly science.[4]

This view of science in contradistinction to philosophy stemmed directly from the tacit wartime competition between OR and more traditional means of planning. If OR did not

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[3] These motivations were enumerated in a letter written by John Lathrop in 1956 to both ORSA and TIMS. John Lathrop, "A Proposal for Merging ORSA and TIMS," *Operations Research* **5** (1957): 123-125. Also see Melvin E. Salveson, "The Institute of Management Sciences: A Prehistory and Commentary on the Occasion of TIMS' 40th Anniversary," *Interfaces* **27** (1997): 74-85; and for a different perspective, see William W. Cooper, "The History of TIMS," available online: <http://interfaces.pubs.informs.org/ORMS%20History.htm>, c1995.

[4] Philip M. Morse and George E. Kimball, *Methods of Operations Research* (New York: The Technology Press of Massachusetts Institute of Technology and John Wiley & Sons, 1951), p. 10b; emphasis in original. George Kimball would become the 12th president of ORSA in 1963.

obtain results pertinent to the heuristic activities of the military, people like Morse and Kimball would have likely been returned to laboratory work.

Morse and Kimball's ideas about science contrasted with ones laid out by the mathematician Merrill Flood in 1955 upon his retirement as president of TIMS, in which he attempted to clarify the institute's aims by characterizing management science as something distinct from OR. He compared advances in management science to the fundamental advances made by Gregor Mendel in genetics and Max Planck in quantum theory. In total opposition to the view offered by Morse and Kimball, Flood believed that "scientific contributions of first importance in understanding management are not very apt to result from any direct interest in management problems," and pointed to John von Neumann's initial formulation of game theory in 1928 while exploring the mathematical foundations of quantum mechanics.[5] In Flood's mind, "Management science of the greatest importance is apt simply to be a great scientific advance of *general* importance." Of course, Flood was no stranger to practical problems. He acknowledged that OR had "a scientific ingredient"; but he felt that, unlike management science, it was really more of a form of engineering wherein fundamental scientific principles are applied.[6] By his reckoning, a science was not a thorough and systematic study of specific bodies of non-

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[5] Von Neumann's major product of this period was John von Neumann, *Mathematische Grundlagen der Quantenmechanik* (Berlin: J. Springer, 1932), translated as *Mathematical Foundations of Quantum Mechanics*. In actuality, von Neumann had other mathematical interests, which led him to game theory; see essays in E. Roy Weintraub, ed., *Toward a History of Game Theory* (Durham: Duke University Press, 1992).

[6] Merrill M. Flood, "The Objectives of TIMS," *Management Science* **2** (1956): 178-184, p. 179; emphasis added on 'general'. According to Flood, "objective evidence of the kind of work in operations research that meets highest present standard in that field" could be found in the recent award of the first Lanchester Prize by ORSA to a study of toll booth operations by the New York Port Authority, in which, Flood claimed that "the techniques of the scientist were well employed," but he also insisted that it was "an engineering study done by an engineering group, and completed before Edie and his associate budget engineers were aware that they were doing operations research." The Lanchester Prize, incidentally, was originally sponsored by Johns Hopkins University and the Army Operations Research Office.

scientific knowledge; it was something that derived principles and knowledge of its own. Effectively, Flood's science was Morse and Kimball's philosophy.[7]

Given these differing views of science, it superficially makes sense that the early leaders of ORSA, who viewed their organization as a guarantor of professional quality akin to the American Bar Association, would insist on stratifying its membership to provide OR outsiders with an indicator of whether any given practitioner's work represented bona fide OR. It makes equal sense that members of TIMS would resist such stratification on account of the fact that the correctness of theory would be self-evident in the validity of its mathematics, that its relevance to any given managerial problem was inevitably a matter of personal interpretation, and that anyone could contribute to it regardless of their professional status. However, the notion that OR and management science could be so neatly divided around attitudes toward science did not reflect reality.[8] While it was true that *Management Science* tended to be the more prolific publisher of articles on linear programming and inventory theory, and ORSA's journal published more case studies and material of military interest, ORSA and TIMS had substantially more in common than Flood's speech might indicate.

The ongoing mystification caused by the separation of the two new societies was expressed publicly in 1956, when John Lathrop, a former member of the Navy's Operations Evaluation Group then working for the Lockheed Aircraft Corporation, wrote to the editors of *Operations Research* and *Management Science* suggesting that the

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[7] This duality tracks Peter Dear's examination of fundamental tensions in the history of science tracing back to the early modern period. See Peter Dear, "What Is the History of Science the History *Of*? Early Modern Roots of the Ideology of Modern Science," *Isis* **96** (2005): 390-406.

[8] Again we may look to Vazsonyi. He was among the movement that created TIMS (and was elected the first constitutionally-mandated "past president" of TIMS without ever being president), but he was unusually dedicated to practical uses of math, and was part of a camp that did not feel that the military-minded scientists of ORSA were attuned enough to real management problems. Vasonyi, *Cadillac*, pp. 132-135.

societies be merged because of their substantially overlapping membership, because many of the initial complaints about ORSA had become less apt,[9] and, most importantly, because he could not see any real difference between their activities.[10] His letter, however, was met with two others that disagreed, and suggested that the organizations should strive more forcefully to situate themselves with respect to each other. One of those letters was from David Hertz, a former Columbia mathematician working for the Arthur Andersen & Company accounting firm. He suggested that maintaining the split between the organizations would allow individuals to choose between supporting professionalization and the development of what Flood considered to be scientific principles, and that, naturally, there would be many who would support both.[11] As it turned out, the societies did not merge at that time. However, they also remained as practically indistinguishable as Lathrop had said they were.[12] The intellectual tensions between ORSA and TIMS were not resolved so much as they were diluted through cross-membership. In the end, the complex relationship between OR theory and practice would not be managed through professional affiliation, but on a much more local and idiosyncratic basis.

In this chapter we will examine four venues where such management between theory and practice was evident. The first is the Cambridge, Massachusetts consulting

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[9] According to Lathrop, military members constituted a much smaller percentage of ORSA's membership, its journal published regularly on theoretical topics, and was considering abolishing its stratified membership.

[10] John B. Lathrop, "A proposal for merging ORSA and TIMS," *Operations Research* **5** (1957): 123-125. Lathrop would become the 7th president of ORSA the following year.

[11] David B. Hertz, "ORSA and TIMS should affiliate rather than merge," *Operations Research* **6** (1958): 296-297. Hertz would become the 11th president of TIMS in 1964. The other letter was David A. Katcher, "On the proposal for merger of ORSA and TIMS," *Operations Research* **5** (1957): 563-564.

[12] They finally did merge in 1995 to form the Institute for Operations Research and the Management Sciences (INFORMS). "OR/MS" remains a common catch-all term. In the United Kingdom, no such divide ever existed, and the term operational research enjoyed unchallenged dominance.

firm Arthur D. Little. Unlike other management consulting firms, Arthur D. Little had traditionally been devoted to research and development issues, and saw OR as a way to apply their technical experience to the burgeoning field of management advice. The second venue is the Massachusetts Institute of Technology, which, as we saw in chapter three, administered the Navy's Operations Evaluation Group. MIT was eager to develop an OR training program in the tradition of the wartime activity and struggled for some years to determine just what constituted a pedagogy of OR. Our third case follows the philosophers of science C. West Churchman and Russell Ackoff from the University of Pennsylvania to the Case Institute of Technology, where they established a program of OR pedagogy and practice as a test of their philosophical ideas about the nature of science. Moving to the fourth venue, we return to the RAND Corporation, where a debate between systems analysts and economists redefined the purpose and methodology of systems analysis, resulting in its divorce from its origins in equipment design and remarriage to OR, with which RAND had long been associated because of its status as a center for contract civilian analysis.

We will find that in each of these four venues the key problem running through the relationship between theory and practice was the epistemological role that operations researchers spelled out for themselves with respect to managers. This role was by no means intrinsic in the nature of OR, which, of course, was not a stable entity at that time. If one adhered to the idea that operations research was a certain set of mathematical theories, then its practical utility in business would surely be limited in scope and importance to those few problems that would be pliant to those methods. This path forward, of course, was not very popular. On the other hand, if one adhered to the idea that OR was any kind of research into any arbitrarily accepted aspect of policy knowledge, then it was potentially expandable to any level, *ad absurdum.*[13] At a certain point OR simply faded into well-informed planning, and the role of the operations researcher, if stretched too far, became indistinguishable from that of intelligent managers, their staffs and the consultants they hired. During the war, as we have seen, OR had in fact been closely related to standard military planning practices and work in equipment design. However, to prosper as something more than an *ad hoc* response to the heuristic pressures of war, operations researchers had to find ways to address important problems by means that emphasized their *unique* skills, but still in ways appropriate to the environment in which they worked.

Operations Research at Arthur D. Little

At the end of World War II, the Arthur D. Little consulting firm, headquartered in a small building on Memorial Drive in Cambridge, Massachusetts, specialized in helping companies with research and development problems. Prior to the war, the firm had also developed a modest effort in management and economic advising in relationship to its technology-related activities, but, for the most part, it had not taken part in the boom in

[13] We will recall from Chapter 3 that Ellis Johnson, the director of the Army ORO, had a particularly expansionist view of OR. In a March 1951 meeting of the National Research Council OR Committee, Johnson "stated the view that operations research should be broadened to include social and political phenomena and that an operations research group should be placed in the Executive Office of the President to attack problems of 'National Political Objectives'." Bob Robertson, who had replaced Philip Morse as research director of the Weapons Systems Evaluation Group, objected, suggesting that OR first address "the more concrete problems of the control of national defense production, to which it can make contributions in quantitative or at least semi-quantitative form" and was concerned that decisions taken at that level were mostly taken for political reasons, which OR did not have special tools to address. The committee agreed that Robertson's plan was best. "Minutes of 3/23/51Committee meeting," 6/1/51, *PMM*, Box 9, "NRC Committee on OR" folder. This was the same meeting that decided to set up a sub-committee to explore the possibility of setting up a professional society in OR. We will encounter another proposal for an OR program to address problems of national planning at the end of this chapter.

management consulting of the 1930s.[14] Operations research, however, provided a springboard for the scientifically-skilled consultants at the company to make a new entry into the problems of management. The company's initial ideas about OR were based entirely on the wartime experience.[15] The metallurgist Bruce Old was a new addition to Arthur D. Little. He had served in the Navy's Office of the Coordinator for Research and Development, and was one of the so-called "bird dogs" who played a major role in the establishment of the Office of Naval Research, and had had some experience with OR as well.[16] Gilbert King was a research associate in chemistry at nearby MIT and an associate of the firm who had been a member of the OSRD's OR groups in the later years of the war. They convinced Raymond Stevens, a vice president of Arthur D. Little, that the company might try and develop OR into an industry-related activity.[17]

Stevens assigned Harry Wissman, a rare holder of a business degree in a company of scientists, to construct an OR group on an experimental basis. In the fall of 1949 Wissman hired John Magee, a young graduate of Harvard Business School then working as a financial analyst in New Jersey, to assist him. Magee later recalled that he was hired as Wissman's "briefcase carrier", but he soon became a driving force behind the OR group, and would go on to become president of the firm.[18] Various members of the Navy's wartime OR effort also worked for the group part time, including both Columbia

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[14] See E. J. Kahn, *Problem Solvers: A History of Arthur D. Little, Inc.*, (Boston: Little Brown, 1986) for a general history of the firm.

[15] The *Industrial Bulletin of Arthur D. Little, Inc.*, No. 236, October 1947, p. 2, discussed the wartime legacy of OR, and how it was already being used in business in government, which defined OR along the loose lines of quantitative investigation in support of "general policy". According to the bulletin, "With a quantitative measure of several possibilities for action, the administrator can combine the new knowledge with the qualitative aspects with which he is familiar to arrive at a sound final decision." The bulletin was not so much a summary of activities at Arthur D. Little as a newsletter describing trends in industry.

[16] He was, for example, present at William Shockley's talk on "Simulated Hunts for U-boats," discussed in chapter 4.

[17] John Magee, interview with William Thomas, 4/11/2005, Concord, Mass.

[18] *Ibid.*

University's George Kimball (who later joined full time and became a vice president), and MIT's Philip Morse.[19] In fact, in the early 1950s, a significant proportion of the early permanent staff of the OR group were recruited from the Navy Operations Evaluation Group, including Arthur Brown of the NRC committee on OR, and John Lathrop, before he joined Lockheed.[20]

Meanwhile, Sears, Roebuck, and Company, a client of Arthur D. Little's R&D consulting services, agreed to serve as a test case for the new OR activity. Sears had collected punch card records of the names, addresses and ordering histories of some ten million of its mail-order customers. Because of the prohibitive costs of mailing their bulky spring and fall catalogs, Sears knew it was only worthwhile to send them out to those customers who were most likely to make a purchase. Over the course of decades, the company had established an elaborate set of rules to manage catalog distribution, and had even conducted tests wherein they sent catalogs out to everyone on the list in certain markets to see how reliable the rules were. The Arthur D. Little OR group was given the task of improving on these rules. If OR, whatever it was, could indeed improve upon them, then it would seem more likely that it was something new and important.[21]

The OR group began their test by trolling through the data that Sears had collected seeking out hidden regularities. Their mathematical analysis revealed facts

[19] Morse consulted with the groups mostly on the question of group-building, which was certainly commensurate with his experience as the administrative head of the wartime Operations Research Group and the first research director of the Weapons Systems Evaluation Group, not to mention his stint as the first director of the Brookhaven National Laboratory. *Ibid.*

[20] John F. Magee, "Operations Research at Arthur D. Little, Inc.: The Early Years," *Operations Research* **50** (2002): 149-153. Horace Levinson, the head of the NRC Committee on OR, also consulted with Arthur D. Little. Levinson had worked in mail-order retailing prior to joining Bamberger's and Macy's, and, as we will see, his experience would have been useful in dealing with Arthur D. Little's first OR client, mail-order giant Sears, Roebuck and Company. One of his studies at the (unknown) mail-order firm was determining the impact order delays had on the propensity of customers to not accept delivery on cash-on-delivery orders. See Horace C. Levinson, "Experiences in Commercial Operations Research," *Journal of the Operations Research Society of America* **1** (1953): 220-239.

[21] Magee, "Arthur D. Little".

such as that the frequency of ordering was a far better guide to future ordering habits than order size. George Kimball suggested that the predictive value of customer information would decay exponentially with the data's age, and turned out to be correct. In the end, although the changes made in the rules on account of the OR group's study were not radical, they did result in millions of dollars in extra revenue every year at no additional advertising cost. Magee published the results of the study in the second issue of the *Journal of the Operations Research Society of America* in 1953, disguising Sears as a coffee distributor deciding how to allocate its promotional aid to stores. For its part, Sears continued to employ Arthur D. Little's OR consulting services for many years afterward.[22]

The Sears study is an excellent example of a commercial OR effort balancing the veteran perspective of OR as the act of scrutinizing existing policy with the newer perspective of seeing advanced quantitative methods as a unique feature of OR. The study was crucially dependent on the large body of theory, data, policy and heuristic practice already surrounding the question of catalog distribution, and succeeded by looking for places where theory was incomplete and could be improved. At the same time, although the study did not make use of any (as-yet non-existent) stock of mathematical OR tools, it did deploy methods of data analysis that would not necessarily have been obvious to others who were more ordinarily associated with the catalog distribution problem, and was thus able to find efficiencies that remained hidden to simpler forms of analysis.

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[22] Magee, "Arthur D. Little"; John F. Magee, "The Effect of Promotional Effort on Sales," *Journal of the Operations Research Society of America* **1** (1953): 64-74.

This balance in wartime and mathematical perspectives was also evident in a 1953 article on operations research that John Magee co-wrote for the *Harvard Business Review* (*HBR*) with Cyril Herrmann, a professor of management at MIT. Publishing articles in the review was a common method used by management consultants to promote their services to executives,[23] and Magee and Herrmann's article was clearly written with the review's executive audience firmly in mind. The article made only the briefest mention of the military, but clearly translated the role OR played with respect to military planners to the corporate executive, framing OR as a way to help "single out the critical issues which require executive appraisal" and to provide "factual bases to support and guide executive judgment." However, Magee and Herrmann were also careful to convey the idea that OR represented something new and unique, embracing its connection to the "scientific method." A two page sidebar of "exhibits" worked through various technical examples, complete with integrals, "π^2"s, and "$e^{-g/S}$"s.[24] The burden of the article was to convince managers that OR was both scientific and compatible with existing methods of corporate management.[25]

Magee and Herrmann's task was made difficult by the fact that science had no inherent authority in the business world. In fact, in the introduction to that issue of *HBR*, the editors marveled in mock incredulity: "Apparently a team of men, most of them unfamiliar with the business, can go into a company and solve industrial problems that a

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[23] Christopher D. McKenna, *The World's Newest Profession: Management Consulting in the Twentieth Century* (New York: Cambridge University Press, 2006), p. 72. Arthur D. Little was not mentioned in the article, but Magee's affiliation was listed in the introductory comments for that issue.
[24] The term "exhibit" was the standard term used in *HBR* for figures and sidebars.
[25] Cyril C. Herrmann and John F. Magee, "'Operations Research' for Management," *Harvard Business Review* 31/4 (1953): 100-112.

veteran management cannot!"[26] Business culture, like military culture, was rational, but it was not intellectual. It was primarily founded on negotiations between individuals identified as reputable who collaborated on projects or negotiated deals by intelligently examining their mutual goals and conflicts of interest because usually the most rational path of action was revealed by exploiting personal connections to expand and improve available policy options, among which a few would prove clearly superior. In such situations, establishing the reputability of a contact or the utility of a new hire was more likely to be relevant to them than a sophisticated synthesis of information. Within this culture, overt salesmanship was not considered necessary or desirable between serious men. Correspondingly, business managers often suspected that remedies to their problems proffered by cultural outsiders, scientists included, would prove impractical because they did not conform to their everyday notions of rational business practice.[27]

So, Magee and Herrmann had to demonstrate that intellectual liaison between operations researchers and managers was not only possible but actually in their mutual interest. To do so, they had to find a way to bridge the cultural and intellectual divides between business culture and OR.[28] Part of this bridge could be built by putting the right

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[26] "In This Issue," *Harvard Business Review* **31**/4 (1953): 7-12, p. 8.

[27] On the importance of professional culture in management consulting practice, see McKenna, *World's Newest Profession*, chapters 6 and 8. The interpretation of rational business culture is my own, and I invite a critique of it.

[28] We will not have the opportunity to discuss some alternative means of creating this bridge. The aforementioned mathematician and Ramo-Woolridge employee Andrew Vazsonyi published a series of articles in *Management Science* and *Operations Research* describing how he translated production scheduling problems into mathematical language by means of such novel devices as "Gozinto" ("goes into") diagrams. According to Vazsonyi, "the tempo of ... changes [in inventory and production control methods] will greatly depend on the degree of integration effected between the production men, the management scientist, and the electronic engineer." He explicitly noted it was necessary to "build a bridge" from the language of operating personnel "to mathematical equations. That such a thing can be done and that it can be useful," Vazsonyi alerted his audience of management scientists, "most likely is a perplexing thought to the reader." See Andrew Vazsonyi, "The Uses of Mathematics in Production and Inventory Control," *Management Science* **1** (1954): 70-85, quotes on pp. 70, 71. See also Andrew Vazsonyi, "The Use of Mathematics in Production and Inventory Control—II (Theory of Scheduling)," *Management Science* **1**

kinds of people into OR groups. The introduction to the article in *HBR* identified the business school graduate Magee as a "mathematician", while Herrmann was a "business specialist," which was to say someone who "goes around to the client company's plants and offices, gets the necessary information, and sizes up the business problem" for the more technical members of the OR team. Another part of the bridge was built by demonstrating an acute awareness of the failures of previous self-identified "scientific" approaches to business problems on account of the inability to liaise effectively with business culture.[29] For instance, Magee and Herrmann recognized the danger that "regular employees might resent 'outsider' investigators dipping into the internal operations of the company." To allay such fears, they leveraged the established reputation of management consulting agencies, the employees of which, they claimed, were "trained to approach their work with integrity and tact". These were individuals who understood how analysis integrated itself into corporate decision making: "Professional personnel in operations research strongly emphasize [the] distinction between the operations research responsibility for analysis and the executive responsibility for decision," Magee and Herrmann wrote. Executives always had a role in shaping analysis by instilling their organization's unique values, concerns and practices into an analysis. They assured managers that their businesses would not suddenly be run by some overly rigid, amoral, by-the-numbers approach the moment the operations researchers were let in the door.[30]

(1955): 207-223; and A. Vazsonyi, "Operations Research in Production Control—A Progress Report," *Operations Research* **4** (1956): 19-31.

[29] The article explicitly noted the threat "to send the term 'operations research' along the way of others, like 'efficiency engineering,' which sooner or later became victims of undiscriminating acceptance and careless usage." Herrmann and Magee, "'Operations Research' for Management," p. 112.

[30] Herrmann and Magee, "'Operations Research' for Management," pp. 109, 111. They also related an incident (p. 111), to which, apparently, operations researchers "point[ed] with approval," in which an

Magee and Herrmann did not, however, identify OR one-to-one with management consulting. Unlike consulting, OR was not something that, by definition, was external to an organization, especially in medium-to-large-size organizations that might have constant use for quantitative policy investigations. (Conveniently, consulting agencies such as Arthur D. Little could be used to "try out operations research before it commits itself permanently," and to help initiate and organize an internal OR group once the firm had decided to go ahead.[31]) The authors recognized, though, that the institutional and conceptual independence of OR tended to separate its interests from those of its businessmen clients. Fortunately (if one followed in the wartime tradition), the quality and, indeed, the very intellectual legitimacy of OR was actually dependent on its compatibility with extant policymaking commitments and frameworks: the less well operations researchers addressed the problems specific to that framework, the less successful their work was likely to be, the less representative their work was of OR. Magee and Herrmann distanced themselves slightly from other self-identified OR practitioners in the nascent profession and sympathetically critiqued their professional strategies in terms of how well their approach to the discipline allowed them to integrate their work with the existing role of managers, and, hence, how well they advanced the profession.[32] If managers' successes were the profession's successes, and if success was largely a result of bridging the divide between OR practitioner and manger, then managers and OR practitioners had every incentive to find ways of bridging the divides

executive overrode OR recommendations that would lead to better efficiency at a plant on account of "his estimate of the psychological effect that increased in volume would have on the plant personnel."
[31] Herrmann and Magee, "'Operations Research' for Management," p. 110.
[32] For instance, they pointed out, "Some practitioners even take the rather broad point of view that operations research should include rather indefinite and qualitative methods of the social fields. Most professional opinion, however, favors the view that operations research is more restricted in meaning, limited to the quantitative methods and experimentally verifiable results of the physical sciences," Herrmann and Magee, "'Operations Research' for Management," p. 102.

between them. Management consulting firms such as Arthur D. Little could offer their professional reputation for successful collaboration as collateral that such a bridge could be built in spite of the strangeness of applying OR's mathematical methods to new kinds of problems.[33]

That the Arthur D. Little consultants really did judge the epistemological legitimacy of their work by the standards of their practical applicability is evident in their published treatments of inventory and production control theory, which we examined from the perspective of its theoreticians in the Chapter Four. Arthur D. Little began working on inventory problems with a 1951 study for Johnson & Johnson led by George Kimball and soon built up an expertise in the area.[34] The firm's first publication on inventory theory was a 1955 article in ORSA's journal by Herbert Vassian following on Herbert Simon's 1952 paper on the application of servomechanisms to production control, modifying it to handle discrete variables rather than continuous functions as representations of system behavior, thereby reflecting the way most data were actually obtained. While mathematically sophisticated, the paper did not attempt to blaze new trails as most papers in inventory theory would. Instead, it focused sharply on the practical aspects of the problem of decision rule design. For instance, a substantial part of the paper was dedicated to the representation of the inventory reorder system in

[33] Some criticism was later made from within OR, and is usually brought up by historians, to the effect that OR placed itself in a politically tendentious position by so closely identifying with corporate culture. The point is certainly correct enough, but at the same time it seems strange to bring up much larger questions of business ethics out of historiographical reflex within a discussion of the intellectual foundations of postwar OR. Further, unless one takes the radical position that business culture, at large, was at odds with a more progressive American culture, the idea that any substantial portion of OR study was burdened by such an ethical component is surely false. Discussions of inventory control, for instance, often showed a strong concern for employee and consumer relations. For an opposite viewpoint, see Stephen P. Waring, *Taylorism Transformed: Scientific Management Theory since 1945*, Chapel Hill: University of North Carolina Press, 1991, chapter 2; Stephen P. Waring, "Cold Calculus: The Cold War and Operations Research," *Radical History Review* **63** (1995): 28-51. Also see the discussion of West Churchman and Russell Ackoff later in this chapter.
[34] Magee, "Arthur D. Little," p. 151.

graphical form, thereby lending further "insight into the system behavior, thus aiding the choice of the decision rule."[35] The paper is probably best read as a theoretical examination of inventory control from the perspective of someone with an eye trained firmly on practical implementation.

Subsequent publications by employees of Arthur D. Little would venture further away from theory and closer to practical application with well-educated business managers as a primary audience. In 1956 John Magee published an unusual three-part series on inventory policymaking in *HBR* that combined the insights of the previous several years on inventory theory with the spirit of outreach from his *HBR* article on OR three years earlier.[36] This series made only passing mention of OR, and instead concentrated on introducing business executives to the field of scholarship that might help them view their own inventories as a policymaking tool rather than simply as a necessary evil. Magee framed the problem as that of the executive attempting to mediate between different departmental perspectives on inventory, writing that the inventory problem was

> made more difficult [for managers] by the fact that generally each individual within a management group tends to answer the question [of how big an inventory should be] from his own point of view. He fails to recognize costs outside his usual framework. He tends to think of inventories in isolation from other operations. The sales manager commonly says that the company must never make a customer wait; the production manager says there must be long manufacturing runs for lower costs and steady employment; the treasurer says that large inventories are draining off cash which could be used to make a profit.[37]

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[35] Herbert J. Vassian, "Application of Discrete Variable Servo Theory to Inventory Control," *Journal of the Operations Research Society of America* **3** (1955): 272-282, quote on p. 282. The article was based on a talk Vassian had given to ORSA in 1954. Vassian had died prior to the submission of the article; John Magee made a few minor corrections and added an abstract before submitting it.

[36] John F. Magee, "Guides to Inventory Policy, I. Functions and Lot Sizes," *Harvard Business Review* **34**/1 (1956): 49-60; John F. Magee, "Guides to Inventory Policy, II. Problems of Uncertainty," *Harvard Business Review* **34**/2 (1956): 103-116; John F. Magee, "Guides to Inventory Policy, III. Anticipating Future Needs," *Harvard Business Review* **34**/3 (1956): 57-70.

[37] Magee, "Guides to Inventory Policy, I," p. 50.

How might a skilled manager create a coherent policy from varying perspectives? In order to assess the problem from an overarching managerial perspective and to develop an effective policy, one first had to ask the question of what roles an inventory served in one's specific company. Were they the result of economies achieved through large production runs or buying in bulk? Did they guard against volatile shifts in demand? Were they being built up for an anticipated busy season?

Once managers asked such questions, they could begin to formulate a more rational response to the inventory problem so as to minimize inventory-associated costs. Addressing the problem from the perspective of the cost accountant was key. In cost accounting, one did not simply enter credits and debits into a ledger as money came and went: one examined company policies in terms of how much one stood to gain or lose by using resources in alternative ways. If one sunk one's resources into inventories, for instance, that act cost the company in terms of lost interest rates on bonds or other investment opportunities not chosen. However, if one avoided other larger costs by keeping an inventory, then the inventory could be regarded as a productive investment. Magee observed that these costs, such as those associated with depletion, could be difficult to calculate, but (much as Dvoretzky, Kiefer, and Wolfowitz pointed out in their paper discussed in the last chapter) these costs were always present in inventory policies. By exploring what functions inventories served in any given company, managers in that company could more easily determine what expenses were really avoidable and what expenses were necessary. Crucially, the evaluation of such costs could only be interpreted through reference to the traditional practices of any individual company. For example, rather than assign a more-or-less arbitrary dollar cost to lost sales, a company could instead simply fix what it defined as a "reasonable" standard of customer service, and then setting inventory costs associated with fluctuating demand around that standard. Magee recognized that the calculation of such costs was ultimately a cultural matter. "Fluctuation stocks are part of the price we pay for our general business philosophy of serving the consumers' wants (and whims!) rather than having them take what they can get," he wrote. "The queues before Russian retail stores illustrate a different point of view."[38]

How sophisticated a particular inventory system needed to be was also a function of the needs of individual companies. In some cases, simply establishing relatively arbitrary standards of inventory behavior would suffice. One could chart expected inventory levels versus actual inventory levels on a graph and then determine one's inventory policies on the basis of the efficacy of that graph in satisfying the company's inventory control requirements. Alternatively, in more cost-sensitive or in better monitored systems, one could actually calculate optimal policies using more or less rigorous mathematical standards. In a 1958 book on inventory and production control written with his Arthur D. Little colleague (and OEG veteran) David Boodman, Magee offered basic but robust inventory formulas while pointing in footnotes to the more sophisticated theoretical treatments, such as that of Arrow, Harris and Marschak. In some cases where there were many product lines, linear programming methods might find economical solutions, but in simpler versions of the problems trial and error would

[38] Magee, "Guides to Inventory Policy, I," pp. 50-57, quotes from pp. 52, 56. See also Magee, "Guides to Inventory Policy, II," p. 105.

tend to converge on the correct solution soon enough without the need for sophisticated calculation.[39]

Above all, Magee stressed, an inventory policy was an evolving thing. It was constant and controlled enough to achieve new economies, but it was meant to be flexible enough to accommodate new information and respond to unexpected events. Some companies might be immediately ready for complicated inventory and production control methods, whereas other companies had to undergo a lengthier process of research and meditation on business goals before appropriate policies could develop.[40] For John Magee and the consultants at Arthur D. Little any inventory policy, or really any operations research study, represented a most rational guess of policy based on current knowledge of a given policy problem, always leaving open the possibility of future refinements when convenient opportunities arose.

The Long Establishment of OR at MIT

Another center of thinking about the postwar nature of operations research was at the Massachusetts Institute of Technology, just a short walk along the Charles River Basin from Arthur D. Little. OR seems to have first been suggested as a pedagogical topic at MIT during World War II by its professor of electrical engineering Edward Bowles while he was Expert Consultant to the Secretary of War.[41] At the time MIT was

[39] Magee, "Guides to Inventory Policy, I," pp. 58-60; Magee, "Guides to Inventory Policy, III," pp. 60-63; John F. Magee and David M. Boodman, *Production Planning and Inventory Control* (New York: McGraw-Hill, 1958), pp. 136-138.
[40] Magee, "Guides to Inventory Policy, II," pp. 110-116; see also the discussion of development of inventory policies over a season in Magee, "Guides to Inventory Policy, III," p. 60.
[41] I have not found the original suggestion, so it remains unclear if Bowles referred explicitly to OR. However, a memorandum from J. R. Killian to George Harrison and Karl Compton, 9/5/1944, on "Conversation with Professor Morse Regarding Acoustical Program," *MIT*, AC 4, Box 150, Folder 6, relates, "Morse also indicated a great interest in Bowles' suggestion of a course in Methods and Problems

still swamped with war work and could not put any thought into the issue. After the war, however, interest in the idea persisted, but no one in MIT's administration seemed entirely certain what an OR program should do.[42] If OR was really just close scrutiny of the rationales behind policies, then how could a standardized pedagogical program be of benefit to future practitioners? As we saw in the last chapter, MIT even invited Patrick Blackett over from England in January 1948 to discuss the issue. Despite Blackett's lukewarm appraisal of the prospects for OR pedagogy, MIT's administrative relationship with the Navy's Operations Evaluation Group provided some continuing impetus to starting up some sort of training effort geared toward producing both military and non-military practitioners.

MIT's first real initiative in OR training was an "experimental" graduate level course that was offered by MIT's mathematics department. It was run by the mathematician and resident OEG liaison, George Wadsworth along with other members of the OEG staff who came up from Washington to participate. The first offering of the course lasted for the full 1948-1949 academic year. The second offering was only a semester offering in the spring of 1950, but a full description of the semester's material survives in the form of an OEG report. This report stressed that "a strong effort was made to convey certain basic principles and viewpoints of operations research." These principles largely amounted to warnings about the potential pitfalls of analysis. For

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of Operational Analysis, and he said he would welcome an opportunity to participate in such a course." On MIT's preoccupation with the war, see letter from J. R. Killian to Edward Bowles, 10/28/1944, *MIT*, AC 4, Box 34, Folder 6. At MIT the word "course" can represent a department, but it seems highly unlikely that MIT was considering such a move at this early date.

[42] See a series of undated memoranda in *PMM*, Box 2, "Institute Committee on O/R" folder concerning the establishment of an "operations analysis" activity at MIT. One item, however, John E. Burchard, "Questions of Scientific Librarianship Which May Be Subject to Operations Analysis," is dated 5/7/1946. See also a letter from Henry Loomis to Philip Morse, 10/24/1947, *MIT*, AC 4, Box 150, Folder 6, which indicates plans to set up a course in OR.

instance, it stressed, "Real problems do not fall neatly into one or another of the usual categories of knowledge, but cut widely across boundaries. Successful O/R," it argued, made sure that the "wholeness of the problem" was "not artificially suppressed." It also observed that qualitative understanding of a problem was a "prerequisite to useful quantitative work. A crude solution of the real problem" could have great benefits, "whereas a precise solution of an unjustified idealization of the problem is at best worthless and, if its misleading conclusions are acted on, may do great harm." The report pointed to the factor of "urgency" in determining what kind of a study should be designed, to the "utmost importance" of the "relation of an O/R group to the organization which it serves," and to the need to resolve conflicts between "the requirements of scientific freedom" and "the discipline and authority necessary to the effective functioning of a large organization, military or nonmilitary.[43]

These practical guidelines were not simply introductory material to be rushed though on the first day before moving swiftly on to a semester's worth of mathematical theory. They represented the most important concerns handed down from the wartime OR experience and the culture of postwar military OR, and they were reflected in the contents of that offering of the course. Six sessions of the course dealt with the "formulation and solution of real problems," while a full ten sessions dealt with "measures of effectiveness and effort," which entailed the selection and pitfalls of various measures of the effectiveness of operations, as well as the dangers of relying exclusively on qualitative explanation. The report lamented that only one session could be devoted to the administrative problems of OR, though references to Philip Morse and George

[43] Operations Evaluation Group memorandum, "A Course on Operations Research," *PMM*, Box 9, "NRC – Comm on OR" folder.

Kimball's postwar summary report on methods of OR was seen as making up for that somewhat.[44]

Technical material covered by the course included eleven sessions on the uses of statistics, and three sessions on "kinematics", which included search theory. Both topics were taught with an eye cast toward applicability. Five sessions of the course were devoted to the nascent game theory, primarily highlighting its applications and its potential, and noting that its theoretical formulation had far outstripped its applicability. Some of the discussion of the theory revolved around when and when not to act on game theoretical solutions. For instance, there was some discussion of the utility of mixed strategies. One of the instructors pointed out that strict adherence to the conservative minimax-driven mixed strategies of game theory could prevent capitalization on an opponent's mistakes; and that a riskier strategy should be explored if the minimax solution proved tantamount to defeat.[45] This approach to game theory was definitely motivated by the insights it was supposed to lend to practice, not on its importance for theoretical development.

Students in the course were required to turn in written assignments. These could be quite specific mathematical exercises such as finding a minimax solution for a certain game theoretical problem, including such continuous strategy problems familiar from early RAND work. One advanced problem asked what an optimal strategy would be if "each player chooses a real number; if X chooses x, and Y chooses y, then the payment to X is sin(x+y)." However, some problems emphasized the everyday experience of OR, including the formulation of a problem, as well as how to set up operational experiments

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[44] *Ibid*, including Appendix D.
[45] *Ibid*, Appendix D.

to gain data required for a solution and how to submit a report to an executive. The final written assignment was simply: "Make an operational study of the taxicab business. Submit results in the form of a report to the executive of the company." The course organizers pointed out in their report that they kept such problems purposefully vague in order to reduce the artificiality of the classroom experience and to simulate the unformed nature of a problem when it arrived in the hands of an OR group and the problems involved in making it into something that could be systematically investigated.[46]

The course's instructors were also constantly on the lookout for signs that the course's participants were not treating the material in the spirit of real operations research work. The report took special note of an

> interesting general tendency of many of the students, especially those majoring in mathematics...; they appeared to be disturbed emotionally by the uncertainty and large scope of some of these problems, and too quick to seize on some neat set of assumptions and then quickly reduce the discussion to a problem in pure mathematics.

These students tended to spend only a few lines formulating the problem and then working out pages worth of analytical solution. The report went on:

> It was necessary to combat this tendency vigorously by emphasizing the importance of qualitative considerations and crude approximate analysis, and by de-emphasizing elaborate analysis by showing that in many cases the essential features of such analysis are understandable in very simple terms and, where not, are not reliable anyway.

In the end, apparently, "all but two or three fell into the spirit of the assignments and produced very creditable reports." The course organizers added parenthetically that such reactions to problems could be used to gauge the suitability of actual OR recruits, suggesting that a person "who persists in a tendency to replace the real problem by a tidy little exercise in mathematics, will probably not become an effective O/R man (although

[46] *Ibid*, Appendix D, problems are in Appendix F.

he might become a valuable member of a mathematical service section)."[47] Recall the separation between the wartime OR groups and the Applied Mathematics Panel from chapter two. The bureaucratic ideas underlying this separation were alive and well five years later.

The course was deemed moderately successful by its creators, but already by 1951 some problems became clear. Fifteen students enrolled the first semester the course was given, dropping off to eight when "laboratory work" was introduced in the second semester. In the one semester version of the class the next year, enrollment was at twelve. Students in economics and business administration who were interested in pursuing research on OR-type topics were found not to have adequate mathematical background to approach some of the problems involved. It was also found difficult to arouse student interest in the subject in the absence of a broader pedagogical program.[48]

It was around this time, though, that interest in establishing OR outside the bounds of the military began to shift into high gear. The Case Institute of Technology in Cleveland and the Naval Postgraduate School were taking steps toward setting up their own programs in OR. Arthur D. Little had set up their OR group, recruiting heavily from the OEG. The National Research Council (NRC), meanwhile, had set up its committee to help promote OR to industry by organizing conference presentations, publishing articles, and producing an explanatory pamphlet. In the summer of 1950, Philip Morse returned permanently to the physics department at MIT following his year and a half as the first research director of the Weapons Systems Evaluation Group in the Department of Defense. That fall General Motors magnate Alfred Sloan agreed to donate funds from his

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[47] *Ibid*, Appendix D.
[48] George Harrison, "Memorandum of Meeting of the Advisory Committee on the Operations Evaluation Group," 1/9/51, *MIT*, AC 4, Box 165, Folder 4.

foundation for the creation of a new School of Industrial Management (SIM) at MIT. Shortly thereafter this donation was agreed upon (but still not announced), the NRC committee sent Horace Levinson and Alan Waterman to MIT to advocate for the establishment of an "Operations Research Center" there to supervise training, and, after meeting with MIT's provost Julius Stratton, OR was placed in the front-running as a field that could help define an innovative new approach to industrial management appropriate to MIT's leadership in scientific and technological research.[49]

In 1951 Pennell Brooks, a vice president at Sears, Roebuck & Company, was chosen to be the first dean of SIM. To investigate the appropriateness of OR to the new school's pedagogical interests, Brooks (who had apparently not been privy to Arthur D. Little's catalog distribution study at Sears) assigned Tom Hill, a young professor of accounting, to look into OR and report back to him. In January 1952, Hill registered his skepticism about the importance of OR after surveying the largely promotional literature on OR, speaking with members of the Arthur D. Little OR group, and attending a seminar series on OR being run by Morse. Since OR seemed to follow only a general "*modus operandi* of the scientists," he astutely concluded that, "the validity of any claim to innovation made on behalf of Operations Research hinges entirely on the novelty of applying the scientific method (and the scientific mind) to areas outside the usual

[49] On proposals at the foundation of SIM, see memoranda in *MIT*, AC 132, Box 10, "Industrial Management, School of," folder before and after the November 20th meeting; also see *PMM*, Box 10, "Committee on Operations Research (NRC)" folder, including diary note, "Meeting of Levinson and Waterman with Stratton and Advisory Council of MIT to discuss Operations Research," 11/20/1950, and a letter from Stratton to Horace Levinson, 12/1/1950. A subsequent meeting was held on 2/12/1950. On subsequent development of the plan see letter from Levinson to Stratton, 3/1/1951; and Levinson, "Memorandum of Telephone Conversation with Dr. Stratton," 4/27/1951, both in NRC Committee papers, "PS: Com on Operations Research 1950-1951, Operations Research Center: Massachusetts Institute of Technology: Proposed" folder. Recall that Stratton had worked as a consultant in Edward Bowles' office during World War II and would have been well aware of OR's wartime reputation. One should not assume that MIT's later Operations Research Center was a direct result of this suggestion, even though they bore the same name.

purview of the scientist..." While, he imagined, the establishment of OR had been productive within a military context on this basis, in business it seemed much less likely to prove important. He argued:

> Case discussions in the OR seminar have revealed that OR groups have frequently done no more than to arrive at operating methods which we recognize as corresponding to existing practice in certain well-managed, progressive companies. This fact has been disappointing to those of us who were anticipating dramatic revelations of startling results achieved by new techniques completely foreign to our own experiences.

He blamed OR's supporters for building up "expectations and perhaps subconscious antagonism," by insisting on their work's novelty. He felt that only industrial secrecy prevented more examples of work performed by "groups less naive concerning American industrial practice" from becoming well-known.[50]

Hill did not think that the new OR enthusiasts were charlatans, however. He recognized that their comfort with mathematical techniques was an advantage, and that it was significant that "persons entirely ignorant of certain best practices laboriously developed in business so often arrived at those same practices by independent and perhaps simpler routes."[51] Like some members of the NRC OR committee, he also felt that the "employment of the truly scientific approach in commerce and industry is spotty; and, viewed in proper perspective, the tentative exploratory efforts of OR groups indicate possibilities for far more generalized application." Ultimately the question came down to what sort of relationship SIM should establish with respect to OR. As Hill put it:

> How do we capitalize on the current interest in Operations Research in furthering our educational objectives? To fail to take advantage of the fact that we have a ready-made

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[50] Memorandum from T. M. Hill to Dean Brooks, "Operations Research," 1/9/1952, *MIT*, AC 132, Box 12, "Operations Research – P. M. Morse" Folder.

[51] Compare this statement with C. H. Waddington's statement in Chapter One that OR required "sustained and often laborious work" while "non-scientific methods of thought" required "rather little drudgery". I argued that the military's doctrine-building and planning process entailed a heuristic process. Here Hill recognizes that such methods are also "laborious". This labor, I argue, is often hidden because it occurs throughout an organization, but was more visible to someone with more direct management experience such as Hill.

> common meeting ground with our colleagues in science would seem to me unsound. On the other hand, I am equally convinced that neither a slogan nor a technique is any foundation on which to build an educational program.

He recommended that SIM steer an independent but friendly course, and that the school simply incorporate quantitative methods into its regular pedagogy, perhaps establishing a course entitled, "The Scientific Method in Industry". Upon seeing the memorandum, the provost Stratton was convinced by Hill's analysis, and scrawled "Good! have TMH run it" in the margin next to the suggestion for the course.[52] While Tom Hill's comments were a worthy appraisal of OR in 1952, he did not anticipate its professional growth and its ability to capitalize on new mathematical techniques. He went forward with the independent course as Stratton suggested, but, significantly, he also became a founding member of ORSA several months later.[53]

Back among the OR insiders at MIT, Philip Morse seems to have been unaware he had a confederate in his ranks, and was mystified that Pennell Brooks showed so little enthusiasm when it came to establishing a pedagogical program in OR at SIM, given the strong interest that OR's boosters had managed to foment. In July 1952 he wrote to Brooks, exclaiming, "I have had an average of about three inquiries a week, all last spring, from spaghetti factories and from textile mills and from railroads, asking questions about the subject and wanting to know where they could learn more." Brooks waited until September to make his noncommittal but sympathetic reply, at which point Morse turned to Stratton, explaining the enormous pressure industrialists were placing on him to tell them what action MIT was taking to produce operations researchers. He wrote that while

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[52] *Ibid.* The existing copy of this memorandum is in Stratton's files, and a subsequent memorandum from Stratton to Brooks, 2/11/1952, *MIT*, AC 132, Box 12, "Operations Research – P. M. Morse" folder, repeated the suggestion in more formal terms.
[53] We know that Hill at least proposed a course entitled "Quantitative Analysis of Industrial Problems," to MIT's curriculum committee; see memorandum from R. A. Knight to Dean Brooks, 3/11/1942, *MIT*, AC 132, Box 12, "Operations Research – P. M. Morse" folder.

he was hesitant to do anything with OR and thus be seen as "usurping" what was naturally SIM's prerogative, Brooks' September memo seemed to "throw the ball back" to him. Since, Morse figured, OR had begun as an "application of methodology of *physical* science into problems of management," he acknowledged that the physical scientists "could be actively in the picture from the beginning here at Tech." That fall, with Stratton's authorization, Morse established an interdisciplinary committee on OR to coordinate research and supervise graduate work on OR-related topics.[54]

This committee was comprised of members of several departments, including physics, mathematics, the management school, mechanical engineering, electrical engineering and economics,[55] and its members encouraged students to undertake equally diverse coursework. In a 1953 survey he filled out for ORSA's education committee, Morse mentioned, in addition to the mathematics department's OR course, Machine Computation, Communication Theory, Social Psychology of Industry, Human Communication Networks, Group Organization, Econometrics, Technology of Industrial Control, and, of course, Probability and Statistics as courses relevant to an OR curriculum. The overall format of the graduate program seems to have emphasized the practical and applied aspects of OR familiar from the wartime tradition. The committee maintained a relationship with the Navy's OEG, and students were assigned local problems at MIT to work on, such as library circulation and parking. Sometimes they also worked on the projects with the consultants at Arthur D. Little.[56]

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[54] See memorandum from Philip Morse to Pennell Brooks, 7/10/1952; memorandum from Brooks to Morse, 9/16/1952; memorandum from Morse to Julius Stratton, 10/1/1952; memorandum from Stratton to Morse 12/15/1952 formally authorizing the establishment of the committee; all in *MIT*, AC 132, Box 12, "Operations Research – P. M. Morse" folder.

[55] Future Nobel Prize laureate Robert Solow represented the economics department on the committee.

[56] Philip Morse's reply to ORSA Education Committee survey, 1/5/1953, *MIT*, AC 4, Box 165, Folder 5; Philip Morse discusses the OR program at MIT in Morse, *In at the Beginnings*, chapter 9; on participation

Ph.D. dissertations done under the committee were supposed to be undertaken on behalf of *outside* authorities to give students experience in working in an actual operating context. The first student to receive a Ph.D. from the committee was John Little, a student of Morse's in the physics department. Initially interested in machine computation, he completed his dissertation on water management in the system of dams along the Columbia River in the Pacific Northwest.[57] The problem was to determine policies for storing and expending the water in the reservoir behind a dam for electricity generation during dry winter months and to minimize the need for supplemental electricity generation to fulfill the demands of the grid before spring melts created an effectively inexhaustible flow of water. These policies had to account for the fact that water was more valuable for electricity generation when the water levels were at their highest, since water would then gain the most energy going over the dam. Little approached the problem by adapting inventory models and Richard Bellman's dynamic programming methods to it, twisting the usual inventory control scenario by taking demand for electricity to be a constant, while supply, i.e. rainfall, was modeled with a stochastic distribution derived from a historical records of water flow that had been accumulated over the previous thirty-nine years.[58]

A notable aspect of Little's dissertation is its recognition of the rationales lurking behind extant policies. He was not simply making a model of a system that had been governed arbitrarily up until that point. He knew he was actually pitting his own models

at Arthur D. Little, Magee, interview with William Thomas, 4/11/2005. The education committee was chaired by Glen Camp, who had been one of the instructors involved with MIT's experimental course.

[57] Morse, *In at the Beginnings*, p. 296; on Little's initial interests, see Institute Committee on Operations Research meeting minutes, 5/27/1953, *PMM*, Box 2, "Committee on OR Minutes" folder.

[58] John Dutton Conant Little, "Use of Storage Water in a Hydroelectric System," Ph.D. dissertation, Massachusetts Institute of Technology, 1954; see also the shorter article, John D. C. Little, "The Use of Storage Water in a Hydroelectric System," *Journal of the Operations Research Society of America* **3** (1955): 187-197.

against existing dam operation guides, referred to as "rule curves". A rule curve dictated water management policies based on historically low levels of water accumulation. Little noted that, unlike in his model, cost functions were not included in the existing model beyond a tacit assumption that "large supplemental [electricity] generation is particularly bad," and that there was no "necessary", that is to say statistically rigorous reason why the curve should reach an optimal solution. Significantly, however, he did grant it validity, and suggested reasons why it had proved operationally effective: because costs were nonlinear, and because "dry years tend to have similar flow patterns." On this basis, he pointed out, "the high marginal costs in a dry year make it advisable to use available hydroelectric to smooth supplemental generation. Since dry years are somewhat alike, what is good for one may work well in another."[59]

Feeding his own models through MIT's Whirlwind I digital computer, Little found only little reason for triumph over the more arbitrary rule curve. If instituted over the thirty-nine years of operation for which records were available, his model produced an average of 1% cost savings over it. In some years the rule curve method actually proved superior. He felt, given the potential promised by advances in computing, that his model merited further development.[60] Meanwhile, the Columbia River Authority was in no rush to implement the computerized model.[61] It had been put to the test against a method derived out of simple expediency; and in this case, the result was close enough to be considered a draw for the moment. In 1955 Little received the first-ever Ph.D. in OR for his efforts, and moved on to the Army's Combat Operations Research Group before taking a position teaching OR at the Case Institute of Technology.

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[59] Little, "The Use of Storage Water" (1955), p. 195.
[60] *Ibid.*
[61] John Little, comment to William Thomas, 10/21/2003 at INFORMS Annual Meeting in Atlanta, Georgia.

Meanwhile, beginning in 1953, Morse's OR committee also took part in a broader outreach to industry through a series of summer courses held at MIT that it put together with help from the Navy's OEG and Arthur D. Little. The course lasted for three weeks, with each day divided up into morning and afternoon sessions. The morning sessions were devoted to lectures on longstanding topics of probability in OR as well as those quickly becoming part of the OR mathematical canon, such as linear programming and queuing theory. Afternoons were devoted to what were called "laboratory sessions," which stressed practical applications. The course participants were divided up into groups of four who were to become an OR team, while the instructor was to take on the role of the executive, with the object of instructing students how to arrive at a final analysis acceptable to both. An initial interview would take place between the mock OR team and the mock executive wherein the latter would lay out the nature of the operation and make note of the operational problems that were bothering him the most. It was then up to the OR team to formulate a research proposal. If the instructor found the proposal acceptable, he then furnished the team with data, but not, apparently, all the data that was requested. The experience was to be as real as possible.[62]

All of this took a fair amount of preparation from the instructors who took it upon themselves to familiarize themselves with the executive functions of a few different types of operations and to obtain data on them. Morse was an MIT administrator and a hotel manager. The mathematician George Wadsworth ran a warehouse, a restaurant, and a maintenance facility. Arthur D. Little's John Magee was an inventory manager, and the head of both a sales force and an advertising department. Others ran libraries, taxicab

[62] "Minutes of the Planning Committee for the 1953 Summer Session in O/R," 4/22/1953, *PMM*, Box 2, "Committee on OR Minutes" folder.

fleets (familiar from the OEG course), railroads, an airport, a power company, and so forth. They obtained actual data from their contacts in industry, and, in those cases where real data could not be obtained, it could be simply made up with an effort to make it as realistic as possible. In any event, the organizers seemed to feel it was important that the data not be "too pre-digested." OR teams would then have three days to work on the problem before reporting back to their instructor and the other teams before ultimately submitting a final report in writing.[63] The course apparently did not go entirely well since the OR committee's review of the course's second offering in 1954 observed that the second year's attendees were more professional, and that discussion was aided considerably by the lecture notes and problems developed during the previous summer.[64] The course continued in one guise or another for fifteen years.[65]

It was through the acts of curriculum development, graduate advising, and educational outreach to industry that OR at MIT began to take on a coherent pedagogical identity that was strongly influenced by Philip Morse's wartime experience. By the mid-1950s, his program was showing signs of staying. Meanwhile, on a national level, the alliance between mathematicians who proffered useful new optimization tools and OR's boosters had begun to secure OR a favorable reputation at large. In 1954 Tom Hill reported to Pennell Brooks that he and other professors at SIM felt the school should push forward with "quantitative analysis in what are now regarded as the 'non-technical' areas of industrial management." They saw the success of OR as evidence that industry was ready for such methods. While they recognized that "a substantial gulf exists

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[63] *Ibid.*
[64] Institute Committee on OR meeting minutes, 10/6/1954, *PMM*, Box 2, "Committee on OR Minutes" folder.
[65] Morse, *In at the Beginnings*, p. 292.

between the typical management and the theorist in autocorrelation, linear programming, game theory, and other such areas pertinent to industrial activity," they also felt that SIM was well-situated to "make an important contribution" in "the reduction of theory to practice." To accomplish these goals they recommended that the school set up a "research center" to oversee new methodological and pedagogical developments. They were forced to admit that the work proposed for this center was "in large part identical with those of the present Inter-departmental O/R Committee." Nevertheless, they believed that SIM would, henceforth, be the most appropriate venue for the kind of work performed by the committee.[66]

They were too late. While Hill seemed to believe that OR would be defined by its increasingly prevalent theories and that SIM would be a place where these theories would be distilled into the training of a new kind of manager, OR had, in fact, already made a strong claim to both theory and practice. Upon receiving Hill's new report, Morse acknowledged that he believed "the present Institute Committee on Operations Research [was] an interim measure, designed to get things started until S.I.M. has had a chance to decide what is needed for the long pull." He was glad to see that SIM had an increased interest, and wrote, "If you people are ready to pick up the ball, I stand ready to help where and when I can." However, in his work on the OR committee, he had also become convinced of the power of interdisciplinary work in OR. Hence, he thought, "it may be that the effort should never be entirely within S.I.M."[67] It never was. When the MIT Operations Research Center was established soon thereafter, it remained an independent

[66] Memorandum from T. M. Hill to Dean Brooks, 5/10/1954, *PMM*, Box 10, "ORSA Corres." folder. Thomson Whitin, whom we met in the last chapter as the author of a book in inventory theory, was a member of this group of professors.
[67] Memorandum from Philip Morse to T. V. Hill [sic], 5/14/1954, *PMM*, Box 10, "ORSA Corres." folder.

interdisciplinary organ of the institute that attracted major participation from SIM. Furthermore, although Morse appeared ready to cede leadership in OR to SIM in 1954, when the center was actually established he kept "the ball" for himself, becoming its director until his retirement in 1968. He was then replaced by his first OR student, John Little, who had since returned to MIT as a professor in what had become known as the Sloan School of Management.[68]

West Churchman, Russell Ackoff, and the Philosophy of Science

It's hard to recall how and why I moved my intellectual dwelling some half century ago from epistemology to management. The two questions, "What's wrong with logical positivism's theory of knowledge?" and "How many 15½-33 men's shirts should be kept in a retail store's shelves?" do seem a bit different, don't they?

C. West Churchman, "Management Science: Science of Managing and Managing of Science," 1994[69]

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[68] See Morse, *In at the Beginnings*, pp. 296, 355. SIM would, however, continue to attempt to find its own unique approach to management pedagogy, hiring the electrical engineer Jay Forrester away from MIT's Lincoln Laboratory in 1956 (partially with the backing of Edward Bowles, who was then consulting with SIM). Forrester would produce a new pedagogical program called "industrial dynamics" (later called "system dynamics"), a computer simulation method designed to help managers understand the systematic consequences of policy changes. He viewed this method as more powerful than OR's optimization techniques, because of its potential to identify and explore system behavior problems of importance to high level managers, but, ultimately, the method would not sharply distinguish itself from other computer simulation techniques. His first demonstration application was to none other than inventory and production control problems. See Jay W. Forrester, "Industrial Dynamics: A major breakthrough for decision makers," *Harvard Business Review* **36**/4 (1958): 37-66; Jay W. Forrester, *Industrial Dynamics* (Cambridge, Mass.: The MIT Press, 1961). Lambert Williams and I are currently preparing a paper focusing on the relationship between OR and industrial dynamics called "Cultures of Computing, Part I: Modeling Practice and Industrial Dynamics".

[69] C. West Churchman, "Management Science: Science of Managing and Managing of Science," *Interfaces* **24** (1994): 99-110, p. 99.

In the early 1950s, aside from Philip Morse's program at MIT, the only other major center for research and training in OR was the Operations Research Group in the Department of Engineering Administration at the Case Institute of Technology in Cleveland, Ohio. This group was set up by two transplants from the Department of Philosophy at the University of Pennsylvania named West Churchman and Russell Ackoff. Unlike virtually all the other OR supporters of that era, their interest in the subject did not stem from a desire to replicate a wartime OR experience into a peacetime context. Although Churchman had become familiar with Abraham Wald's work on sequential analysis while working at the Frankford Arsenal during the war, neither Churchman nor Ackoff had actually had any contact with the wartime OR groups.[70] Instead, they were attracted to OR as a means of putting into practice the "experimentalist" ideas of Churchman's mentor, Edgar Singer, Jr., a philosopher of science in the pragmatist tradition.[71]

Singer, Churchman, and Ackoff all eschewed the idea that science was defined by a constantly expanding body of knowledge. Instead, they preferred to define science teleologically around its method of solving problems. In their view science was something that occurred when a goal was explicitly stated, various means of achieving that goal were unscientifically hypothesized, and the alternatives were explored through experimentation in order to find the best possible option. They believed that scientific method differed from ordinary acts of problem solving in that it actively sought out the best or most "efficient" option rather than suffice with a less than optimal solution (what they termed "lag"), or even move from greater to lesser efficiency ("anti-lag") just for the

[70] On Churchman's wartime experience and its impact on his thinking, see Churchman, "Management Science," pp. 99-101.
[71] Singer had been a student of William James; see Churchman, "Management Science," p. 107.

sake of change.[72] So long as problem solvers strived to find the best method hypothesized, their work employed the scientific method. If they failed to seek improvement and sophistication in method, they lagged behind science or even worked actively against it.

Statistical inference was at the heart of Churchman and Ackoff's vision of methodological sophistication. In certain simple scenarios one hypothesis out of many might prove obviously superior, but, for them, advances in statistical methodology promised to revolutionize scientific methodology in general by increasing human ability to judge between competing hypotheses, even if the perpetual impossibility of absolute certainty in truth statements persisted. To demonstrate the point Churchman offered a simple example at the beginning of his 1948 book, *Theory of Experimental Inference*. He pointed out that the industrial problem of deciding whether a new and improved material is stronger than an existing material is ultimately resolved by performing a series of tests yielding results that might or might not appear to offer a resolution to the problem. A simple side-by-side comparison of test results might show the new material to be consistently stronger than the older one, but a large variance in the strength measurements of the new material might hint at some inconclusiveness in the test. Fortunately, he observed, advanced statistical methodology offered ways of distinguishing between the sets of data in a rigorous way by determining just how likely it was that the new material was actually stronger.[73]

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[72] See, for instance, C. West Churchman and Russell L. Ackoff, "Varieties of Unification," *Philosophy of Science* **13** (1946): 287-300, esp. 287-290; and C. West Churchman, *Theory of Experimental Inference* (New York: The Macmillan Company, 194)8, pp. 58-59; and, later, Russell L. Ackoff, *Scientific Method: Optimizing Applied Research Decisions* (New York: John Wiley & Sons, 1962).
[73] Churchman, *Experimental Inference*, chapter 1.

One of the points that statistical analysis—and especially the wartime development of sequential analysis—made apparent was that the act of deciding whether or not any given inference was true ultimately became a value judgment about just how certain the experimenter wanted to be that the statement was, in fact, true. For instance, because the only way to determine absolutely that the new material was in fact stronger was to conduct an infinite series of tests, experimenters were forced to define their tolerance of the risk that their conclusion would be in error. Defining such tolerances is always to an extent arbitrary, but in reality, Churchman and Ackoff hastened to point out, they are defined as much by human values as by a roll of the die. One had to ask, according to one's values, just how *important* it was that the correct conclusion had been reached. Because these decisions could have a clear ethical dimension—for instance, in determining the lethal dosage of a chemical—declarations of correctness and ethics could not be separated. Defining how confident one can be in determining a lethal dosage, and, likewise, whether it is better to overestimate or underestimate that value, is clearly determined by the value one places on preventing fatalities.[74]

In positing the interconnectedness of knowledge and values, Churchman and Ackoff acknowledged a certain debt to the relativist resolution of the frictions between rationalism and empiricism. However, they were also intent on not falling into the nonproductive viewpoint of simple skepticism that relativism implied. By building on pragmatist foundations, their experimentalism admitted that it had no firm fundamental basis for knowledge, but they also believed that knowledge could be made stronger by acknowledging the arbitrary assumptions and values that sustained it.[75] In fact, this

[74] Churchman, *Theory of Experimental Inference*, pp. 22-23, 247-251.
[75] *Ibid.*, chapter 9.

acknowledgement even presented an opportunity. In their view, the idea that problems, hypotheses, experimental investigations, human values, inferences, and ultimately decisions were all interwoven into a single framework meant that scientific method could be applied to problems in the natural sciences, the social sciences, policymaking, and even the study of ethics, all with equal validity. Because, in their minds, scientific inquiry only centered around the selection of best explanations of phenomena and best solutions to problems—not around definitively established facts—it was natural that issues such as just allocations of resources were as researchable as the properties of materials. In fact, as philosophers, Churchman and Ackoff were *especially* dedicated to the development of new ethical programs by means of scientific method. To them such a program did not mean finding an ethics that was somehow dictated by scientific principles,[76] but using science to study what values people held and to determine means of mediating between those values in ways that the they could agree were fair.[77] In this sense, Churchman and Ackoff's project was closely tied to the project of the decision theorists sketched out at the end of Chapter Four.[78]

What Churchman and Ackoff emphasized that the decision theorists did not was the need to establish valid means of experimentation. Even if one applied the most rigorous statistical methods to a data set obtained from an experiment, unless that experiment was set up in such a way that the data produced were directly relevant to the

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[76] Churchman later became quite explicit about this point; see C. West Churchman, *Prediction and Optimal Decision: Philosophical Issues of a Science of Values* (Englewood Cliffs: Prentice-Hall, Inc., 1961), pp. 26-27.

[77] See Russell L. Ackoff, "On a Science of Ethics," *Philosophical and Phenomenological Research* **9** (1949): 663-672; and also C. West Churchman and Russell L. Ackoff, "An Experimental Definition of Personality," *Philosophy of Science* **14** (1947): 304-332; and Churchman, *Experimental Inference*, pp. 236-247, wherein he discusses personality traits in terms of "lag" and "anti-lag" in efficiency to group or individual goals as operative definitions of personality traits.

[78] Beginning in the 1950s, Churchman and Ackoff began to cite decision theorists such as Kenneth Arrow, R. Duncan Luce and Howard Raiffa regularly in their writings.

evaluation of the hypothesis being tested, the whole activity was meaningless. In order to arrive at valid results, they stressed how necessary it was to take care that the problem and the various means set out to solve the problem be rigorously defined in terms of existing knowledge so that experiments would yield definitive results. Some hypotheses, for instance, could simply not be proven because limitations in prior knowledge prevented an experiment capable of correlating the ends with the suspected means from being designed. Therefore, the experimenter had to have a firm knowledge of extant experimental results to determine what sorts of hypotheses might at that time prove testable, and what sort of tests would produce valid results.[79]

Churchman and Ackoff felt that specialization was the most pressing problem facing experimental science faced after World War II. Because scientists worked increasingly in large teams of specialists, there was an acute need to understand how diverse points of view combined to design experiments that could produce valid results. In order to remedy this need, Churchman and Ackoff suggested that a way had to be found to reunify the different branches of scientific inquiry. They did not, however, countenance the "unification of science" movement still percolating in the postwar years, and strenuously resisted the logical positivist aspiration of creating a unified scientific language based on fundamental empiricist concepts.[80] Rather, they viewed the

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[79] See, for instance, Churchman, *Experimental Inference*, chapter 13, for a discussion of "applications of experimentalism".

[80] Churchman and Ackoff, "Varieties of Unification". In fact, much of their work was dedicated to moving beyond logical positivism, but in the annals of science studies their work has been neglected amid the uproar of other "anti-positivists" of the same era. Philip Mirowski has shown considerable interest in the relationship between philosophy of science and operations research, but seems to view OR as an outstretch of logical positivism, pointing, for instance, to the employment of Hans Reichenbach by the RAND Corporation. He lumps Churchman in with this group of "analytical" philosophers. Philip Mirowski, "The scientific dimensions of social knowledge and their distant echoes in 20th-century American philosophy of science," *Studies in the History and Philosophy of Science* **35** (2004): 283-326, esp. p. 301, as part of a section on "Reichenbach's philosophy for the operations researcher". Mirowski incorrectly suggests that

interrelation of disparate viewpoints as a prerequisite to advancement in the art of experimental design, pointing out, for instance that biology, psychology and sociology made a difference in the physical act of observation—a point, they noted, the nineteenth century astronomer Friedrich Bessel had discovered when he found that observers' different reaction times had an effect on the results of his experiments.[81] By Churchman and Ackoff's reckoning, because science was a method rather than a body of knowledge, no science was more fundamental than any other science. Rather, each science leaned on the others to further techniques of experimental design. The question was how these sciences could continue to do so in the face of mounting complexity of information.

At that time, Churchman and Ackoff looked to an institutional solution to the growing problems of scientific methodology, and proposed the establishment of a series of "Institutes of Experimental Method". These institutes, divided into four sections, would train methodologists—specialists in the issues of scientific methodology—who would serve as consultants in scientific work. Members of a "general methodology" section would seek to criticize and improve the methods of experimentation and the criteria used to determine the adequacy of proposed solutions to a problem. A mathematical statistics section would develop the insights of statistical theory and ensure that they were properly employed. Experts in a separate sampling techniques section

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any philosophy contained in OR strove to separate science from society, thereby representing an explicit departure from John Dewey's philosophy of science and society. However, this is the exact view that Churchman, as an experimentalist and a philosophical descendant of Dewey, constantly argued against. As for Reichenbach, his association with the RAND Corporation should not be taken as a sign of any important affiliation with the OR community. Mirowski's article is a broadside against recent philosophers of science (especially Philip Kitcher), chastising them for their claims to novelty with respect to the "social dimension of science" as "historically inaccurate" (p. 284). This point has merit, but Mirowski seems to have fewer bones to pick with sociologists who have been at least as enthusiastic in proclaiming their own discoveries that science and society cannot be separated.

[81] In their teleological definition of science, Churchman and Ackoff dismissed the idea that one science was more fundamental than another; see Churchman and Ackoff, "Varieties of Unification," p. 297.

would help formulate the "presuppositions" behind sampling techniques so that they could also be scrutinized. Finally, in recognition of the fact that scientific work does not always remember why it has taken the paths that it has, a history of science section would investigate the ways scientific investigations of the past influenced the ways current scientific inquiries were framed and would help determine what aspects of past scientific work could be revived most fruitfully in the present.[82]

As a first step toward setting up their institutes, Churchman and Ackoff helped organize a series of conferences on issues they saw as of particular methodological significance. They held their first conference in May 1945, covering the fields of statistics, psychology and physics. Their second conference, on the "measurement of consumer interest", was held the following May. This second conference was directly related to the problems of advancing knowledge in socially beneficial ways that so concerned them. As they (with Murray Wax, another Penn philosopher) wrote in the introduction to the conference proceedings, because the very notion of measuring consumer interest was so foggy, it was a model problem for philosophers interested in the development of scientific methodology to attack. By bringing together "methodologists", statisticians, sampling experts, psychologists, and marketing researchers, they hoped it would provide a template for how to "make all fields of research more self-conscious," and would help "determine the most fruitful steps to be taken toward making research scientific…." This goal, they remarked as an aside, was "similar to that of 'operational

[82] C. West Churchman, Russel L. Ackoff, Murray Wax, "Introduction," in *Measurement of Consumer Interest*, edited by Churchman, Ackoff and Wax, 1-7 (Philadelphia: University of Pennsylvania Press, 1947), pp. 4-6. A discussion of these institutes in Churchman, *Experimental Inference*, p. 234, does not mention the history of science section.

analysis' discussed by Professor Wilks" who had had been invited to participate in an informal panel on the "specifications for consumers' goods".[83]

As we saw in the last chapter, Samuel Wilks, a mathematical statistician at Princeton and a member of both the U. S. Navy's Anti-Submarine Warfare Operations Research Group and the Applied Mathematics Panel, was also a key figure in the postwar push to establish OR outside of the military in America.[84] In a talk entitled "Research on Consumer Products as a Counterpart of Wartime Research," he professed his ignorance of the field of consumer research, but suggested that the goals of the activities of the Navy's OR group and other "mixed teams" of OR personnel had important commonalities with the interests of the attendees of the conference.[85] Pointing out that during the war the industrial power of Britain and America had been "geared to the training of men and the production of war materiel," he related how OR groups "studied the effectiveness of these products for the purpose for which they were designed, by going to the theatre of operations and working with the users of the products—the men in combat." Now that peace had arrived and there was "a huge segment of [the] economy geared to manufacturing consumer products," and a new emphasis had arisen around the "problem of studying the effectiveness with which the peace-time consumer products, such as refrigerators, automobiles, and electric appliances, are doing the job they are supposed to do." To get a handle on this kind of problem, Wilks thought it was necessary

[83] Churchman, Ackoff and Wax, "Introduction," p. 3.

[84] His talk at Churchman and Ackoff's conference took place a year and a half prior to his two session panel on OR at the joint conference of the American Statistical Association and the Institute of Mathematical Statistics.

[85] Wilks employed the British terminology, referring to "operational analysis," "operational research" and even the "Navy Operational Research Group". It is not possible to say whether the term "operational analysis", coincidentally also used by Percy Bridgman to represent his approach to "analyzing and giving meaning to the concepts of physics", had anything to do with Churchman and Ackoff's interest. See P. W. Bridgman, "Operational Analysis," *Philosophy of Science* **5** (1938): 114-131, p. 114; which is cited in Churchman, *Experimental Inference*, but not in the context of military OR.

to develop appropriately "scientific" means of polling, which meant understanding how the interview process impacted the information received from consumers.[86] He spent the remainder of his short talk discussing the need for cooperation among businesses, universities, and various "agencies and organizations" in order to promote the social sciences and improve these methods—a popular topic in the immediate postwar years.[87]

Wilks' talk was not especially informative on either wartime OR, or consumer research, but it did strike a chord with Churchman and Ackoff. Their philosophical ideas, which recognized no barriers between practical problem solving and scientific inquiry, accorded well with the idea that military operational planning and doctrine-building processes could be improved by subjecting them to the systematic scrutiny and statistical investigation by scientists.[88] However, OR did not immediately transform their careers.[89] While the American effort to bring OR out of the military remained in stasis in the late 1940s, the two philosophers continued to press their own program, even as they moved from Penn to Wayne University in Detroit. In 1947 they held two more conferences under the Institutes of Experimental Method rubric, one on city planning, and another on "service efficiency of settlement houses". Ackoff also began pressing for universities to set up methodology departments, which would not only train methodologists, but also alert science students to methodological concerns, and inform non-scientists about the

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[86] We will recall that wartime OR groups emphasized the importance of paying close attention to methods of data collection to ensure the data from reports and interviews actually meant what it was presumed to mean. In Chapter One we discussed the evaluation of the usefulness of reporting forms. Morse and Kimball addressed the issue of report form and interviewing validity in the conclusion of their postwar report, "Methods of Operations Research," p. 146. As we will see in chapter six, there were individuals in Britain who, coincidentally, also related OR to consumer research.

[87] S. S. Wilks, "Research on Consumer Products as a Counterpart of Wartime Research," in *Measurement of Consumer Interest*, edited by Churchman, Ackoff and Wax, 135-138 (Philadelphia: University of Pennsylvania Press, 1947).

[88] On this point see especially the later Ackoff, *Scientific Method.*

[89] Ackoff confirms, however, that Wilks' presentation was the original source of his and Churchman's interest in OR; email from Russell Ackoff to William Thomas, 6/7/2006.

capabilities of science. Like the institutes, the department would also offer consulting services to researchers and sponsor conferences on "pressing" scientific issues.[90]

By the time Churchman and Ackoff moved to the Case Institute of Technology in 1951, however, they had decided to ally their own philosophical program with the nascent profession of OR. Churchman had joined the Department of Engineering Administration as a visiting professor to undertake a statistical sampling study for the Chesapeake and Ohio Railway Company, which, for reasons apparently unrelated to Churchman's interest in the subject, had agreed to sponsor a professorship in OR in the department.[91] At Case, Churchman and Ackoff organized the first industry-oriented conference on OR that November, and established both a consulting service and the first full-scale educational program in OR there the same year. Even as they appropriated the guise of OR, however, they also viewed their work as a practical extension of their prior philosophical visions. A 1957 report on their program published in *Operations Research* reveals that the Case vision for undergraduate education in OR included courses in OR itself, but also a four-semester Introduction to Management, the History of Science and Technology, Measurements for Management, Organization Structure and Operation, Data Processing and Computers, among other offerings. The graduate curriculum, meanwhile, was also comprised of a combination of elements of their methodological program and applied mathematical techniques that were increasingly associated with OR. These courses included Methods of Operations Research; Problems in Operations Research;

[90] Russell L. Ackoff, "An Educational Program for the Philosophy of Science," *Philosophy of Science* **16** (1949): 154-157.
[91] Burton V. Dean, "West Churchman and Operations Research: Case Institute of Technology, 1951-1957," *Interfaces* **24** (1994): 5-15. See also C. West Churchman, "The application of sampling to LCL interline settlements of accounts on American railroads," *Handbook of Industrial Engineering and Management*, edited by William Grant Ireson and Eugene L. Grant (Englewood Cliffs: Prentice Hall, 1955), pp. 1051-1057.

Sampling Theory Applied to Industrial Problems; Scientific Method; Costs, Utilities and Values; Computers in Operations Research; Mechanization of Management Decision Processes; Production and Inventory Control; Operations Research Seminar (Advanced Topics); Theory of Games; Mathematics of Management Systems; and Linear Programming, which were "supplemented by related courses in mathematics, statistics, physical science, engineering, and management."[92]

It is not possible to evaluate precisely how responsible West Churchman and Russell Ackoff were in defining the intellectual content of OR and management science in the 1950s, but their participation was clearly central. They were both founding members of ORSA. Ackoff was the president of ORSA for 1956, while Churchman was the founding editor of *Management Science* and the president of TIMS for 1962. A paper by Ackoff entitled "Some New Statistical Techniques Applicable to Operations Research" appeared in the first issue of ORSA's journal alongside Bernard Koopman's talk on new mathematical methods (discussed in the last chapter).[93] In accordance with the fact that Churchman and Ackoff's philosophy was indebted to statistical methodology for determining "efficient" choices among hypotheses, they also proved strong supporters of the development of a mathematical canon for OR throughout the 1950s. Because their ideas about scientific method also focused on real experimental setups, they were deeply committed to the development of OR practice as well. Their pedagogical program, like Morse's, emphasized gaining experience in application through the Case OR Group's consulting activities. Like Morse, they also sponsored short courses on OR, and many of

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[92] E. Leonard Arnoff, "Operations Research at Case Institute of Technology," *Operations Research* **5** (1957): 289-292. The OR Group was about to award its first Ph.D. in OR that year.
[93] Russell L. Ackoff, "Some New Statistical Techniques Applicable to Operations Research," *Journal of the Operations Research Society of America* **1** (1952): 10-17.

their students were professionals from industry taking evening courses.[94] Both the theoretical and practical elements of OR were also stressed in their 635-page 1957 textbook, *Introduction to Operations Research*, which was based on their short course, and was co-written with their colleague Leonard Arnoff.[95]

Although professionally Churchman and Ackoff had diverged somewhat from their initial aims of providing methodological assistance to scientists, they remained deeply committed to their philosophical vision, and doubly so to the scientific study of ethics. Throughout the 1950s, they were probably the most consistent link between decision theory and operations research. In the early issues of ORSA's journal, for instance, they became embroiled in a formalized debate with authors working in the Army's Operations Research Office (which, like RAND, often supported more theoretical work) over whether an experimentalist attempt to measure "absolute values" was a better approach to considering values from a "relative and arbitrary" standpoint as "constructs of human rationalization."[96] These kinds of issues were related to their interest in OR. Churchman and Ackoff developed a keen interest in inventory theory, for instance, precisely because an effective inventory policy involves balancing the desires of individuals or departments with conflicts of interest in ways that are just from the higher perspective of the good of the firm—a point we saw earlier from the perspective of the

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[94] Arnoff, "Operations Research at Case".
[95] C. West Churchman, Russell L. Ackoff, and E. Leonard Arnoff, *Introduction to Operations Research* (New York: John Wiley & Sons, 1957).
[96] C. West Churchman and Russell L. Ackoff, "An Approximate Measure of Value," *Journal of the Operations Research Society of America* **2** (1954): 172-181; which was a response to Nicholas M. Smith, Jr., Stanley S. Walters, Franklin C. Brooks, and David H. Blackwell, "The Theory of Value and the Science of Decision: A Summary," *Journal of the Operations Research Society of America* **1** (1953): 103-113. Quotes are from Smith's counterresponse, Nicholas M. Smith, Jr., "Comments," *Journal of the Operations Research Society of America* **2** (1954): 181-187, pp. 181-182.

management consultant.[97] In Churchman's and Ackoff's minds, gaining a foothold on these kinds of issues lent insight into broader problems of evaluation and mediation of higher ethical values that were not measured simply in terms of dollars. Accordingly, they also maintained the interest in the empirical problems of social science, such as measurement of public welfare, that had originally inspired their conference on the measurement of consumer interest.[98]

Although both Churchman and Ackoff were sympathetic to the wartime tradition of OR that held executive policies to be an object of investigation, they were not content to let their commonalities simply be commonalities. They pressed for OR to be not only science *in aid* of management, but to be the science *of* management, which meant that operations researchers should consider *all* of the problems of management. Because they knew every business problem contained an element of public policy, they felt operations researchers would not be solving the real problems faced by managers unless they addressed the ethical components of management as well. They would likewise "lag" behind the scientific vanguard if they contented themselves with the immediate set of problems their methods were most capable of handling and disregarded ethics, just as natural scientists would ultimately run up against a wall and find their methods sterile if they failed to take into account the contributions of other fields to the act of experimentation.[99] In Churchman and Ackoff's view, if operations researchers wished to

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[97] Churchman, Ackoff, and Arnoff, *Introduction to Operations Research*, pp. 4-7 describes the "general nature of operations research" as the study of "executive-type" problems, which involve conflicts of interest within an organization; inventory theory is used as an example. Churchman, *Prediction and Optimal Decision: Philosophical Issues of a Science of Values*, p. 314, uses an inventory and production control-type problem to illustrate an issue in the "values of social groups".
[98] Notably, see Russell L. Ackoff, *The Design of Social Research* (Chicago: University of Chicago Press, 1953).
[99] On the necessity to integrate ethics into scientific research, see Churchman and Ackoff, "Varieties of Unification," pp. 293, 296. Churchman, *Experimental Inference*, p. 250 puts the problem this way: "The

remain scientists, they would have to move forward—even if it meant leaving behind the methods that had distinguished their postwar contributions to industrial decision making.

Most operations researchers, of course, shared neither Churchman and Ackoff's philosophy of science, nor their views about OR as an all-encompassing science of management in all its various facets. For most of the members of ORSA and TIMS, there was a point where operations research and management science ended and management began, and, if the matter even crossed their minds, they did not feel it was their role to enter into the broader ethical questions that the two philosophers pressed, if only because they had no means of making a unique contribution to these kinds of problems. We will return to this view at the end of this chapter. For their part, Churchman and Ackoff became increasingly disillusioned with the field they had so opportunistically joined in the first place. In their minds, by the late 1950s, OR seemed to be failing to tackle more than a small subset of problems, which were not the most pressing ones for high level managers, and believed that operations researchers were shying away from these problems in favor of an endless refinement of their techniques that were only appropriate for less important and less risky problems.

Specialization, we will recall, was exactly what Churchman and Ackoff felt most threatened scientific inquiry, as they defined it, and they became restless with their status as members of the OR profession. Churchman left Case for the School of Business Administration University of California at Berkeley in 1957, where he established the Center for Research in Management. He remained there for the rest of his career. In

science of ethics… must on the one hand belong to experimental science, and yet not be an aspect of any of the special disciplines now recognized. The science of ethics must, in other words, be subject to all the checks experimental science imposes on its fields of investigation; it must be subject to all the conditions of experimental control. At the same time, the science of ethics will have pervasive influence throughout all the sciences, from logic to sociology."

1964 Ackoff took the entire Case OR Group to the Wharton School at the University of Pennsylvania, where it joined with the Department of Statistics.[100] However, facing faculty resistance against his efforts to expand beyond OR into broader, less mathematical areas of "systems thinking", Ackoff would leave the department and set up his own academic program in "Social Systems Science". Finally, in the late 1970s, citing a diminished status in OR at universities and in the organization of companies, he took it upon himself to pronounce OR scientifically dead and to announce that he no longer considered himself a part of the profession.[101] The OR community did not take much notice.

The Reconfiguration of Systems Analysis at the RAND Corporation

While West Churchman and Russell Ackoff could take up and leave operations research as it did and did not suit their notions about the relationship between the analyst and the manager, the RAND Corporation was in no such position with respect to systems analysis. Following the failure of the Air Force to accept the conclusions of Edwin

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[100] The Wharton School is the University of Pennsylvania's business school. The OR program also took over the existing Management Science Center at the school.

[101] Churchman, "Management Science," demonstrates this keen sense of disappointment. Russell L. Ackoff, "OR: after the post mortem," *System Dynamics Review* **17** (2001): 341-346, recounts both his frustrations and his departmental moves. His declarations of the death of OR can be found in Russell L. Ackoff, "The future of operational research is past," *Journal of the Operational Research Society* **30** (1979): 93-104; and again in Russell L. Ackoff, "President's Symposium: OR, a post mortem," *Operations Research* **35** (1987): 471-474. Historical discussion of Ackoff's (and others') disaffection with OR can be found in M. W. Kirby, "The intellectual journey of Russell Ackoff: from OR apostle to OR apostate," *Journal of the Operational Research Society* **54** (2003): 1127-1140; and Maurice W. Kirby, "Paradigm Change in Operations Research: Thirty Years of Debate," *Operations Research* **55** (2007): 1-13. Kirby does not delve into Churchman and Ackoff's philosophy of science beyond noting their ethical commitments and devotion to "systems" thinking. Both have tended to be portrayed as voices of conscience amid an otherwise ethically-blind mathematical profession, and hence as necessary reactions of the more socially conscious 1960s and '70s to the stiff conformity of the 1950s; see especially Thomas P. Hughes, *Rescuing Prometheus* (New York: Vintage Books, 1998), pp. 192-194; and Waring, *Taylorism Transformed*, pp. 38-42. I believe this attention to Churchman and Ackoff's disaffection is overstated. At the end of this chapter, I offer an alternative view of this disaffection, which accords more with a rote refusal to recognize the historically-defined limitations of operations research as a legitimate profession vis-à-vis the intellectual terrain of fields such as economics, management consulting, and, indeed, management itself.

Paxson's analysis of the desired characteristics of the B-52 bomber, as described in chapter three, RAND was left with a deeply flawed signature intellectual product. Although systems analysis did represent the least arbitrary approach to equipment design then available, it was packaged as an all-or-nothing approach to the topic, which took no account of the fact that most design decisions took place in stages as research and development progressed and strategic requirements and budgeting limitations changed and became better defined. Consequently, rather than take RAND's full systematic inquiry as a finished product, the Air Force and aircraft manufacturers cannibalized the study, integrating some of the expectation values it was able to derive into their own designs. (Figure 5.1 shows survival rate curves derived from Paxson's work that were used by Convair in its unsuccessful pitch of the "swept wing" version of its B-36 bomber to the Air Force.[102]) The ability of equipment designers to integrate their work into the Air Force's operational planning was clearly augmented by this kind of work, but RAND's analysts had aimed to deliver a study that was useful as a synthetic whole.[103] The entire point of doing a full-scale systems analysis, after all, was that partial analyses might prove misleading in light of broader issues.

[102] This aircraft was eventually dubbed the YB-60, but was not pursued by the Air Force due to its clear inferiority to the B-52. Until historians procure records from the Air Force or even the aircraft companies themselves, it will not be clear just how deep a role these studies played in the design process.
[103] David A. Jardini,"Out of the Blue Yonder: The RAND Corporation's Diversification into Social Welfare Research, 1946-1968," Ph.D. dissertation, Carnegie Mellon University, 1996, p. 69, draws our attention to the industry's interest in "myriad component analyses," but he leaves the nature of these analyses unclear. The survival rates plotted in Figure 5.1 represent a relatively integrated level of analysis, anticipating battle outcomes on the basis of physical capabilities.

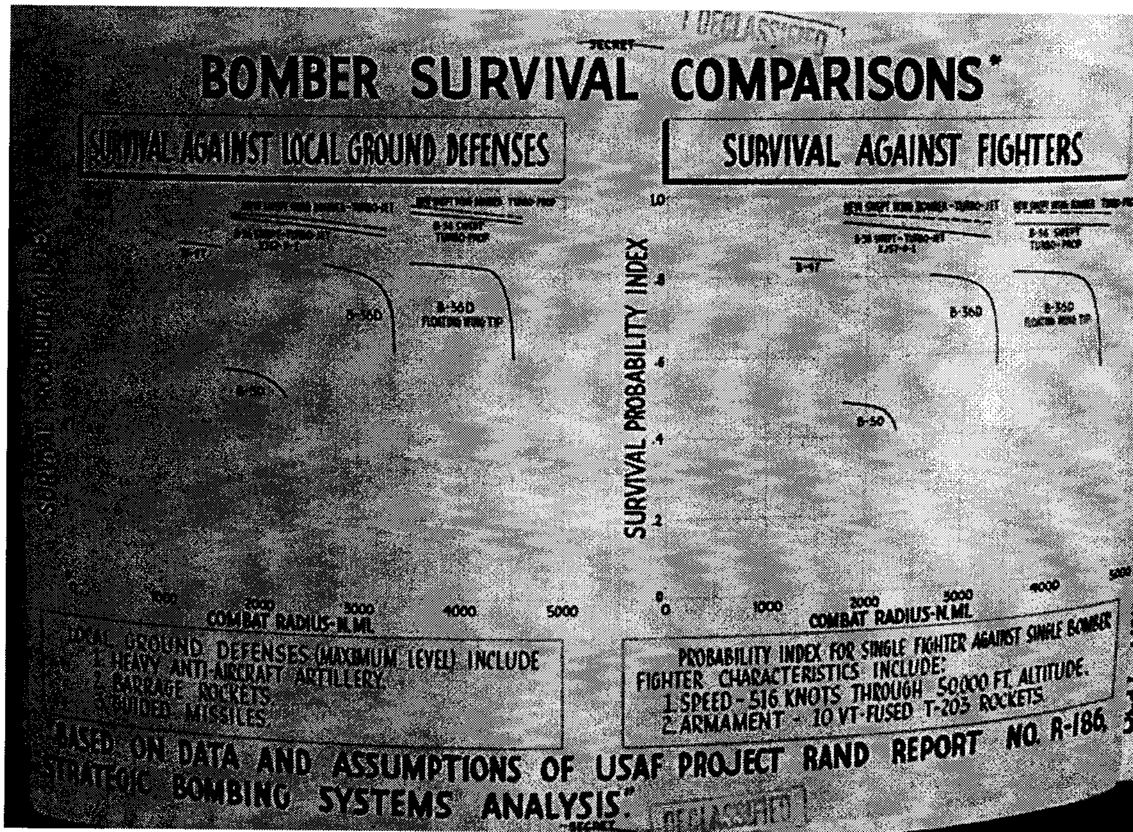

Figure 5.1. A chart comparing anticipated survival rates for potential bombers of varying range capabilities against defenses of certain attributes. Source: Convair Swept Wing B-36 Program summary, 11/11/1950; *CEL*, Box B96.

RAND's follow-up to the Strategic Bombing Systems Analysis, called the Air Defense Study, was a detailed evaluation of American defense capabilities against an anticipated Soviet strike that was directed by another RAND analyst named Edward Barlow and completed in 1951.[104] It is important to note that the generalized "study" was different from a "systems analysis," which continued to connote Paxson's style of appraising competing system design characteristics through an expansive quantitative

[104] "Air Defense Study," RAND Document R-227, 10/15/1951, RAND Corporation Library, Santa Monica, CA. Jardini, "Blue Yonder," pp. 64-69 indicates the study's major accomplishment was that it "avoided a recurrence of Paxson's humiliation," but that it "did little to establish the credibility of systems analysis among Air Force leaders" (p. 69). Bruce L. R. Smith, *The RAND Corporation: Case Study of a Nonprofit Advisory Corporation*, Cambridge, Mass.: Harvard University Press, 1966, p. 90 suggests that while MIT's Lincoln Laboratory became the main research center for air defense, "RAND continued to do some air defense research, but of a background and analytical nature. Some of the recommendations [...] led to important changes in the armament used in interceptor aircraft, radar support equipment, data-handling techniques, and other aspects of air-defense operations in the 1953-1957 period." In order to gauge the status of RAND in this period effectively, historians will require detailed empirical research concerning to what purposes a study was likely to be put, and to what ends RAND's analysts expected it to be put.

analysis. In this case, what was called the Defense Systems Analysis constituted a substantial "numerical phase" within the overall qualitative framework of the study. Situated within this framework, the Defense Systems Analysis was intended to serve as a rigorous guide to the overall problem of defense against large-scale air attack, bringing to light what aspects of defense were likely to be critical and what factors turned out to be less significant than initially imagined, as only mathematics could reveal. However, departing from the tradition of L. B. C. Cunningham and Edwin Paxson, the new study also aimed to provide its readers with some guidance as to how the quantitative aspects of the study had been formulated, pointing out where its presumptions were especially arbitrary, in order to allow new information or different assumptions to be more easily integrated into a revised version of the analysis.[105] The overall study also situated itself within the context of a broader policy debate, pointing out that many of its conclusions simply reinforced those already arrived at by "other agencies," and were included because they represented "documentation and corroboration of these other investigations," but the study also brought to light new factors to be considered inside of broader policymaking processes. As we have seen in several cases with OR, reproducing known results with a more rigorous basis was seen as a sign that confidence could be placed on aspects of the work that went beyond what was already known or that disagreed with more intuitive results.[106]

As opposed to Paxson's systems analysis, I would argue that the Air Defense Study was written with the policymaking process in mind, rather than as a rationalized design for the Air Force and its contractors to follow. It, like much of RAND's activity

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[105] "Air Defense Study," chapter 3, sections IV and V on "The Defense Systems Analysis: The Study's Numerical Phase," and "Some Limitations of the Defense Systems Analysis," respectively.
[106] "Air Defense Study," chapter 2, section VI.

in the 1950s, can be read as an attempt to grapple with how one development and design program can be considered better than another without pretending to understand them in all their fine details. The idea of *time* entered the analysis. Paxson's analysis, with its World War II heritage, had presumed that one decision about aircraft design had to be made at one point in time containing all the rational expectations available and arbitrary assumptions necessary to complete that design. In reality, in a world where war might occur in a year, or it might never occur, only some decisions had to be made right away, while others could wait until later. At the meta-calculative level, if one spent three years designing a very detailed study, the circumstances surrounding that study might change. Studies themselves had to be designed with the fact that one had to obtain as much useful knowledge as possible in a short span of time. Ultimately, yes, design decisions had to be made in such a way that encompassed both rational and arbitrary elements, but those final designs would be shaped and reshaped by any number of decisions made over the course of time. The question thus became: what constituted the most rational way to make a phased series of decisions, and how could RAND help make this process more rational? The Air Defense Study's looser structure was clearly one response to this challenge.[107] However, the burgeoning coterie of economists at RAND also had their opinions on the matter.[108]

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[107] Conceptually, this realization is similar to that suggested by RAND mathematician Richard Bellman's dynamic programming technique, but whether there is any substantial connection remains uncertain.
[108] The contributions of economists have been discussed in Jardini, "Blue Yonder," pp. 107-113; Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (New York: Cambridge University Press, 2002), pp. 397-402; and, above all, in the highly useful David A. Hounshell, "The Medium is the Message, or How Context Matters: The RAND Corporation Builds an Economics of Innovation, 1946-1962," in *Systems Experts, and Computers: The Systems Approach in Management and Engineering, World War II and After*, edited by Agatha C. Hughes and Thomas P. Hughes (Cambridge, Mass.: The MIT Press, 2000), pp. 255-310, esp. pp. 255-270. Much of what follows accords with their arguments. What is new is my assertion that the economists' critiques of systems analysis misinterpreted it as a technique of policy rather than a technique of design, and were thus off the mark. While systems analysts followed the

When John Williams began considering hiring economists and other social scientists for Project RAND in the late 1940s, a RAND physicist named Dana Bailey, who was a friend of Williams from the University of Arizona, told him about an American economist teaching at Oxford named Charles Hitch. Hitch, another Arizona alumnus, had originally gone to Oxford as a Rhodes Scholar, and had been elected a don at Queens College in 1935. In 1941 he joined the war effort, undertaking studies of British wartime materiel controls at the Lend-Lease mission in London, before moving to the War Production Board in Washington, DC. Shortly thereafter, he was drafted and assigned to the Office of Strategic Services and was sent back to Britain to become the deputy head of the joint American-British RE-8 division of the Ministry of Home Security at Princes Risborough, which was assessing the impact of air raids over Germany.[109] He then did similar work for the Joint Target Group at the Pentagon, assessing the effects of air raids on Japan, before finally returning to Oxford at the war's end. Bailey, who had met Hitch when he himself had been a Rhodes Scholar at Queens College, thought that he would be a good fit for RAND. Much to Williams' surprise, they managed to recruit him, and he became the head of RAND's new economics department in July 1948. Over a decade later, after Hitch had left to become the Comptroller in Robert McNamara's Pentagon, Williams reflected, "I'm still so pleased about getting Hitch that I can hardly contain myself. I think that of all the men that have

line of the critique, abandoning the original alliance with equipment design, I would argue that this move was already underway with the move from the Strategic Bombing Systems Analysis to the more flexible "study" orientation of the Air Defense Study. As I will argue here, the systems analysts largely kept their program of comparing weapons systems on an empirical basis, which the economists argued needed to be abandoned. in favor of economic analysis.

[109] RE stands for Research and Experiments. RE-8 is best known for its surveys of German bombs on British cities under the direction of zoologist Solly Zuckerman and the crystallographer J. D. Bernal. Although we do not know the dates of Hitch's service there, they had probably left by the time Hitch arrived.

been at RAND, a list which by now has contained hundreds of my peers and betters, to my mind Charlie is still the number one boy…."[110]

At RAND, Hitch's career stood on a borderline between economics, policymaking, systems analysis and operations research. In the 1950s, certain branches of the economics profession, most notably at the Cowles Commission, were becoming increasingly bound to formulating objective models of economic behavior, which we briefly discussed in the last chapter. Hitch explicitly rejected the stance. Casting himself in the tradition of Adam Smith, David Ricardo, Alfred Marshall and John Maynard Keynes, he saw economics as a normative profession. His emphasis on analyzing policymaking decisions would bring him into kinship with the nascent OR profession, and, in fact, he was president of ORSA in 1959.[111]

The primary theme running through Hitch's work in economics and policy is the tension between formality and calculability. In 1949 Hitch wrote an internally circulated RAND paper entitled "Planning Defense Production" that grappled with the difficulty of relating powerful analytical techniques such as Wassily Leontief's input-output analysis, linear programming, and game theory to problems of military budget allocation—a separate question from Paxson's study, which was then in progress. We will not go into the allocation problem here, except to point out that while surveying the potential behind calculating "military worth" and engaging with the "philosophy of game theory," he warned,

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[110] See interview with Charles Hitch, 2/9/1988, *NASM*, RAND Oral History Project, pp. 1-13;and Vaughn D. Bornet, "John Williams: A Personal Reminiscence (August, 1962)," RAND Document D-19036, 8/12/1969, RAND Corporation archives, John D. Williams collection, Box 1, pp. 30-33, quote on p. 3.
[111] See Charles Hitch, *The Uses of Economics*, an address given at the dedication of the Center for Advanced Study of the Brookings Institution on 11/17/1960, printed in RAND P-2179-RC, which was distributed outside RAND.

> I am not, however, proposing a grandiose system of Walrasian equations—game theoretic or otherwise—to solve the whole allocation problem. Such a system may or may not be a desirable ultimate objective: within the foreseeable future all that we can hope to do is to expand the area of "rational" decisions and reduce the element of "judgment" or hunch in the planning process.[112]

Here again was the RAND mantra posing rationality in a constant struggle against arbitrariness. Instead of trying in vain to eliminate arbitrariness through excessively large models of military budgets, he suggested it was better to design a "satisfactory general framework of analysis" and then solve "a multiplicity of sub-optimization problems at various lower levels."[113]

Hitch's approach was predicated on his suspicion that "the efficiency of the defense system may be more sensitive to the lower level decisions than to the high level ones." Sub-optimization rejected the idea that the overall efficiency of a large system, such as an economy, ultimately depended on the macroscopic arrangements of its parts (think of the transportation problem). Rather, so long as low level parts tended to function in a way consistent with higher level goals, finding their most optimal macro-configurations was unnecessary.[114] Although Hitch apparently initially intended sub-optimization to apply to more general questions of efficient budget allocations, it increasingly became a critique of systems analysis as well. His first public discussion of

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[112] Leon Walras was a French economist who modeled the economy as an auction in which schedules of prices and demands were posted at intervals. Between intervals these schedules, in a process known as *tatonnement*, were assessed against inventories and demands, and changed before the next auction. Game theory, which was mathematically equivalent to the optimizing technique of linear programming, promised potential benefits by calculating, in conjunction with economic formulations, such as Wassily Leontief's input-output charts, efficient resource allocations such as the ones at which Walras' theoretical auctions were supposed to arrive. The attachment to Walrasian formulations drives Mirowski, *Machine Dreams*, and further elucidation of debates occurring at this time surrounding such formulations is necessary.
[113] Charles Hitch, "Planning Defense Production" RAND P-105, 9/29/1949, RAND Corporation library, Santa Monica, California. Anticipating the trauma to be caused by McNamara's imposition of new RAND-designed budgeting methods on military procurement over a decade later, his paper included the warning, "In one respect I propose to do violence to the facts of American political life; I assume that appropriations for defense can be freely transferred, within the limits of the total defense budget, from any one category of defense expenditure to any other."
[114] Hitch, "Planning Defense Production".

sub-optimization was at the first full meeting of the ORSA in 1952, wherein he discussed the need to apply consistent economic criteria of analysis to component optimization studies, and pointed out how important it was to perform optimization with the component study's larger context in mind. In this talk, Hitch became one of the first of a few but growing number of people actually to identify OR with mathematical optimization—an impression garnered partially from the mathematical content of Morse and Kimball's *Methods of Operations Research* and partially from the cost minimization problems of industry and systems engineering that were apparently going to become constitutive of industrial OR work.[115] Significantly, he had had no exposure to the wartime tradition of OR.

Although Hitch stressed the need to look at the larger picture, he was, of course, skeptical about RAND's use of sprawling, mathematical systems analyses, which he saw as an ill-conceived attempt to obtain an "*optimum optimorum*".[116] In his 1960 book, *The Economics of Defense in the Nuclear Age*, which he co-wrote with his frequent collaborator at RAND Roland McKean, he directly addressed the kind of problem that had set Paxson along his own path. Rehearsing the familiar problem of selecting a "'best' gun sight", Hitch and McKean observed how an analysis can quickly balloon outward, "fitting the gun sights into planes, then the planes into fighter groups, then the groups into relevant military operations." The analyst might even "want to ask what tasks or budgets make sense in view of the whole military operation and of political

[115] The talk was published in the third issue of ORSA's journal: Charles Hitch, "Sub-optimization in Operations Problems," *Journal of the Operations Research Society of America* **1** (1953): 87-99. See Thornton Page, "The Founding Meeting of the Society," *Journal of the Operations Research Society of America* **1** (1952): 18-25, for a discussion of industrial problems in cost minimization identified as OR at that meeting.
[116] Hitch, "Sub-optimization," p. 98.

realities." At some level systems analysis and economics eventually came together. Selecting a criterion for system optimization became easier the further one expanded the problem, but the analysis would eventually break down under limits in investigative resources, computational power, the ability of the analyst to influence decision making, the time available for study, and even the ultimate lack of importance at the highest level of which gun sight was actually chosen. A line where calculated results ceased being more rational than arbitrary had to be (arbitrarily) drawn somewhere, thereby accepting "a limited context" of study validity and keeping aware of the "shortcomings and biases" of the analysis "in a general and qualitative way". Ultimately, the analyst had to "get on with his inevitable job of sub-optimizing."[117]

Meanwhile, though, the problem that systems analysis had been created to address—aligning increasingly complicated equipment designs with increasingly uncertain expectations of military requirements—was not going away. Design was an expensive process involving the production of specially-built prototypes, and, according to a 1953 memorandum written by RAND systems analyst Edward Quade, the Air Force's contractors were becoming increasingly agitated that they were not being adequately compensated for design and development work that did not lead to a procurement contract. Despite its flaws, systems analysis still seemed to promise a fruitful approach to this problem. Some industry representatives were suggesting that the Department of Defense begin to perform systems analyses prior to entering contracts, and, as we have seen, began pilfering component studies from RAND to inform their own designs. "Indeed," Quade wrote in his memorandum, "there seems to be a feeling in

[117] Charles J. Hitch and Roland N. McKean, *The Economics of Defense in the Nuclear Age* (Cambridge, Mass.: Harvard University Press, 1960), pp. 130-131.

some parts of the Air Force that the systems approach may provide the complete answer to all questions of development, procurement, and operation as well as those of design."[118]

Quade, to an extent, welcomed this enthusiasm. The increasing use of "RAND in the role of an information collecting and transmitting agency" was "generally considered to be desirable." Furthermore,

> A properly executed systems analysis might not only furnish an 'optimum' choice (subject to the restriction that the requirements and assumptions on which it is based are likely to be extremely arbitrary) but also an effective means of argument to the Air Force (provided the Air Force understands systems analysis!) that the particular choice is superior to those made by competitors. Both the Air Force and the contractor should save money.

If everyone could agree to the arbitrary assumptions, or the Air Force could at least explicitly dictate their operational requirements, design would become more coherent and successful—provided the analyses were properly constructed. Components of RAND reports could not simply be plugged into new reports without understanding that the *operational* aspects of RAND reports, such as expectations of enemy capabilities, were based on highly variable and subjective information that required significant interpretation when incorporated into revised analyses. While industry could obtain people competent in the application of "techniques of operations research" (by 1953 everyone at RAND had begun to accept the new connotation of OR as component optimization), RAND opinion of the broad systems studies employing these techniques was low. Quade lamented that "there is little indication that industry has profited from RAND's mistakes." In addition, he felt that the Air Force had been "oversold" on

[118] E. S. Quade, "The Proposed RAND Course in Systems Analysis," D-1991, 12/15/1953, RAND Corporation library. He pointed to industry studies at major aircraft manufacturers. One article by a Lockheed representative suggested that the Department of Defense precede all planning decisions for design and procurement with a systems study: Robert A. Bailey, "Application of Operations-Research Techniques to Airborne Weapons Systems Planning," *Journal of the Operations Research Society of America* **1** (1953): 187-199.

systems analysis, and worried that "a surfeit of low quality studies may cause not only a complete loss of faith in the product but also in the salesman."[119]

However, a proposal had been floated that RAND offer a course in systems analysis that would "teach the airframe industry the 'correct' way to do such a study and the Air Force how to understand one". Quade felt the course was a good idea. Insofar as RAND could be said to know how to do a systematic study properly, the corporation had never published any sort of report on the methodology itself. "Unfortunately," he observed, "it is completely unorganized and much of it is buried in highly controversial opinions concerning the range of application, the validity of the results, and the adequacy of the method of systems analysis as a means of solving Air Force problems." The course would not only inform industries how to construct their own analyses, it would "disabuse the Air Force as to the technique and range of the method," pointing out areas to which it did not apply, such as in making basic "research decisions,"[120] but also instructing lower level Air Force planners how to "appreciate" analyses they received. At the same time, it would give RAND a chance to collect and collate the various ideas then in circulation about what systems analysis was and what it could accomplish. "Like a university faculty," Quade argued, "the faculty assigned to any systems analysis course could profitably spend the major part of its effort on research about the subject in general. A research effort in this field is called for regardless of any educational effort we might make."[121]

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[119] Quade, "Proposed RAND Course".
[120] Research refers to unpredictable basic research, as opposed to more directed development and design problems. This statement may have been inserted in response to RAND economists' brewing disagreements with the systems analysts; see Hounshell, "Medium is the Message," pp. 260-268.
[121] Quade, "The Proposed RAND Course in Systems Analysis".

The need to decide on the nature of systems analysis was acute. Charles Hitch and the Economics Department's work in budget allocation (as opposed to the problems of coordination of requirements with design) cast doubt on whether RAND's analyses were not leading the Air Force down a dangerous path. By offering the illusion that requirements and design configurations could be coordinated ahead of time, systems analysis might lead the Air Force and its contractors to believe that the pursuit of fruitless design paths could be avoided, even though uncertainty in the research and development process dictated otherwise. RAND economists Armen Alchian and Reuben Kessel took up this line of critique.[122] Responding to Quade's 1953 memorandum, they observed, "Inadequate compensation for development work is the reason developers feel inadequately compensated." There was little that systems analysis could do to remove the fact that research led to waste and the long lengths of time between development and production. The Air Force might simply have to offer more lucrative development contracts to offset the inevitable costs. Following the economists' concern for budgetary allocation, they argued that only the development of several projects could serve as an adequate hedge against the dangers of failed technical development.[123]

Alchian and Kessel knew they could be accused of sounding a false alarm. There was nothing in systems analysis dictating that only a single project should be pursued. Systems analysis referred only to processes of drafting sensible designs, not to the economists' goal of informing overall R&D funding policies. The economists recognized

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[122] Armen Alchian received his PhD in Economics from Stanford in 1944, and was an employee of RAND from December 1949 to February 1960. Reuben Kessel received his PhD in Economics from the University of Chicago in 1954, and was an employee of RAND from September 1952 to September 1956. See Hounshell, "Medium is the Message," pp. 260-268, and, for biographical information the appendix, pp. 293-295. The economists had become involved in broader questions about the "phasing" of research and development plans, and saw systems analysis as not addressing these concerns.

[123] Armen A. Alchian and Reuben A. Kessel, "A Proper Role for Systems Analysis," RAND Document D-2057, 1/27/1954, RAND Corporation library.

this fact, but they were still concerned that decision making bodies in the Air Force would not recognize the difference and use systems analyses to plan single-minded lines of research. High level Air Force policymakers had to that date been more likely to ignore RAND study conclusions than to take that route. Nevertheless, Alchian and Kessel wrote, "Little boys and matches neither logically nor inevitably lead to fires, but the probability is distressingly high, if it's your boy and house." Employing Hitch's erstwhile critical tool, they insisted that the uses of systems analyses were dictated by the criterion of evaluation used. They argued that systems analysis, as it stood, was only really useful if forecasts of future circumstances were not more or less arbitrary. If forecasts were poor, as they so often were, it would be better to develop studies of how to spend money hedging bets and improving forecasting methods than in working out the analytical conclusions of debased assumptions. Effectively, they wanted systems analysis to be about economics of budget allocation rather than about equipment design.[124]

Quade accepted the wisdom of the economists. In a more detailed proposal for the course, which he and the economist Malcolm Hoag[125] offered in 1954, they allowed that strikingly different options for weapons systems had to be considered, and offered examples for study in the course with that point in mind. Even though they would be "on safer ground if the examples were restricted to a selection from within a family of essentially similar instruments," which were easier to compare by mathematical means,

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[124] *Ibid.*

[125] Malcolm ("Mac") Hoag received his Ph.D. in Economics from the University of Chicago in 1950, and was an Assistant Professor of Economics at the University of Illinois from 1948 to 1950. He lectured in International Finance at the University of California at Los Angeles in 1951, and was a Program Officer for the Marshal Plan Mission to the UK from 1950 to 1952. He joined the Economics Department at RAND in 1952.

they felt the examples would be "less interesting and less relevant" as well. They pointed out that economic methods did not seem to offer any guidance on such matters aside from pointing out that diversity of investment was a virtue. However, it was not an unlimited virtue. "To hedge without limit by developing everything is, in view of the vast array of technically feasible choices, economically out of the question," they wrote. Potential development programs had to be compared, and these comparisons had to be made on some semi-rational basis, which led them to ask: "Should [these comparisons] not be made as carefully and as explicitly as possible, with no exclusion of alternatives arbitrarily or on the basis of petty considerations? Is that not Systems Analysis?" They reckoned it was: "To select by analysis is by our definition Systems Analysis, which is emphatically not to say that the techniques and results need be the same for procurement and development decisions nor that unique rather than multiple choices are inevitable." Quade and Hoag concluded their 34 page proposal by reemphasizing that the theme of their proposed course was to discuss "important questions" explicitly "albeit necessarily imperfectly," and to "treat them as best one can, and end with the analyst being honest with himself and his customers about the arbitrary and uncertain elements of importance in his analysis."[126] The problem was still unresolved, however: how should one undertake such an analysis?

The most immediate answer to the question was to abandon systems analysis' heritage in Edwin Paxson's experience with the design of fire control systems and to move more toward the plan-oriented problems of operations research, which, through the adoption of a mathematical canon, had itself come to bear some resemblance to design-

[126] E. S. Quade and M. W. Moag, "An Outline for the Proposed Course in the Appreciation of Systems Analysis," RAND document D-2132, 3/15/1954, RAND Corporation library.

oriented systems analysis. Systems analysis also began to embrace Charles Hitch's ideas about sub-optimization. Paxson's sprawling sets of interconnected equations were dispatched in favor of more localized quantitative analyses that were evaluated by criteria consistent with an overarching qualitative policy framework. The object of systems analysis shifted accordingly. As the quick reaction habits of the late war faded into the siege mentality of the Cold War, the idea of an optimal policy ceased to be one that best addressed a most likely scenario, and instead became one that would prove robust under any number of possible scenarios. Suddenly, deducing the analytical consequences of current knowledge was replaced by a spirit of what might be called disciplined creativity, which stressed imagining and testing the feasibility and cost-effectiveness of robust plans rather than trying to select a single best blueprint for future policy. As we have seen, the Air Defense Study had already been moving in that direction, but the intervention of the economists pushed systems analysis to redefine itself more thoroughly.[127]

This new image of systems analysis significantly altered the technical methodology used by systems analysts. Logistical methods, such as linear and dynamic programming, continued to prove crucial to testing the feasibility of potential policies. However, the uses of game theory at RAND morphed radically. Whereas in Paxson's systems analysis duel strategies and equipment selection could make direct use of solutions of game theoretical formulations, as the mathematical integration of component studies came to be replaced by more qualitative frameworks, these uses for game theory as a mortar connecting component bricks dropped off precipitously. Game theory did, however, maintain a presence at RAND as its decision theoretic uses began to become

[127] This approach also mollified the economists. See, for instance, Charles Hitch, "An Appreciation of Systems Analysis," *Journal of the Operations Research Society of America* **3** (1955): 466-481.

more important, not in providing precise strategies, but in providing insights into such questions as whether it was better to develop a few technologies that would prove adequate against a variety of threats, or a wide variety of technologies designed for specific scenarios. These uses of game theory, however, tended to remain confined to the theoretical side of RAND's work, serving more as a check on the instincts of those working in policy than as a tool for everyday use.[128]

Even as game theory faded into the theoretical background at RAND, "gaming" or simulation techniques became central to RAND policy work. We encountered an early version of a game in the last chapter in working out anti-submarine search tactics. Essentially, a game entailed working out the consequences of a stated policy to test its feasibility and robustness. Games could be played in the long tradition of war games; they could be played on boards like chess, except using a referee so that opponents' movements could not always be immediately discerned; or they could be played on computers wherein stated policies could be played out under varying circumstances, or varying policies could be played out under the same circumstances. The results of a

[128] Paul Erickson, "The Politics of Game Theory: Mathematics and Cold War Culture," Ph.D. dissertation, University of Wisconsin—Madison, 2006, esp. pp. 167-168, traces these changes at RAND, but notes (p. 102) that the history of game theory has tended to be warped by conflating its earlier and later uses. I would add that the strict models of the initial Strategic Bombing Systems Analysis has also been read forward onto later uses of systems analysis as rationalistic attempts to determine strategy. See, for instance, Jardini, chapter 3, and Fred Kaplan, *The Wizards of Armageddon*, pp. 63-68. Philip Mirowski has mocked game theorists for failing to capitalize on such visions, pointing out that game theory seemed to serve as more of a "salutary bracing regimen (perhaps like daily piano practice, or Grape Nuts in the morning), and a wonderful inspirational Muse," *Machine Dreams*, p. 329. In this case Mirowski is correct, except that strategic uses of game theory, unlike its early uses in Paxson's systems analysis, had never actually aspired to finding mechanistic uses for it. In 1949, Hitch, "Planning Defense Production," hinted at such "inspirational" uses, and in Hitch, "Appreciation," p. 480, he espoused this position explicitly, asking: "Suppose you have your defenses deployed as well as you can. Now you get more defenses. How do you deploy them?" He admitted, "Well, my intuition told me (and so did most people's) that you deploy them to protect additional targets… that you did not previously have enough to defend." A game theoretical formulation would suggest otherwise. "You use additional defenses mainly to increase the defense of targets already defended." Faced with this result, "you think about it and begin to see the rationale." His larger point of discussion was the virtues of combining intuitive kinds of policymaking with analysis, which, of course, has been a consistent theme of all the thinkers throughout this chapter and dissertation.

game, or even very many games could never point to a specific policies, but they might help explore hidden ways that policies could collapse, such as amid unsuspected logistical bottlenecks; or they might bring to light unsuspected strategies, such as the "continuous gambit" discussed for the search problem in the last chapter.[129]

Notoriously, both games and the new uses of game theory were also linked to the highest profile aspect of RAND work: strategic studies. Strategic studies became important to systems analysis in the wake of the first systems analysis performed in the new tradition, a study of alternative arrangements for basing the Air Force's strategic bombers completed in 1953, and directed by Albert Wohlstetter, a consultant to RAND and a mathematical logician by training, who had been urged to do the study by Charles Hitch. Wohlstetter worked on the project for some time without any help as he thought over the facets of the problem, and the study he eventually produced advocated strongly—and ultimately successfully—for basing the Air Force's nuclear strike force in the United States on account of its vulnerability to Soviet strikes if planes were stationed overseas. Although quantitative, the study boasted none of the in-depth mathematical analysis that had defined previous systems analyses, but it became the exemplar of the reconsidered analysis, because its recommendations were both unexpected and robust under a variety of potential circumstances. Much of its success stemmed from the fact that it showed that considering the basing problem from the strategic perspective of retaliation against a Soviet invasion of Europe proved shockingly fragile (and expensive) in light of the possibility that planes based overseas would be vulnerable to a surprise

[129] On gaming, see Sharon Ghamari-Tabrizi, "Simulating the Unthinkable: Gaming Future War in the 1950s and 1960s," *Social Studies of Science* **30** (2000): 163-223; and Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear Warfare* (Cambridge, Mass.: Harvard University Press, 2005), especially chapter 6 on "faith and insight in war-gaming". Mirowski, *Machine Dreams*, pp. 360-369 also discussed gaming within the context of his larger argument.

attack unless fortified heavily. If the bombers were based in America, however, they would better serve their foremost purpose, to deter both land and nuclear aggression, despite the fact that overseas basing was, on the surface, easier in the event the planes were actually ever deployed against their targets.[130]

Ultimately, strategic thinking became a complement to technical, logistical, and operational planning, giving rise to a journalist-friendly view of RAND, not as individuals who worked out the consequences of a vast array of technical knowledge and expert expectations, but as a think tank full of isolated brains who conjured up all kinds of military plans for the end of the world out of the sheer mathematical power of game theory. The publications of RAND political scientists such as Bernard Brodie, as well as systems analysts such as Albert Wohlstetter and, especially, Herman Kahn (who published his legendary book *On Thermonuclear War* in 1960[131]) only reinforced this view, as did their affiliation with non-RAND conflict theorists such as Thomas Schelling and the economist Oskar Morgenstern who both made careers out of their game theoretic studies of strategy.[132] From this point it would only be a couple of years before the filmmaker Stanley Kubrick introduced the world to Dr. Strangelove who commissioned studies from the "BLAND Corporation", counseled the nation's leaders about nuclear strategy, and soothed their dismay at their failure to prevent nuclear war with visions of

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130 A. J. Wohlstetter, F. S. Hoffman, R. J. Lutz, and H. S. Rowen, "Selection and Use of Strategic Air Bases," RAND Document R-266, April 1954, available online free of charge from the RAND store. A good overview of the study can be found in Bruce L. R. Smith, *The RAND Corporation*, chapter 6. See also E. S. Quade, "The Selection and Use of Strategic Air Bases: A Case Study," in *Analysis for Military Decisions*, edited by E. S. Quade (Chicago: Rand McNally, 1964). Also see Jardini, "Blue Yonder," pp. 123-125.

131 Herman Kahn, *On Thermonuclear War* (Princeton: Princeton University Press, 1960).

132 See Ghamari-Tabrizi, *Herman Kahn*. On strategic studies at RAND see Kaplan, *Wizards*, on which he is much more competent; and Andrew David May, "The RAND Corporation and the Dynamics of American Strategic Thought, 1946-1962," Ph.D. dissertation, Emory University, 1998. Neither deal extensively with the overall intellectual currents at RAND, and so their discussions should be taken as a rough guide to strategic thought, rather than as a thorough exploration of its rationales. On the uses of game theory in strategic thought, see Erickson, "Politics of Game Theory," chapter 3.

them tucked safely away underground with bevies of women who boasted "sexual characteristics of a highly stimulating nature," all awaiting the day they could reemerge and repopulate the decimated surface of the planet.

What has been less well-noted is systems analysts' commitment to developing ways to make the actual work of policymaking more robust.[133] As systems analysis itself diverged from a clear process of equipment design and more toward an anarchic medley of analytical techniques, including those associated with OR, the way systems analysis actually translated into policy became even less clear than it had been before. Every analysis had the potential to be misleading if it advocated a policy that would be negated if a crucial assumption were overturned, if some vital factor were neglected, or if certain values had been estimated incorrectly. As much as these studies promised to make policymaking more rational, they were also fraught with pitfalls. In 1957 Herman Kahn (with his colleague Irwin Mann) produced a chatty report called "Ten Common Pitfalls," which was supposed to serve as a section of a larger work on the technique and practice of OR and systems analysis. They introduced the paper by observing that, "Probably no applied professional group is so intensely and continuously concerned with methodological and philosophical questions as Operations Analysts and Systems Analysts." They partially attributed this preoccupation to "the normal introspection to be expected in any new field. However," they wrote, "it is hard to avoid the feeling that much of this self-questioning is caused by a sort of mass inferiority complex or at least a general sense of insecurity." They suspected this feeling was partially caused by the

[133] Ghamari-Tabrizi, *Herman Kahn*, is a crucial and informative step away from this trend.

"nebulous and unspecialized nature of most of the work," and the feeling that a lot of the work done was "not quite passable," due to the prevalence of "common mistakes".[134]

I believe the feeling to which Kahn and Mann referred can best be traced back to the uncertain relationship between the analyst and the policymaker. Policymakers bear responsibility for arbitrary decisions, but this fact is accepted because they are legitimately chosen to bear this responsibility. Analysts, meanwhile, can only reduce the burden of arbitrariness on a decision, not eliminate it. Furthermore, there is no easy way of ensuring that their analyses will actually reduce rather than add to the arbitrariness present in a decision by using an inappropriate model, meaning that there is no simple way to validate their participation in the policymaking process. Kahn and Mann took a therapeutic approach, warning analysts to avoid concentrating too much on a model ("Modelism"), doing studies too far removed from the first-hand knowledge of policymakers ("Hermitism"), mistaking probabilistic uncertainty from "real" uncertainty, making unrealistic assumptions about enemy capabilities, concentrating too much on one topic, choosing a topic that is too ambitious, and other mistakes likely to consign an analysis to irrelevancy with respect to the policymaking process.[135] This approach was echoed in the OR community as individuals such as Bernard Koopman (echoed by Charles Hitch) warned against "mechanitis" and "authorititis", which stemmed from overreliance on the power of computers and policymakers' formulations of problems, respectively.[136] Meanwhile, Edward Quade's aforementioned course in systems analysis was made entirely into an "appreciation" course for consumers of systems analyses, and

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[134] Herman Kahn and Irwin Mann, "Ten Common Pitfalls," RAND document RM-1937, 7/17/1957, p. vii. See also Ghamari-Tabrizi, *Herman Kahn*, pp. 12-14 on Kahn's style in live presentations of this document.
[135] Kahn and Mann, "Ten Common Pitfalls".
[136] Bernard O. Koopman, "Fallacies in Operations Research," *Operations Research* **4** (1956): 422-426; and Charles Hitch, "Comments by Charles Hitch," *Operations Research* **4** (1956): 426-430.

was offered in 1955 and 1959, a published summary appeared in 1964, the course was given again in 1965, and published again in 1968, incorporating new revisions. It attracted participation from RAND's top analysts, and stressed the possibilities and, especially, the limits of an analysis.[137]

Throughout the 1950s RAND attempted to negotiate a role for independent analysis in policymaking. Its reputation rested on its ability to make decisions more informed, but by concentrating on the disciplined yet creative act of creating alternative policy options, its analysts could not confine themselves to strictly rational conclusions; they had to include the arbitrary elements as well. In order for this strategy to work effectively, they had to discern what the Air Force already knew, even if Air Force personnel did not always state their knowledge explicitly, and they had to communicate their own judgments back to the Air Force, making equally clear what aspects of their analyses were well-founded, what aspects were tentative, and what aspects remained wholly arbitrary in the hope that the Air Force would be able to somehow incorporate this knowledge appropriately into their own policymaking. In addition to objectivity, this process required another virtue, honesty: to admit both to themselves and to their patrons what they did not know. A rationalized policy built on overly arbitrary foundations did not contribute to the rational act of policymaking—it detracted from it.

Conclusion

[137] Quade, ed., *Analysis for Military Decisions*; and E. S. Quade and W. I. Boucher, eds., *Systems Analysis and Policy Planning: Applications in Defense* (New York: American Elsevier, 1968). See also Hugh J. Miser and Edward S. Quade, *Handbook of Systems Analysis: Overview of Uses, Procedures, Applications, and Practice* (New York: North Holland, 1985); and Hugh J. Miser and Edward S. Quade, eds., *Handbook of Systems Analysis: Craft Issues and Procedural Choices* (New York: North Holland, 1988).

If the story of chapter four was that the introduction of widespread mathematical modeling helped sustain OR's wartime tradition rather than replace it, the story of this chapter has been how the tradition of wartime OR was preserved amid the threat to it that modeling brought. As much as mathematical sophistication made OR into something that could be uniquely identified outside the military, it also always threatened to split OR away from its original source of strength: its connection to the rationales underlying actual policies. It was only by continually translating between models and policies that OR managed to maintain its vitality, but, as we have seen in this chapter, this act of translation, by necessity, always happened at the local level. How much operations researchers would rely on professional integrity, and how much they would rely on their methodology in shaping their identity as operations researchers and with respect to the policymakers they served was a tension that tore at the profession long after its wartime origins had been recast in light of the self-sustaining profession that OR had become.

This tension can be most clearly seen in a short 1957 debate between two of the major players in this chapter: Russell Ackoff and Charles Hitch. In his speech as retiring president of ORSA, Ackoff implored operations researchers to take an interest in national planning, and, in particular, the economic planning of that frequent target of postcolonial developmental assistance, India. Consonant with his views about OR and scientific method, which we have examined here, Ackoff did not think that the canonical mathematical tools of OR would be of much assistance, but he did believe that operations researchers' "knowledge of system design, control processes, and the structure of decision-making," would make them particularly well-suited to aid in the establishment of effective governmental organizations. He made no mention of economists or other kinds of policy experts.[138]

Ackoff's speech drew a stern reply from Hitch, who, while sympathetic to Ackoff's goals and enthusiasm, argued that he knew "of no evidence… that operations research has (as yet) much to offer at the level of national planning." Instead, he argued once again that OR was the "art of sub-optimizing, i.e., of solving some lower-level problems," and that "difficulties increase and our special competence diminishes by an order of magnitude with every level of decision making we attempt to ascend." None of this was to say that "some operations *researchers*" could not "make excellent advisers on some high-level problems." He acknowledged, "The profession contains some men with a combination of first-rate minds, broad interests, wide and relevant experience, and excellent judgment." Nevertheless, he also pointed to the many ways these problems were far from straightforward, and suggested that operations researchers should concentrate their abilities on problems in which they had expertise, and avoid tarnishing their reputation by flailing at extremely difficult questions when other kinds of policy advisers were far better equipped to attack it. He pointed to a large number of reckless comments in Ackoff's talk as evidence of the dangers.[139]

Ackoff's response to Hitch was weak but revealing of his position. Effectively, he agreed with Hitch, but repeated that OR was not represented by its well-known techniques, but by its "method," which he refused to define, except insofar as it revealed a "logic" of "procedure". While he acknowledged that he did not have the necessary

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[138] Russell L. Ackoff, "Operations Research and National Planning," *Operations Research* **5** (1957): 457-468, quote on p. 464.
[139] Charles Hitch, "Operations Research and National Planning—A Dissent," *Operations Research* **5** (1957): 718-723, emphasis in original.

knowledge to approach India's development problems, he argued, "I would surround myself with such knowledge. But the concept of a *team* of *mixed disciplines* is an essential part of OR, is it not?"[140] With this statement, Ackoff rhetorically divided the world into those who approached problems in the most rational way possible, i.e. operations researchers, and the rest who did not. By this logic, the coordinated advice of any team of experts constituted operations research.[141]

If Ackoff's tilt at the windmill of national planning demonstrates that OR, at some level, *had* to be defined by its methods rather than by its commitment to better policymaking, we are still left with the question of how exactly OR managed to stabilize in the postwar years. There can be no simple answer. At the Arthur D. Little consulting firm veterans of the wartime tradition and new operations researchers such as John Magee were given a certain degree of freedom in navigating the divide between management and science by acting within the professional confines of consulting. Down the road at MIT, and at Case as well, students were encouraged to follow the wartime tradition and work within extant policy structures, but were also instructed in more advanced techniques. Finally, at the RAND Corporation, systems analysis, which had begun as a highly mathematical approach to equipment design transformed into a counterpart to operations research. Instead of exploring and scrutinizing an extant policy framework, as military OR did, systems analysts created and explored their own framework, and in so doing, gave policymakers with authority a template that they could use to inform their own deliberations. Often systems analysts used the more advanced

[140] Russell L. Ackoff, "On Hitch's Dissent on 'Operations Research and National Planning'," *Operations Research* **6** (1958): 121-124, emphasis in original.
[141] We will return to this strange identification of any kind of rational policymaking with operations research in the next chapter.

techniques of OR to analyze components, but just as often they did not. Thus their work blurred into more general acts of policy research. Yet, given that operations research began simply as a term connoting research into operations, this fate makes sense.

CHAPTER SIX

Pounds and Pence: Operational Research and the Politics of Science in Britain

In 1948 the venerable British science journal *Nature* opened that year's volume with an editorial on the "deployment of scientific effort in Britain," a subject discussed a month earlier in an address by the president of the Royal Society, Sir Robert Robinson. In the lean postwar years and under a Labour government with a large mandate, economic planning had become a staple of Britain's political discourse, and spokesmen for the scientific community, such as Robinson and the *Nature* editorial board, agreed that the coordination of Britain's limited scientific resources should be an integral part of this planning process. Intriguingly, the editorial observed that the need for coordination "brought to the fore a question…, namely, how far the methods of attack on urgent objectives adopted during the War, and generally known as 'operational research', can be applied in the present emergency, or even as a regular feature of our peace-time system." While the editorial waxed optimistic about the possibilities, it warned that it "may be doubted whether even the most brilliant application of the methods of operational research could do much here to elucidate the uncertainties which surround new industrial development arising from scientific research even in normal times…", and that if false hopes were raised, "the new method [will be] discredited before it has been fairly tried."[1]

The editorial's identification of wartime operational research as a peculiar "method of attack on urgent objectives" that could be applied to the planning of Britain's

[1] "Deployment of Scientific Effort in Britain" *Nature* **161** (1948): 1-3.

scientific effort was, given the wartime experience, bizarre, and it drew a rebuke from the embryologist, geneticist and political radical C. H. Waddington, who, we will recall, had been the head of the RAF Coastal Command OR Section for a time during the war. He noted, "In spite of the frequency with which operational research has been referred to recently, there are extremely few published discussions from which the layman can obtain an idea of what it is." Thus, "we find *Nature* implying that operational research is a method of planning." Waddington maintained it was necessary to distinguish operational research from planning processes, pointing out: "Research by itself can never produce a plan; its function is to prepare the basis on which a rational plan can be founded, by identifying qualitatively the factors in the situation, and, as far as possible, by evaluating them quantitatively." In his view, OR was any research into the issues underlying executive decisions, whether it was called operational research or not, and he found it "astonishing" that anyone could contemplate planning the redistribution of scientific manpower without it.[2]

The exchange between Waddington and the editors of *Nature* showed clear signs of misunderstanding resulting from an intellectual incoherency that inhabited the discourse surrounding science's relationship to British society and the British state at that time. It was obvious that wartime developments in science management provided significant lessons for the organization of postwar science, but these lessons were expressed in a way that twisted many different issues into a single tangled conversation. This conversation exhibited what we might term the "oppositional view", which held that science and policymaking were isolated realms, and that the former had to be brought

[2] C. H. Waddington, "Operational Research" *Nature* **161** (1948): 404.

into the considerations of the latter, despite the latter's suspiciousness, if scientific effort was to be coordinated effectively with the nation's needs.

Contrary to this discourse that had developed, we can distinguish no fewer than six *separate* issues that exercised the minds of Britain's scientific and administrative elites during the war, most of which *did not* pit the unified interests of a scientific community against the unified interests of military and government administrators. First, there was the question of how the efforts of the various military-run research establishments were coordinated into a coherent research and development (R&D) strategy. Second, there was the question of how a coherent R&D strategy was formulated against the background of the military's changing requirements. Third, there was the question of how academic scientists were incorporated into research organizations run by the civil service. Fourth, there was the question of how scientists became advisers to high-level military and government figures. Fifth, there was the question of how military operations came to be a subject of study for scientists. Finally, sixth, there is the utterly confusing question of how all these other questions were incorporated within a single "science policy" rubric.[3]

These issues were, of course, interrelated, but there was no way to coherently discuss them all at once. There was, however, one man who had a major hand in dealing with all of them: Sir Henry Tizard. Since the end of the First World War, Tizard had been a high-level government science administrator with strong connections with industrial and military R&D. In this capacity, he felt he had to understand government

[3] On the importance of keeping different scientific roles separate, see David Edgerton, *Warfare State: Britain 1920-1970* (New York: Cambridge University Press, 2006), pp. 163-166. While Edgerton is correct in claiming that scientific advisers had little to do with the direction of scientific research, they did advise the military personnel who were the regular liaisons with the military's research establishments, and their role, though an indirect one, should not be dismissed.

and industry's technology requirements, as well as laboratories' capabilities for providing for them, in order to make certain that research was being prioritized correctly. He was also a bureaucratic reformer, dedicated to creating science policymaking bodies that could route knowledge between different laboratories and positions of authority most efficiently. In both activities he acted with a certain virtue, which held that science administrators should marshal information drawn from executive and scientific hierarchies, and they should *not* make proclamations on the basis of their own status and expertise as scientists. From an administrative standpoint such "Tizardian" virtue, as we will call it, was simply good practice, but for academic scientists who handled all of the knowledge flowing through their offices and laboratories personally, it must have seemed a fairly ingenious way of handling science policy, and they closely identified it with Tizard himself.[4]

Tizardian virtue fit fairly cleanly into the oppositional view. The creation of the policymaking bodies he advocated brought more scientists into administrative positions of their own, and thus into closer contact with government officials, and Tizard himself came into contact with some of the highest officials. Accordingly, the standard narrative of the politics of science in the war has been told not as the resolution of a series of separate policy issues, but (usually with a few detours in the story) as a pilgrim's progress: Tizard, enlightened by his virtue, making his difficult way into the peaks of British government. In 1934 Tizard became chair of the Committee for the Scientific Survey of Air Defence (CSSAD), which led to radar, which saved Britain. In 1935 and 1936 he sparred with Winston Churchill's confidant and personal science advisor, the Oxford

[4] The best biographical source on Tizard remains Ronald W. Clark, *Tizard* (Cambridge, Mass.: MIT Press, 1965).

physicist Frederick Lindemann, who was not possessed of Tizard's virtue—and won. In 1939 he became Scientific Adviser to the Chief of Air Staff, but in June 1940, after Churchill came to power, Tizard resigned because Lindemann had become more influential. So, he went to America on an enormously successful technology exchange mission, and when he returned that winter he was made an independent adviser to the new Ministry of Aircraft Production (MAP), and was also soon appointed to the Air Council, which set policy for the whole Air Ministry—but this position was ultimately disappointing because the Air Council, strangely, did not deal much with science issues. Meanwhile, Tizard's academic allies from the CSSAD, the physiologist A. V. Hill and the physicist Patrick Blackett, attempted to make their own inroads into government. Hill, a secretary of the Royal Society, lobbied aggressively for a high level body to advise the Cabinet on science issues, but the Scientific Advisory Committee that resulted was a disappointment. Blackett, though, created operational research, which swiftly spread throughout the American and British militaries. But in 1942, Tizard and Lindemann, who was now called Lord Cherwell, sparred again, this time over the strategy of area bombing German cities. Tizard, who felt area bombing could not accomplish its objectives, lost out, and soon thereafter retired to the presidency of Magdalen College, Oxford.[5]

The victories and disappointments that fill this narrative are actually quite chimerical, though. The narrative implies that high level administrators were, as a group, reluctant or unwilling to develop effective science coordination tools: they had to listen to Cherwell because of Churchill, the War Cabinet refused to create an adequately strong

[5] See the historiographical discussion at the end of this introduction for information on sources of the standard narrative and challenges to it.

coordinating body, and the Air Council did not even see fit discuss science. Every setback for Tizard was a setback for science. The history of OR, meanwhile, is presented as a great victory for scientists who were asking to sit in on military planning sessions, and a sign of the enlightenment of a few military patrons who welcomed them. In fact, though, each of these incidents on Tizard's odyssey unfolded according to its own dynamic, often with great dissent among scientists (even ones not named Lindemann/Cherwell), and usually with broad sympathy and support of government policymakers. Tizard, we will find, handled these issues deftly, not explicitly attempting to bring science into government, but making certain that the appropriate scientists and the appropriate authorities knew enough about the others' activities to formulate good policies.

In Part I of this chapter, we will return to the war to untangle the various incidents in the standard narrative and gain a better understanding of their internal dynamics. We will find that operational research, or at least the way it fit into the politics of science, *was* a distinctly Tizardian innovation. Before the rise of OR in 1941, Tizard's political moves had focused largely on the establishment of advisory committees and individual science advisory positions to coordinate R&D activities. After 1941, Tizard found that OR could provide science advisers (and military authorities) with a clear image of evolving military operations they needed to offer quality advice (or, for military authorities, to make good decisions), but it did not itself represent an extension of scientific authority. To subscribers of the oppositional view, though, science advisers and OR were simply two components of the advancement of the scientific agenda, but this view proved awkward when confronted with the question of what OR actually was, as the editors of *Nature* discovered when Waddington scolded them.[6] Part II will expand on our brief discussion of *Nature*'s editorial and Waddington's retort to examine the ways OR was—and was not—employed rhetorically after the war, and the ways OR presented a problem for those who wanted to use it as an icon of the oppositional view to further their own rhetorical ends.

We have left this discussion until the end, because the rhetorical use of OR in the oppositional view did not significantly impact its postwar development.[7] But it has had an outsized impact on the historiography not only of OR but of the politics of British science in general. The oppositional view had become dominant even while the war lasted, but the historiography of science and the war did not coalesce around it in earnest until C. P. Snow delivered two sets of lectures: "The Two Cultures" in 1959, and "Science and Government" in 1960. The first criticized the gap between scientists and classically-trained administrators, and solidified the oppositional view as the *de rigueur*

[6] Some of the rhetoric surrounding OR was supremely awkward for precisely this reason. *Nature* pointed out, "It has been suggested [...] that Cabinet planning bodies such as that provided over by Sir Guy Plowden and the Advisory Council of Scientific Policy [chaired by Tizard, back from the political dead], may be regarded as operational research sections established at a strategic level in the economic field." Waddington observed, "It is, perhaps, because operational research scientific workers had to depend so largely for detailed information on the generous collaboration of their specialist colleagues that some people appear to think that they invented new techniques of teamwork." As Waddington at least understood, both visions of OR were nonsensical. See "Deployment of Scientific Effort in Britain" and Waddington, "Operational Research".

[7] This statement directly opposes the views of Jonathan Rosenhead, "Operational Research at the Crossroads: Cecil Gordon and the Development of Post-War OR" *Journal of the Operational Research Society* **40** (1989): 3-28; and Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (New York: Cambridge University Press, 2002), pp. 182-183, who casts his "British" OR as doomed by its association with leftist planning. Oddly, Mirowski does not cite Rosenhead in his vast bibliography. Literature following Rosenhead makes many valid points, but suffers from failing to distinguish between administrative groups, advisory groups and investigatory groups and can be confusing for this reason. See M. Fortun and S. S. Schweber, "Scientists and the Legacy of World War II: The Case of Operations Research (OR)" *Social Studies of Science* **23** (1993): 595-642, esp. pp. 613-615; Maurice Kirby, *Operational Research in War and Peace: The British Experience from the 1930s to 1970* (London: Imperial College Press, 2003), chapter 6. The concept of a leftist OR standing at a "crossroads" after the war, to be taken over by conservative forces, is quite misleading. The "OR" that Rosenhead refers to is a stand-in for various leftist planning schemes that never took off. We will see, though, in Part II of this chapter, that usually the left (with whom Waddington was identified) used OR more as a rhetorical reference point than as an actual thing to be placed by outsiders into government agencies.

approach to the subject of science and politics in Britain.[8] The second detailed the conflicts between Tizard and Cherwell, and was important in establishing the narrative of how Tizard's virtue saw him through hostile administrative cultures.[9] While Snow's works were clearly polemical, their basic sensibility was mimicked a few years later in more measured historiographical language by Ronald Clark in his works *The Rise of the Boffins* (a "boffin" being a government or industrial scientist, a "back room boy" trotted out to solve problems) and *Tizard.*[10] Their titles should speak for themselves. Whereas Clark's narratives followed a relatively sensible historical trail where scientists in the civil service gained administrative control over the coordination of military research, more recent versions of the oppositional view have more bizarrely stressed the intellectual foment of the "social relations of science movement", a group of outspoken academic scientists, composed partially of radical leftists, who were active in the 1930s and a few of whom had some say in government circles during the war. William McGucken's 1984 book, *Scientists, Society and State* even implicitly purports to

[8] C. P. Snow, *The Two Cultures and the Scientific Revolution* (New York: Cambridge University Press, 1959). Snow's view was roundly criticized from the start, which did not stop its absorption into the historiography. On the essential context of Snow's address see Guy Ortolano, "The 'Two Cultures' Controversy: C. P. Snow, F. R. Leavis, and Cultural Politics in Post-war Britain" Ph.D. Dissertation, Northwestern University, 2005; and Guy Ortolano, "Human Science or a Human Face? Social History and the 'Two Cultures' Controversy" *Journal of British Studies* **43** (2004): 482-505. On the historiographical implications, see Edgerton, *Warfare State*, pp. 196-210. There is a substantial literature on Cherwell that serves as a corrective. See Earl of Birkenhead, *The Prof in Two Worlds: The Official Life of Prof. F. A. Lindemann, Viscount Cherwell* (London: Collins, 1961); Thomas Wilson, *Churchill and the Prof* (London: Cassell, 1995); and Adrian Fort, *Prof: The Life of Frederick Lindemann* (London: Jonathan Cape, 2003). Although not detailed on the wartime experience, see also R.F. Harrod, *The Prof: A Personal Memoir of Lord Cherwell* (London: Macmillan & Co, 1959). For a renewed but better informed criticism of Lindemann in the CSSAD dispute, see David Zimmerman, *Britain's Shield: Radar and the Defeat of the Luftwaffe* (Stroud: Sutton Publishing Limited, 2001), chapters 5, 7 and 9.

[9] C. P. Snow, *Science and Government* (Cambridge, Mass.: Harvard University Press, 1961); based on the 1960 Godkin Lectures, which he delivered at Harvard University. Patrick Blackett set the pace for Tizard hagiography. See P.M.S. Blackett, "Tizard and the Science of War," *Nature* **185** (1960): 647-653, reprinted in *idem.*, *Studies of War, Nuclear and Conventional* (New York: Hill and Wang, 1962), pp. 101-119. See also Blackett's review of *Science and Government*, reprinted in *Studies of War*, pp. 120-127.

[10] Ronald W. Clark, *The Rise of the Boffins* (London: Phoenix House, 1962); and Clark, *Tizard.* Tizard and Cherwell were also compared in the last two chapters of J. G. Crowther, *Statesmen of Science* (London: The Cresset Press, 1965), pp. 301-376.

supercede Clark's narrative by considering the events of the war to be an advanced phase of the movement, and Erik Rau's work, in turn, considers OR a compromised result of movement scientists' ambitions to influence government policy.[11]

In the last decade or so, David Edgerton has dismissed the standard narrative altogether as a construct of "anti-historians" (who were largely card-carrying members of the social relations of science movement) that systematically exaggerates the role of outside scientists and downplays the importance of the military and its civilian research corps when discussing the relationship between the state and science.[12] Both he and the historian Paul Crook have pointed to the rhetorical use of OR and Tizard's travails as an integral part of a small group of politically-active elite scientists' efforts to drive a conceptual wedge between their supposedly pacific scientific interests and the military's social role as purveyors of senseless violence, in spite of their collaboration in the necessary war against fascism.[13] Yet, Edgerton's important contribution to the historiography passes too quickly over the significant role OR did play in supporting the important and enduring mechanisms for offering scientific advice in the British military

[11] William McGucken, *Scientists, Society and State: The Social Relations of Science Movement in Great Britain, 1931-1947* (Columbus: Ohio University Press, 1984); Erik Rau, "Combat Scientists: The Emergence of Operations Research in the United States During World War II," Ph.D. Dissertation, University of Pennsylvania, 1999, chapter 1; and Erik P. Rau, "Technological Systems, Expertise, and Policy Making: The British Origins of Operational Research," in *Technologies of Power: Essays in Honor of Thomas Parke Hughes and Agatha Chipley Hughes*, edited by Michael Thad Allen and Gabrielle Hecht (Cambridge, Mass.: The MIT Press, 2001), pp. 215-252. Also see Hilary Rose and Steven Rose, *Science and Society* (Harmondsworth: Penguin, 1969), esp. chapters 3 and 4; and Gary Werskey, *The Visible College: A Collective Biography of British Scientists and Socialists of the 1930s* (London: Free Association Books, 1988 [1978]), esp. chapter 8; and Solly Zuckerman's war memoirs, Solly Zuckerman, *From Apes to Warlords* (New York: Harper & Row, 1978), pp. 108-113 and Appendix I on his Tots & Quots dinner club, and esp. p. 404 on its purported relationship to OR.
[12] Edgerton, *Warfare State*, esp. Ch. 5.
[13] See D. E. H. Edgerton, "British Scientific Intellectuals and the Relations of Science, Technology and War," in *National Military Establishments and the Advancement of Science and Technology*, edited by Paul Forman and José M. Sánchez-Ron, 1-35 (Boston: Kluwer Academic, 1996); and Paul Crook, "Science and War: Radical Scientists and the Tizard-Cherwell Area Bombing Debate in Britain," *War & Society* **12** (1994): 69-101. I will challenge this view in Part II of this chapter.

that we briefly examined in chapters two and three. The history presented here traces the origins of these mechanisms amid Tizard's and others' efforts to reform the administration of science as well as the origins of the rhetorical conventions that evolved around these efforts and muddled the postwar discourse about politics and science—and OR—so badly.

PART I

Henry Tizard and the Administration of Research and Development, 1920-1936

In order to understand the political conversations with which this chapter will deal, it is necessary to have at least a passing familiarity with the network of R&D establishments that existed in Britain following the First World War, and the means by which their work was coordinated. By far the largest employers of scientific personnel anywhere in Britain were the military's research establishments. Among the growing number of laboratories supported by the military, the Admiralty's Royal Observatory at Greenwich and the War Office's Royal Arsenal at Woolwich were centuries-old institutions, while the Royal Aircraft Establishment at Farnborough was growing quickly along with the importance of the airplane.[14] The National Physical Laboratory (NPL), established in 1900 and administered by the Royal Society up through the First World War, set Britain's standards of measurement, but also conducted important aeronautical research. At the end of the war, the NPL was subsumed within the government's new Department for Scientific and Industrial Research (DSIR), which swiftly earned pride of

[14] A complete list of Military R&D establishments as of 1932 can be found in Edgerton, *Warfare State*, p. 120.

place in British science policy circles as the largest government institution dedicated to civil research. It ran a number of other applied sciences laboratories in areas such as road and building construction, and it oversaw a series of research associations, which were intended to serve as central R&D laboratories and knowledge clearinghouses for whole industries, so that small companies would not bear too great a research burden. In 1921, meanwhile, the Admiralty named NPL physicist Frank E. Smith as its first Director of Scientific Research (DSR).[15] The DSR position itself became a central point in each military service's scientific research infrastructure. There were also a number of permanent and *ad hoc* committees devoted to advising military and government personnel in charge of R&D, and the various civil and military research establishments themselves. Among these the Medical and Agricultural Research Councils, the Aeronautical Research Committee (which reported to the Prime Minister) and the Ordnance Committee were especially powerful.[16]

In 1920, Henry Tizard, who during the war had directed the Royal Flying Corps' experimental flying establishment, and then served in the Ministry of Munitions, was hired by the DSIR as an Assistant Secretary to be in charge of four boards responsible for coordinating research on certain topics across the services establishments, and to keep them in touch with the civil developments with which the DSIR primarily dealt. The boards had no earth-shattering impact, and all but one dealing with radio technology were dissolved in 1927 in favor of *ad hoc* committees, but Tizard still quickly established his

[15] See Charles F. Goodeve, "Frank E. Smith," *Biographical Memoirs of Fellows of the Royal Society* **18** (1972): 525-548.
[16] The most complete discussion of Britain's research infrastructure between the wars is David Edgerton, *Warfare State*, chapter 3. For a detailed look at World War II developments, see M. M. Postan, D. Hay and J. D. Scott, *Design and Development of Weapons: Studies in Government and Industrial Organisation*, (London: HMSO, 1964); and Edgerton, *Warfare State*, chapter 4.

reputation as a capable administrator of scientific research.[17] He became the Secretary of the DSIR (its head) from 1927 until 1929, when he took a step toward the academic world by becoming the Rector of Imperial College. He maintained his influence with government and industrial aeronautical laboratories, however, becoming the chair of the Aeronautical Research Committee from 1933 until 1943.[18]

It was during this earlier period of his career that Tizard developed his much-lauded understanding of research and development, which was apparent in the unorthodox views he offered regarding the importance of pure scientific research in a speech to the Royal Colonial Institute in 1928:

> The scientific man ... tends to over-emphasise the relative value of pure science, that is, of research undertaken only for advancement of knowledge without any regard to utilitarian objects. All practical advances in industry are said to depend ultimately on advances in pure scientific knowledge. It might equally well be argued that all advances in purely scientific knowledge depend ultimately on industrial progress. The two are so intimately connected that it always appears to me to be a waste of time to discuss the relative importance of pure and applied science.[19]

Complementing this clear rejection of the notion that technology descended from scientific principles, he was acutely aware that most research and development was far from an individualistic and heroic enterprise. In fact, he felt that the most useful fruits of industrial research were not even the result of new inventions, but improvements on existing processes. Therefore, although it was, of course, wise to keep up to date with recent scientific developments, the most important thing was to keep channels of

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[17] In 1924 he recommended that the Air Ministry establish its own DSR post. They did and offered it to him. He refused it, and it was given instead to Harry Wimperis, the head of the Air Ministry's research laboratory at Imperial College and a wartime acquaintance of Tizard's. Wimperis would hold the post until his retirement in 1937 when it was given to his deputy David Pye, with whom Tizard had worked even more closely just after the war; see Clark, *Tizard* pp. 53-55.

[18] On this period of Tizard's life see Clark, *Tizard*, chapters 3 and 4. On the Aeronautical Research Committee, see David Edgerton, *England and the Aeroplane: An Essay on a Militant and Technological Nation* (Basingstoke: Macmillan, 1991), pp. 4-5. See also Edgerton, *Warfare State*, p. 126.

[19] H. T. Tizard, "Scientific and Industrial Research", *United Empire: The Royal Colonial Institute Journal* **19** (1928): 186-194; delivered on 2/14/1928 to the Royal Colonial Institute. In addition to the quotes here see the full text of Tizard's 1920 reply to the Secretary of the DSIR Frank Heath's recruitment overture, Clark, *Tizard*, pp. 57-60.

communication open between laboratories that could be developing devices or techniques of value to others. He remarked in a 1938 lecture in India,

> There is an old English proverb which runs thus: 'Take care of the pence and the pounds will take care of themselves'. It has an application to the organisation of industrial research which I may put in this way. Take care of the many people who are capable of applying scientific methods and knowledge to the improvement of known processes and things, and you will then be in the best position to turn to practical use the discoveries of men of outstanding originality. Such men are rare and we do not know how to produce them.[20]

Looking back to the First World War, he noted that, compared to Germany, the British had no overarching organizational scheme, but (speaking as the Rector of Imperial College) he credited the English educational institutions that were designed to "educate men rather than produced specialists." Such men could easily adapt their generalized knowledge to unforeseen situations.[21] Coordination, though, remained Tizard's foremost concern.

Although Tizard's 1934 appointment as the chair of the Air Ministry's new Committee for the Scientific Survey of Air Defence (CSSAD) has often been portrayed in the oppositional view as a breakthrough for the scientific agenda, it is probably best viewed as a particularly consequential instance of extant government machineries creating an *ad hoc* committee to organize an attack on a particular problem.[22] In addition to Tizard, it was composed of the Air Ministry's DSR Harry Wimperis (who had pushed for the committee's establishment), Tizard, and two academics. Nobel Prize-winning University College London physiologist A. V. Hill, an unparalleled expert on the theory and practice of anti-aircraft gunnery in World War I, had been out of contact with

[20] Parin Memorial Lecture, given 1/1/1938 in Jamshedpur, India; *HTT* 566.
[21] *Ibid.*
[22] Among the numerous histories of the CSSAD, see Clark, *Tizard*, chapters 6-8; Zimmerman, *Britain's Shield.* Edgerton's critique of the place of the CSSAD in the historiography is implicit in the incredibly short amount of attention he gives it as part of a list of other *ad hoc* committees, Edgerton, *Warfare State*, p. 127.

military science since then.[23] Birkbeck College physicist Patrick Blackett had recently joined Tizard's Aeronautical Research Committee, but he was primarily identifiable as a top product of Ernest Rutherford's Cavendish Laboratory.[24] The committee immediately devoted itself to learning about, coordinating and prioritizing new work on air defense technologies in the government's research establishments. Its most fundamental contribution came even before its first meeting when Wimperis contacted Robert Watson Watt,[25] the head of the NPL's Radio Research Laboratory, leading to the suggestion that aircraft might be tracked using radio waves. The committee quickly began to arrange a series of tests to determine whether the new technology (which they called RDF and later became known as radar) could be developed to meet Britain's air defense requirements.

The Oxford physicist Frederick Lindemann, who began attending the committee's meetings in July 1935, had his own perspective on its work. He and his political patron, the out-of-power Churchill, had originally pressed for a committee to be established under the Cabinet's Committee for Imperial Defence, *outside* of the Air Ministry's control.[26] In accordance with the outspoken Churchill, he felt it was necessary to assume a militant posture against Germany where the Nazis had recently taken power. So, when a piqued Lindemann learned of and joined Tizard's committee, he envisioned it as performing a gadfly function, coaxing a supposedly lethargic Air Ministry and its

[23] He won the 1922 Nobel Prize for Medicine or Physiology for his academic work that laid the foundations for biophysics. See Bernard Katz, "Archibald Vivian Hill," *Biographical Memoirs of Fellows of the Royal Society* **24** (1978): 71-149.

[24] Blackett had already performed much of his work on particle detection with cloud chambers that would earn him the 1948 Nobel Prize in Physics. He had narrowly missed being credited as discoverer of the positron. In 1937 Blackett would move to Manchester University. On Blackett, see Bernard Lovell, "Patrick Maynard Stuart Blackett, Baron Blackett, of Chelsea," *Biographical Memoirs of Fellows of the Royal Society* **21** (1975): 1-115; Peter Hore, ed., *Patrick Blackett: Sailor, Scientist, Socialist* (Portland, OR: Frank Cass, 2000); and Mary Jo Nye, *Blackett: Physics, War and Politics in the Twentieth Century* (Cambridge, Mass.: Harvard University Press, 2004).

[25] Watson Watt began hyphenating his name after he was knighted in 1942.

[26] The Committee for Imperial Defence was a Cabinet-level organ aiming to coordinate Britain's war-making ability. It was succeeded by the War Cabinet.

supposedly innovation-averse research scientists to action. Tizard bristled at this attitude and warned Lindemann that it would be detrimental to securing the indispensable cooperation of Air Ministry scientists.[27] Sure enough, the committee's meetings quickly turned bitter as Lindemann insisted on incorporating his own project ideas, especially infrared detection and aerial mines, onto the committee's and research establishments' agendas against the recommendations of other committee members and the establishment researchers. Unwilling to take their misgivings at face value, he believed that at least some of his schemes could be made practical, in spite of initial skepticism, through heroic acts of research supported by war-like increases in spending,[28] and he was continually frustrated by the committee's refusal to push the Air Ministry to pursue a variety of bold research projects in addition to RDF.[29]

The political rhetoric of science had become, and would continue to be, expressed in terms of engagement versus isolation. Good science administration was supposedly done by those such as Tizard who were the most engaged with the various branches of the scientific community while those who were isolated tended toward the extremes of

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[27] Among the multitudes of references, see Snow, *Science and Government*; Clark, *Tizard*, chapters 6-8; the Cherwell literature cited in note 8; R. V. Jones, "Lord Cherwell's Judgement in World War II," *Oxford Magazine* (9 May 1963), pp. 279-286; Zimmerman, *Britain's Shield*, chapters 5, 7, and 9; and Crook, "Science and War". On Tizard's meeting and subsequent correspondence with Cherwell, see Clark, *Tizard*, pp. 126-127.

[28] In understanding Lindemann's attitudes, his minority report issued prior to the temporary dissolution of the group is valuable. R. V. Jones alludes to it in "Judgement", but a full copy is available in *PMSB*, PB/9/1/64 (formerly J.44). Lindemann's view reflects a very common story told *post hoc* of important technological advances, especially the atomic bomb and the CSSAD's own radar. Traditionally, it is the large-scale, against-the-odds boldness of these stories of technological development as driven by charismatic individuals (such as Robert Oppenheimer and, ironically, Tizard) rather than their industrial practicality, that has, understandably, been emphasized, and if one can throw in a story about overcoming the skeptics so much the better.

[29] Lindemann was also frustrated by the committee's lack of willingness to aggressively press its own suggestions, particularly that Watson Watt be put in charge of both technical development and implementation of RDF. Lindemann saw it as part of a broader pattern of the committee's meekness toward its sponsors. To the committee's chagrin, Lindemann introduced Watson Watt to Churchill on his own to air the outspoken Scottish engineer's frustrations. See Cherwell's minority report, and, again, references in the Cherwell literature.

fantasy or stagnation. Lindemann accused the research establishments of entrenchment against the outside world—an accusation that would become a constant refrain for Hill, who later came to see himself as a gadfly as well. In 1936, though, it was Lindemann who, out of touch with the sensible minds of the Air Ministry, was viewed by most scientists on the committee and in the establishments as enamored with his own technological fantasies. His continued pushing of his own pet projects, combined with his intolerably scornful and sarcastic personality and penchant for going over the heads of the committee turned the others irrevocably against him, and by the fall of 1936 Blackett and Hill resigned, the CSSAD was dissolved, and then resurrected in short order without Lindemann, but with Cambridge radio expert E. V. Appleton, who connected much more easily with insider science, taking over as Secretary of the DSIR in 1939 from Frank Smith, the first Admiralty DSR who had left that position to succeeded Tizard at the DSIR in 1929.

Insiders and Outsiders: New Schemes and Damp Squibs, 1938-1942

By 1938 Henry Tizard had augmented his already substantial reputation through his coordination of the RDF development effort, which began to stake out its own corner within the Air Ministry's structure of research establishments with early tests arranged at the War Office's Air Defence Experimental Establishment at Biggin Hill and the Air Ministry's new RDF laboratory at Bawdsey Manor. A. P. Rowe, the secretary of the CSSAD, rose to prominence alongside Watson Watt, becoming the Chief Superintendent of the new lab, which became known as the Telecommunications Research Establishment

(TRE), and eventually settled at Malvern.[30] Tizard, Watson Watt, and Rowe also involved themselves with the actual adaptation of RDF into an effective aircraft detection and fighter interception system. Rowe called much of the requisite field testing and interception coordination work operational research, to distinguish it from laboratory work. This work led to the foundation of the Stanmore Research Section at the Fighter Command Headquarters in northwest London under the industrial scientist Harry Larnder. This group would eventually be called the Operational Research Section, Fighter Command.[31] Politically, though, OR was still insignificant, and it will make sense to remain with Tizard a while longer.

Pleased with the progress the CSSAD had made with RDF, in June 1938 Tizard wrote to Kingsley Wood, the Secretary of State for Air,[32] suggesting that the committee had already accomplished its most important work. Because "really new ideas which are practical are rare, and as time goes on the detail of the work becomes much more important," he wrote in typical fashion, it was important to keep personal contact between "outside scientific men and Government staff who are actually engaged in the experimental work. Such contact helps to concentrate the work on the most useful lines, and thereby avoid a waste of time and effort." So he pressed for an organization through which outside scientists could familiarize themselves with the problems of the establishments ahead of time, and which might even incorporate the other two services

[30] The most recent quality survey of the early development of radar in Britain is Zimmerman, *Britain's Shield*.
[31] Among the many histories of early OR, see especially Kirby, *Operational Research*, chapter 3.
[32] The Secretary of State for Air was the Cabinet-level political head of the Air Ministry, which controlled the Royal Air Force.

via a "small, almost informal" committee that would tighten extant informal bonds between service DSRs.[33]

Wood approved of Tizard's idea and passed the suggestion for an inter-Service committee along to the Minister for the Co-ordination of Defence Sir Thomas Inskip's office where it was championed by the civil servant H. G. Vincent.[34] The scheme gained wide approval, but Duff Cooper, the First Lord of the Admiralty,[35] rebuffed it, certain that the Admiralty had little to gain from such a committee.[36] Vincent ended up passing the science coordination issue along to the Cabinet Secretary Sir Edward Bridges, the highest civil servant in the British government.[37] In January Bridges spoke with John Buckingham, the Deputy DSR at the Admiralty. Buckingham was defensive against any suggestion that the Admiralty was not adequately engaged with outside research trends, and he seems to have identified Tizard as an interfering academic outsider—an identification that Tizard was eager to avoid. Buckingham, according to Bridges,

> claimed that the Admiralty scientists were fully in touch with the Universities and knew quite well what work was being done in the Universities. He completely denied the suggestion that the scientific branches of the Service required to be 'fertilised' by fresh scientific ideas brought in from outside.

When Tizard met with Bridges later that same day for lunch and was informed of Buckingham's comments, he dropped his usual diplomatic attitude toward establishment scientists, doubting that the Admiralty was as in touch as they thought, "and that they

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[33]"Scientific Advice on Problems of War" 6/16/1938 and Wood to Tizard, 6/30/1938, TNA: PRO CAB 21/711; The "almost informal" description was given by Tizard in reaction to later criticism. See Tizard to Vincent, 11/15/1938, TNA: PRO CAB 21/711

[34] The Minister for the Co-ordination of Defence headed the Committee for Imperial Defence.

[35] The First Lord of the Admiralty is its Cabinet-level political head, the British equivalent of the Secretary of the Navy. This position should not be confused with First *Sea* Lord, who is the military head of the Admiralty, and is the equivalent of the American Chief of Naval Operations (CNO).

[36] Cooper to Inskip, 9/21/1938, TNA: PRO CAB 21/711.

[37] See Vincent memo to Secretary, 11/18/1938, "Scientists and Defence" TNA: PRO CAB 21/711. The Cabinet Secretary is the foremost civil servant in British government, and even has a degree of authority over Cabinet Ministers. His continuing involvement in the issue of science coordination should indicate its importance.

stood in no need of 'fertilisation'. He also took the view that the line set out in [Cooper's letter] was not the line which would normally be adopted by really first-class scientists, who would be perfectly willing to try and learn from anybody." Tizard felt the matter should be dropped.[38]

So it was, until it was picked up some six months later by CSSAD member A. V. Hill and A. C. (Jack) Egerton, under that aegis of the academic elite, the Royal Society. Hill had been the secretary of the Society for biology since 1935, while Egerton had replaced Frank Smith, the outgoing secretary of the DSIR (with whom Hill did not get along), as the secretary for the physical sciences in 1938. Unaware of Tizard's proposal, in June 1939 Egerton relayed to Bridges a far more ambitious one written by Hill that called for the Royal Society to form a committee to act as a sort of mediator between "Government" and the "scientific community" in both civil and defense matters.[39] Lord Chatfield, Inskip's replacement as Minister for the Co-ordination of Defense, approved the idea and asked for a formal proposal.[40] When a letter arrived for Chatfield from Sir William Henry Bragg, the president of the Royal Society, it included the unfortunate passage:

> The research organizations belonging to separate departments may easily, to a greater or less extent, become isolated from one another and from the main stream of independent scientific thought and discovery. Being in each case devoted to a particular service and employing a particular group of men, they may, by reasons of secrecy or departmentalism

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[38] Bridges memorandum, 1/14/1939, TNA: PRO CAB 21/711. Bridges was careful to note that his meeting with Tizard was "extremely informal (over the luncheon-table)" and warned against quotation, but recorded the response "as indicative of [Tizard's] point of view." McGucken, *Scientists*, pp. 168-169 moves through Tizard's proposal shockingly quickly, by McGucken's standards.

[39] The scheme was to build on the progress being made by the Society and the Ministry of Labour in building a Central Register of scientific manpower. McGucken, *Scientists*, chapters 7 and 8, has described in exhaustive detail Egerton and Hill's multi-year struggle, which we will move through quickly to bring out relevant themes.

[40] See A. V. Hill, "A channel for consultation and advice on general scientific matters between H. M. Government and the Scientific Community" sent by A. C. Egerton to Bridges, 6/14/1939, TNA: PRO CAB 21/711 and Bridges memorandum, 7/6/1939, TNA: PRO CAB 21/712.

> or merely lack of contact, be unaware of advances made in other departments or by scientific people in the world at large.[41]

The statement reflected a certain academic bias (familiar from our brief look at Lindemann) against the creativity and quality of research undertaken in the supposedly isolated worlds of non-academic laboratories.

The letter, of course, invited disaster. The tenor of the debate that ensued was to what extent it was true, and what, in any case, a centralized Royal Society committee could do about it. Those sympathetic to the Royal Society proposal, including a generally uninvolved Tizard, took the view that any new liaison that could be fostered would be a good thing.[42] Those opposed, including, significantly, DSIR Secretary and CSSAD member E. V. Appleton, saw the proposed committee as both a time waster, and an affront to the research establishments. Continuing a familiar theme, they suggested that Bragg, Hill and Egerton were the ones who were isolated from the realities of state research and development. As John Buckingham observed,

> It was clear that Sir William must be ignorant not only of the high degree of co-operation sustained between Governmental Establishments but also of the scope of exchanges made between Governmental Establishments and the scientific world. Some 30-40 Fellows the Royal Society were already actually engaged by Departments and the best available scientific brains were habitually applied for the benefit of Departmental Research.[43]

At best the new body would be superfluous, while, at worst, it would undermine the existing organs of scientific advice that had been established in the twenty years since the First World War.

The Royal Society's supporters were unable to come to terms with the objections to the scheme. Following a second proposal that Bragg floated just after the onset of war in September 1939, Wing Commander William Eliot, the officer in Bridges' office

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[41] Bragg to Chatfield, 7/13/1939, TNA: PRO CAB 21/712.

[42] On Tizard's support see Elliot to Hill, 8/29/1939, TNA: PRO CAB 21/712.

[43] Committee of Imperial Defence, Man Power (Technical) Committee meeting, 7/24/1939, draft minutes, TNA: PRO CAB 21/712.

handling the matter, felt that only the research establishments' "amour propre" and the desire to keep "skeletons in the cupboard" well-hid prevented passage of this sensible plan. He noted the great modesty of this new version. It gave Egerton and Hill no executive powers, and was simply meant

> to establish a means of liaison between Government Departments on the one hand and the whole scientific community, as represented by the Royal Society, on the other hand.... Professors Hill and Egerton will merely be intelligent agents whose virtue will lie, not so much in their own scientific qualifications as from their knowledge of the scientific qualifications of others.[44]

Eliot was clearly convinced by the Tizardian virtue in which Hill and Egerton cloaked their proposal. However, we can also see how the proposal excluded research establishments from the "scientific community", suggesting instead that the administrative elite of the Royal Society—but not even its ordinary Fellows who consulted with the research establishments regularly—remained the most qualified gatekeeper to the "main stream" of British scientific knowledge, as Bragg had called it. In any case, in addition to the usual resistance from the Admiralty (with Churchill now in as First Lord), civil science representatives Sir Edward Mellanby and Sir Edwin Butler, the heads of the Medical and Agricultural Research Councils now joined Appleton in opposition.[45] Eliot continued to see the episode in terms of entrenched resistance, and expressed his amusement that Appleton, a Cambridge professor, should become "*plus royalist que le roi*," more royalist than the king, so soon after taking up his new government post.[46] By summer 1940, though, the Royal Society seems to have lost its supporters in Government and was more or less dismissed as hopelessly out-of-touch

[44] Elliot memorandum to Secretary, 10/2/1939, TNA: PRO CAB 21/1163.

[45] See Churchill to Chatfield, 9/28/1939; Mellanby to Bridges, 10/26/1939; Appleton to Bridges, 10/26/1939; Stanhope to Mellanby, Butler and Appleton, 11/10/1939; all TNA: PRO CAB 21/1163.

[46] Elliot to Secretary, 10/30/1939, CAB 21/1163. Hill, for his part, came up with an extensive list of positive contributions that outside scientists had already made to the services, thus more or less fueling the "royalist" argument, see attachment to 10/2/1939 memorandum from Elliot to Secretary, TNA: PRO CAB 21/1163.

with state research, appearing to simply assume that it was ill-coordinated on the basis of scattered horror stories and the fact that the research was secretive.[47]

However, the Society was persistent. In February 1940, Hill had been elected in a by-election to the House of Commons as an "Independent Conservative" for Cambridge University, and was publicly criticizing the government organization of science from this platform and others.[48] His rhetoric, combined with other outside agitation, convinced members of the Government to establish some sort of committee, as Undersecretary of the Treasury Sir Alan Barlow wrote to Lord Hankey "if only to keep the scientific people quiet."[49] Hankey agreed to take the chair and to take it seriously, and the Scientific Advisory Committee that resulted, though it had no jurisdiction over military research, was not without its merits.[50] However, it hardly played the role its proponents envisioned, and it failed to keep them quiet. Hill, in particular, continued to speak out rabidly and publicly against the organization of government research establishments for another two years, demanding newer, more centralized committees, but it is difficult to say what sort of organization would have actually sated his misgivings.[51]

Hill was committed to Tizardian administrative virtues, but was uncomfortable with their requisite faith of coordination in the lower rungs of hierarchical administrative organs. He preferred to channel knowledge of all scientific activities through an apparatus of which he was a member. He wrote to the sympathetic Tizard,

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[47] See especially unsigned "Royal Society" memorandum, 8/31/1940, TNA: PRO CAB 21/829.
[48] There is some evidence that Hill's rhetoric was intentionally overblown, commensurate with the requirements of politics, though he himself found such methods distasteful. See Katz, "Hill," p. 116.
[49] Barlow to Chancellor of the Duchy of Lancaster, 9/18/1840, TNA: PRO CAB 21/829.
[50] Hankey to Barlow, 9/20/1940, TNA: PRO CAB 21/829. See McGucken, *Scientists*, Ch. 8, on the activities of the Scientific Advisory Committee.
[51] See A. V. Hill, 4/22/1942, "A Joint Technical Staff" and 6/22/1942, "A scientific and technical General Staff", TNA: PRO CAB 127/218.

> The key idea, as you know, is to link technical policy right up to the top with tactical and strategical policy and to have lines of executive authority running right downwards from the top both in the services and in the supply departments, dealing with technical matters, not simply to trust to rather casual cross linkages by 'co-ordination' or 'liaison'. [52]

Diverging from Tizard's high regard for cross linkages and coordination in industrial organization, Hill cited the self-coordination brought about through the open dialogue of university science as the environment he wanted to approximate,[53] but he never made it clear how the mechanics of his proposed centralized infrastructure would simulate that kind of atmosphere within the nation's mammoth research and development institutions. His own statements were vague, uninformed, and bordering on contradictory. In a 1941 speech he warned against "the danger that science will be planned by administrators in offices instead of by young men with their sleeves rolled up, in laboratories or workshops."[54] In 1942, though, he blamed British defeats in Libya on poor tank design and complained to *The Times*, "There is no central technical staff or individual to advise the Cabinet or the Chiefs of Staff, directly on the scientific and engineering aspects either of operations or production, or to ensure, on their behalf, that design and development are efficient and far-seeing. All such functions are left to departments."[55] He seemed to be insisting that an ultra-centralized organization be set up in order to ensure that decision

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[52] Letter from Hill to Tizard, 3/1/1942, *HTT* 57.

[53] A. V. Hill, "The Use and Misuse of Science in Government," speech given 9/26/1941 to the British Association for the Advancement of Science. Printed in *The Advancement of Science*, 2 (1942) 6-9, p.7.

[54] A. V. Hill, "Science, National and International, and the Basis of Cooperation," given to the Parliamentary and Scientific Committee, a scientific lobbying organization, and read in the House of Commons. Printed in *Science* **93** (1941): 579-584, on p. 582. This speech, and its publication in *Nature* (**93** (1941): 579-584), particularly riled the Admiralty's DSR Charles Wright, who was frustrated by his inability as a civil servant to publicly reply to the publication of critiques contained in a government committee's report; see minute from Wright, 3/21/1941, TNA: PRO ADM 1/11577.

[55] A. V. Hill, Letter to the Editor of *The Times*, published 7/1/1942, copy in TNA: PRO CAB 127/218. The editorial drew a private rebuke from the insider M. M. Postan, *who supported Hill's centralizing scheme* but knew the tank criticism was off target (though he was unable to reveal why on account of the "official secrets act"). Postan to Hill, 7/1/1942, *AVHL* I 2/2 Part II. Postan would, of course, later go on to co-write the official insider history, *Design and Development of Weapons*.

making was sufficiently decentralized. Meanwhile more workable means of improving coordination were developing elsewhere.

Scientific Advisers and Operational Research Officers, 1939-1941

The foremost concern for Tizard, Hill and their supporters following the outbreak of war were the symmetrical problems of whether laboratories were developing equipment that adequately reflected changing military requirements, and whether military executives were making the best use of existing equipment in view of those same requirements. One new approach to the issue was the establishment of scientific advisory positions within the military operational chain of command itself, not simply within and between the laboratories and supply branches of the military.[56] Instead of making elite scientists into overseers of research establishments' R&D priorities, scientific advisory positions made them into facilitators serving the military's *ordinary* liaison mechanisms between operational commands and the research establishments. Unlike the Royal Society proposal, this development was more or less uncontroversial among insider scientists and overworked military officers alike, and between 1940 and 1943 these positions proliferated rapidly.

Tizard himself became the first appointment. While still chairman of what was now called the Committee for the Scientific Survey of Air Warfare (CSSAW), he was made Scientific Adviser to Chief of Air Staff in November 1939, specifically to help link

[56] The problem of coordination must be viewed in light of the separation of the supply ministries (the Ministry of Aircraft Production and the Ministry of Supply) from their corresponding service ministries (the Air Ministry and the War Office—the Admiralty retained control of its supply chain). It would be impossible to make any sweeping statements about the effectiveness of R & D coordination with military requirements without first examining the links between these ministries.

research and development with the latest demands of the war.[57] When Churchill became Prime Minister in May 1940, however, Tizard's position in the Air Ministry became tenuous. According to the resignation letter he sent to Sir Archibald Sinclair, the new Secretary of State for Air, Tizard arrived uninvited to what he had been told was an informal meeting on June 7th and was "very surprised to find a formal meeting on a high plane with Professor Lindemann installed as your principal scientific adviser, discussing a priority list which I was primarily responsible for drawing up." The scene has the bizarre quality of a spouse walking in on an act of infidelity, but Tizard couched his resignation in terms of the incident's repercussions for the bureaucratic machinery, writing:

> For some years, before the war, as well as since the war, I have had the chief responsibility for giving the Air Ministry independent advice, and have been Chairman of a strong committee appointed for the purpose. We have worked throughout in close personal touch with the Air Staff and I have made it my business to study the operational needs and difficulties of the Commands.

He continued that it was not appropriate that conflicting bureaucratic machineries for advice should exist, and to avoid "confusion" and the possibility that technical advice would not be "properly co-ordinated," he felt it was important that he leave.[58] The CSSAW was dissolved shortly thereafter,[59] and, at the end of the summer Tizard led his mission to the United States to exchange research advances.[60] When he returned he

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[57] See memo from HQ Bomber Command, 11/12/1939, "Scientific Adviser to C.A.S. – Liaison with Bomber Command", TNA: PRO AIR 14/98.
[58] Letter from Tizard to Sinclair, 6/19/1940, *HTT* 58. Tizard's letter is reproduced in full in Clark, *Tizard,* pp. 233-234. Sinclair (the leader of the waning Liberal Party), had replaced Sir Samuel Hoare as Secretary of State for Air, who himself had replaced Kingsley Wood. David Edgerton has suggested to me that the committee Tizard referred to was the Aeronautical Research Committee, not the CSSAD/CSSAW, but ultimately the letter is not specific enough to say for certain which committee Tizard had in mind. Regardless, the broader point remains valid.
[59] The resignation of A. V. Hill from the CSSAW prompted its dissolution. Note that Hill was in the midst of his struggles as Secretary of the Royal Society to establish a high level committee at this time and also threatened to air the controversy publicly in the House of Commons. See Clark, *Tizard*, pp. 241-242.
[60] Tizard's mission was most famous for its introduction to American laboratories of the cavity magnetron device, which would permit powerful short wavelength radars to be developed. See David Zimmerman,

obtained his position as an independent adviser to the Ministry of Aircraft Production (MAP), which had broken off from the Air Ministry that spring.[61]

Very shortly after Tizard was appointed scientific adviser to the CAS, Watson Watt, serving as the Air Ministry's Director of Communications Development, was elevated to another new Air Ministry post, Scientific Adviser on Telecommunications (SAT). In September 1940, Watson Watt was transferred from the Air Ministry to MAP where he came under the authority of its Controller of Telecommunications Equipment, the ubiquitous Frank Smith. The Assistant Chief of Air Staff for Radio, Air Marshal Philip Joubert de la Ferté, insisted, though, that Watson Watt continue to spend most of his time at the Air Ministry. Watson Watt, doubtless more than willing to retain his influence there, borrowed three scientists from the TRE to function as "Operational Research Officers" (OROs) to liaise on his behalf with the three home commands of the RAF—Fighter, Bomber and Coastal Command—and to apply telecommunications technology to Air Staff needs. The aforementioned Stanmore Research Section was already over a year old, and Watson Watt wanted its head, Harry Larnder, to serve as his ORO to Fighter Command as well. John Kendrew, who was already liaison for the TRE to Coastal Command, would become its ORO. A. O. Rankine would be ORO for Bomber Command. A. F. Wilkins, Watson Watt's erstwhile assistant, would continue to serve that function as the ORO at Air Ministry HQ.[62]

Top Secret Exchange: The Tizard Mission and the Scientific War (Buffalo: McGill-Queens University Press, 1996).

[61] According to Snow, "Science and Government", p. 42, after this incident "there was no more authority for him in that war." Lindemann had won. It is unclear exactly why Snow felt that Tizard's MAP position was meaningless.

[62] See minutes and correspondence in TNA: PRO AIR 2/3181 and TNA: PRO AIR 2/8587.

In January 1941 Joubert created the ORO posts for Watson Watt, and put not only Larnder, but the entirety of Stanmore Research Section under his control.[63] The move was welcomed by all three home commands. Bomber Command, upon learning that Rankine would only study problems of RDF in bomber aircraft, complained that they wanted him to study the entirety of their signals organization.[64] Air Marshal William Sholto Douglas, the Commander-in-Chief at Fighter Command, was fearful that Larnder's reports would bypass him on their way directly to Watson Watt and the Air Staff, and used the opportunity to lobby for Larnder's Section to come directly under the Command rather than continue under MAP.[65] It did not happen. Even still, the bureaucratic structure was quickly set up and appeared to be working well.[66] By this time, though, there were other events brewing that would permanently dissociate Watson Watt from the nascent idea that operational research was a necessary corollary to scientific advice.

The first time that General Frederick Pile, the head of Anti-Aircraft (AA) Command in the British Army, contacted A. V. Hill had been on February 1938 when he was commanding the 1st Anti-Aircraft Division. In discussing problems he was having training AA gunners from the Territorial Army, Pile learned that Hill was an unrivaled expert on the topic based upon his experience in the First World War.[67] However, they seem to have had no further contact for another two and a half years, until after Hill had become an associate member of the Ministry of Supply's Ordnance Board (formerly the

[63] Stanmore Research Section continued to be administered by the TRE. See minute from Watson Watt, 1/7/1941, TNA: PRO AIR 2/8587.
[64] Saundby to Under Secretary of State, Air Ministry, 1/17/41, TNA: PRO AIR 2/8587.
[65] Douglas to Under Secretary of State, Air Ministry, 1/27/41, TNA: PRO AIR 2/8587.
[66] At Coastal Command, Kendrew, on behalf of the TRE, was already producing a series of reports on the implementation of RDF in the command and he continued to do so as ORO. They can be found in TNA: PRO AVIA 7/1004 along with later Operational Research Section, Coastal Command reports.
[67] Pile to Hill, 2/17/1938, *AVHL* I 2/1.

Ordnance Committee) and the Chairman of an its AA committee. In the middle of the Battle of Britain, on August 9th, 1940, Hill, Pile, and the Ministry of Supply's DSR, H. J. Gough, convened a relatively large meeting of military scientific personnel to discuss Pile's technical problems. Following the meeting Hill and Pile decided that Patrick Blackett, who at that time was developing predicting bomb sights for the Royal Aircraft Establishment, should become Pile's scientific adviser to solve the Command's problems with gun-laying predictors.[68]

When MAP loaned Blackett to Pile's staff, he set up a small group of skilled academic scientists. This group, officially called Research Group but popularly known as Blackett's Circus, was also the first component of what later became the Army Operational Research Group. Although Blackett undertook some studies and offered some advice on broad tactical problems such as pointing out statistical fallacies in the best distribution of AA guns around London, the issues that his group handled in the fall of 1940 were overwhelmingly related to the calibration of fire control mechanisms and adaptation of AA training and firing methods to the operational demands of the Battle of Britain.[69] In this sense, the group was very much like Hill's Anti-Aircraft Experimental Section from the previous war. Accordingly, following Blackett's departure in March 1941, the group was soon folded into the RDF-centered Air Defence Research and Development Establishment (ADRDE) in the Ministry of Supply and renamed the

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[68] Materials related to this meeting can be found in AVHL I 2/3 Part I, including invitations and meeting notes. The Royal Aircraft Factory had been renamed the RAE shortly after the institution of the Royal Air Force at the end of World War I.

[69] Blackett has remarked on the work of the command in P.M.S. Blackett, "Recollections of Problems Studied," in *Studies of War*, 205-234, on pp. 208-210, but also see AORG Memorandum No. 615, L. E. Bayliss, "The Origins of Operational Research in the Army," 10/11/1945, copy available in TNA: PRO WO 291/887; and AORG Memorandum No. F.10, A. W. Ross, "An Introduction to the Army Operational Research Group," August 1955, TNA: PRO WO 291/1437.

ADRDE Petersham Research Group.[70] It therefore can be considered, administratively and functionally, as a sort of army twin to Larnder's Stanmore Research Section at Fighter Command.

This is not to say that the establishment of Blackett as Pile's scientific adviser was not an important moment in the politics of British science in the war. For Hill, who was at this time struggling to institute his Royal Society committee, installing Blackett, personally, at AA Command was *immediately* significant. In writing to Blackett on August 12th to offer him the job, he pointed out, "You would be able to do for the AA Command what Tizard ought to have been given a chance to do for the RAF," referring to Tizard's rude awakening to Lindemann's advisory supremacy two months earlier.[71] Blackett's appointment would also soon enter Hill's arsenal as an example of independent scientific aid to the services as an argument in favor of his Royal Society committee, and once his Scientific Advisory Committee was set up in October, Hill began to use his position to push for analogous positions to be set up elsewhere.[72]

In January 1941, just as Watson Watt was setting up his OROs, Hill proposed through Hankey that a scientific adviser, akin to Blackett at AA Command, be posted at Coastal Command to work on the application of anti-surface vessel (ASV) radar to the hunting of German U-boats that were increasingly threatening transatlantic trade and, thus, the whole war effort. Hill had Cavendish physicist John Cockroft in mind for the

[70] The term "operational research group" was considered for the group at the time, but the word "operational" was objected to on "security grounds," until September 1941, when it became the ADRDE ORG, and was later renamed OR Section 1 of the Army Operational Research Group. See Bayliss, "Origins," p. 6.
[71] Hill to Blackett, 8/12/1940, *PMSB*, PB/9/1/52 (formerly J.35).
[72] See Letter from A. V. Hill to Lord President of the Council Neville Chamberlain, 9/21/1940, TNA: PRO CAB 21/829. See also Chairman of the Scientific Advisory Committee to the Lord President of the Council, 3/3/1941, TNA: PRO CAB 21/1165. This letter is in Hankey's name, but it would be surprising if Hill did not have some hand in its writing.

post.[73] Cockroft had just returned from America as part of the 1940 Tizard Mission and was presumably free for a new assignment, but his employers at MAP had already promised him to the Ministry of Supply where he was to become Chief Superintendent of the ADRDE.[74] Meanwhile, the Air Ministry consulted Watson Watt about the idea. Watson Watt, unaware of Cockroft's unavailability, was aghast that such an "exceptionally good physicist of strong personality, exceptional energy and drive" should be assigned to be an independent adviser to work on a single problem within a single section of a single defense service. He thought that employing Blackett, another skilled and driven physicist, at AA Command was already "anomalous and embarrassing" but was "partially justified by the absence of any other provision for operational research in the War Office field."[75] He had bigger things in mind for Hill's suggestion that were less threatening to his own position.

Watson Watt was acutely aware of any challenges to his status as the Air Ministry's only remaining high-level technical adviser. In his letter to the Air Ministry, Hankey had mentioned that in appointing an outside adviser on ASV issues, he did not want to "tread on the toes" of the Ministry's own advisers "such as Mr. Watson Watt." But Watson Watt wryly observed that the proposal "shows concern for the toes of Mr. Watson Watt when attention should have been concentrated on his opposite extremity."

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[73] Letter from Lord Hankey to Sir Archibald Sinclair, 1/6/1940 [sic, 1941], TNA: PRO AIR 19/148.
[74] Letter from Lord Hankey to Sir Archibald Sinclair, 1/21/1941, TNA: PRO AIR 19/148.
[75] Memorandum from Robert Watson Watt to Private Secretary to the Secretary of State, 1/24/1941. The memorandum is unsigned but a reference to "my" operational research officers leaves its author in no doubt. Erik Rau, "Combat Scientists", p. 62, however, has quoted the "anomalous and embarrassing" line from John Buckley, *The R.A.F. and Trade Defence, 1919-1945: Constant Endeavour* (Keele: Ryburn, 1995), p. 172, who attributes it to the Permanent Under Secretary for Air, Sir Arthur Street. The quote, misattributed and used out of context, gives the impression that the War Office was recalcitrant to scientific interference, but was clearly intended by Watson Watt to mean that Blackett was too prestigious to do technical operational research. Buckley, incidentally, gives a strong overview of the use of OR in RAF Coastal Comman.d

So, he suggested that the new post should instead be expanded to work on the antisubmarine problem across service boundaries in order to goad the services into a vigorous and unified antisubmarine campaign. The position would serve as a sort of an antisubmarine counterpart to Watson Watt's telecommunications position. As for the study of ASV radar use in Coastal Command, this service was best supplied by detachments from MAP staff to serve as operational research officers reporting to the SAT, and John Kendrew was already there.[76]

Watson Watt met with DSIR head E. V. Appleton, who was also on Hankey's Scientific Advisory Committee, and crafted a new plan along these lines, and concluded that Blackett himself should be brought in on loan for "a few weeks" to serve on a "roving commission" to report on the situation. Watson Watt noted, "Blackett is an ex-Navy man."[77] As a teenager, Blackett had served in the Royal Navy in World War I, which meant he had a chance to include the always prickly Admiralty in his studies and thus define the position around the topic of antisubmarine warfare; and, indeed, when Blackett arrived at Coastal Command the Air Ministry encouraged the Admiralty to make use of his services.[78] However, Watson Watt's plans immediately began to go awry. The Admiralty was in no hurry to make use of a MAP employee assigned to the RAF and Blackett's position rapidly became defined as a Coastal Command post rather than a topical roving commission. Even worse, in June the Commander-in-Chief of Coastal Command, Air Marshal Sir Frederick Bowhill, was replaced by Watson Watt's primary military patron, Air Marshal Joubert, who got along famously with Blackett. Blackett

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[76] Memorandum from Robert Watson Watt to Private Secretary to the Secretary of State, 1/24/1941, TNA: PRO AIR 19/148.
[77] Minute from Robert Watson Watt to Private Secretary to the Secretary of State, 1/28/1941, TNA: PRO AIR 19/148.
[78] Letter from Sir Archibald Sinclair to First Lord A. V. Alexander, 2/15/1941, TNA: PRO AIR 19/148.

stayed for months rather than weeks, and Kendrew was quickly joined by a group of scientists that Blackett began to gather around him.

Meanwhile, in May 1941, possibly already sensing the danger that Blackett presented to his ORO scheme, Watson Watt made a truly ambitious move to establish his dominance over scientific advice in the Air Ministry, submitting to the Vice Chief of Air Staff (VCAS), Air Chief Marshal Wilfred Freeman,[79] a plan to elevate himself to a position to be called Scientific Adviser to the Air Council (SAAC, "despite the humorists").[80] This position would deal not only with RDF, but with the operational deployment of *all* RAF technologies. His control over OR was key to this plan. He pointed to the success of Larnder's newly renamed Operational Research Section, Fighter Command, as both a boon to the implementation of RDF as well as to Fighter Command itself: the data that the section collected on all facets of the fighter interception problem could also be used to gauge the strengths and weaknesses of both British and German strategies, tactics and technologies.[81] He wanted to extend the ORS concept to the other Commands, and their staffs would spend most of their time in the field and would "do much vertical chasing through horizontal organisations ... because in chasing facts they could also push action." The leaders of the OR Sections would also serve as scientific advisers to the commands, but, to preserve their objectivity, they would be responsible to

[79] Freeman had been an early supporter of Watson Watt's RDF work when, before the establishment of MAP, he was the Air Member (of the Air Council) for Research and Development. His VCAS position, though, was created as something of a consolation prize when his Air Council post was abolished after MAP's creation. See Edgerton, *Warfare State*, p. 152.

[80] This title was Watson Watt's preference, but he was also willing to consider Tizard's old title, Scientific Adviser to the Chief of Air Staff. See "Memorandum on Operational Research" (unsigned and undated) and R. A. Watson Watt to VCAS 5/23/1941 (distilled version of "Memorandum"), attached to Letter from Watson Watt to DCAS, 10/3/1941, TNA: PRO AIR 2/5352.

[81] I paraphrase liberally. Watson Watt's description of OR's "extended" uses in this memorandum is nearly incomprehensible—a characteristic trait of his writing.

him.[82] To cap it off, he included under his jurisdiction a number of other semi-related scientific and technical positions including the Air Warfare Analysis Section, and the Assistant Director of Scientific Intelligence, the physicist R. V. Jones. Remarkably, Watson Watt's new scheme received Tizard's blessing, and was even approved by Freeman. However, after meeting criticism from Sir Arthur Street, the Permanent Under Secretary for Air, it was promptly ignored.[83]

The Rise of OR, June-October, 1941

Watson Watt's bold gambit was incapable of keeping his political balancing act from collapsing slowly but completely. By June 1941 his ORO at Coastal Command, John Kendrew, had become the Officer-in-Charge of ORS, Coastal Command, and there was still a nominal separation between him and the scientific adviser, Blackett. The concept of operational research itself, however, shifted subtly but significantly that summer. While Watson Watt's plan was supremely audacious, by his definition OR still only applied to the study of technology in use in the field, which could have a subsidiary use in command decision making.[84] Blackett apparently had other ideas.

At Coastal Command, the work of the ORS increasingly turned to the production of studies directly in support of Blackett's role as scientific adviser on *command decisions* regarding antisubmarine warfare. As described in Chapter Two, military

[82] Watson Watt actually suggested names for twelve individuals who would be under his authority. Harry Larnder was to be elevated to the post of Watson Watt's assistant, and Blackett's unique scientific advisory role in Coastal Command would be preserved in addition to Kendrew serving as the head of ORS Coastal Command.
[83] See Memorandum from Tizard to VCAS and CAS, "Operational Research," 7/17/1941, *HTT* 302.
[84] There is an intriguing annotation in pencil on Watson Watt's proposal to VCAS (see above) that we cannot connect with a specific author or time, but is worth quoting: "This seems to overlook entirely matters of operational practice, tactics and intelligence; confine the field to technical research. These sections are essentially OPERATIONAL research sections."

heuristics became subjected to the scientific adviser's scrutiny and uncertainties in the assumptions that informed military planning were, in turn, subjected to further inquiry by an OR team. It is important, though, not to overstate the magnitude of this shift. Because the determination of technical capabilities was crucial to military heuristic practice, studies of the effectiveness of equipment as deployed in the field remained important. So, the studies' radically new *function* of supporting the scientific adviser to a command officer produced no clear shift in their *content*. Rather, *some* reports now bore little relation to the *technical* problems of the Command, including the Section's first report, an intelligence-like digest of detected enemy aircraft activities in the western approaches in the month of May.[85] Even a year later one scientist in the TRE had not appreciated the change and complained that many of the studies they received from ORS Coastal Command did not answer questions pertinent to their concerns.[86]

By July, though, Tizard had already picked up on what was afoot. Ever since Street had raised objections to Watson Watt's proposal, he had put some deeper thought into the question of operational research and had decided that he had been mistaken and could not support the scheme after all. He laid out his thinking in a memorandum to the CAS and VCAS on July 17th.[87] Tizard's conclusions represented a crucial moment in the history of OR, and are critical to understanding the subsequent history of scientific advice in the British military not only during the war, but at least until the complete

[85] ORS Coastal Command (CC), Report No. 125, "Activity of Enemy Aircraft in the Western Approaches in May, 1941," TNA: PRO AIR 15/731. It is unclear why numbering began with 125.
[86] Letter from Holt Smith to Prof. Williams, 7/7/1942, TNA: PRO AVIA 7/1005. Holt Smith was complaining that ORS CC Report No. 185, "Air Escort for Convoys" did not address questions related to ASV radar. Many other ORS CC reports did.
[87] Memorandum from Tizard to VCAS and CAS, "Operational Research," 7/17/1941, *HTT* 302. All quotes not otherwise cited in the subsequent four paragraphs are from this document.

reorganization of the British military command structure in 1964, so we will examine the memorandum and the response to it in some detail.

First, Tizard must have recognized that the work Blackett and Watson Watt were engaged in was already taking place in a less crystallized fashion throughout the services, and so he asked the Air Ministry not only for all instances of "the type of operational research that is now being carried out at the Commands and by the various sections of the Air Staff," presumably referring to Watson Watt's definition of the term, but also for "the operational statistics that are now being collected in any form." In short, he wanted insight into the side of military heuristics not directly connected to the research and development establishments.

Although his survey remained incomplete, he felt confident in suggesting that "the object of operational research is to endeavour by scientific study of the results of operations against the enemy to arrive at methods for using existing military equipment in the most economic and effective way, and to guide future technical *and operational* policy."[88] This definition led him thoroughly outside his familiar realm of research and development policy to the important conclusion:

> It must therefore cover a wide field and it should be, and no doubt is, regarded as *a normal function of a well organized force*. If we accept that broad definition it follows that the executive responsibilities should be entrusted to high officers of the Royal Air Force and not to civilians or independent scientific men.[89]

OR, although "scientific study" by definition, was one and the same with the long-standing military heuristic practices, and, as such, Watson Watt, a *technical* adviser, had no proper place in it. In response to another OR scheme Watson Watt proposed on September 1st, Tizard decided to brush him off entirely. He wrote to Freeman:

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[88] Emphasis added.
[89] Emphasis added.

> The chief reason why Watson Watt is busying himself so much on the organization of operational research (at which I do not think he is very good) is that he is not being properly fully used in ways which are appropriate to a scientific adviser on telecommunications. I have suggestions to make to you which I think will enable him to pull his weight, which is considerable, in appropriate directions.[90]

Tizard never mentioned that his ideas about the new direction in OR would also make Blackett's role in Coastal Command redundant, but he probably still considered Blackett's position to be a temporary one to help Joubert establish effective antisubmarine policies.

Tizard was fully attuned, however, to the role that trained scientists could play in the day-to-day work of operational research. "Although I feel now, after mature consideration, that these operational research groups must be responsible to the Commanders-in-Chief or directly to the Air Staff, it does not follow that they should be staffed by serving officers," he wrote. "The work itself demands a scientific training and outlook, and the staff should therefore be appointed largely, if not entirely, on the recommendation of the Director of Scientific Research and the Controller of Telecommunications Equipment," David Pye and Frank Smith, respectively, both career civil servants and Fellows of the Royal Society. He seems to have decided that scientific personnel could best determine what kinds of data should be gathered to elucidate operational problems pointed out by military commanders, and what methods could best be employed to obtain it.

Revising Watson Watt's conception, then, operational research would have *primary* significance to the commanders, and, if good liaison was practiced, such as through the DSR, it would have *secondary* benefits to technical advisers. While we have

[90] Minute from Watson Watt to ACAS(I), 9/1/1941, TNA: PRO AIR 2/5352; and Minute from Tizard to Freeman, 9/2/1941, full version available in *HTT* 302; an excerpted version not including the quote is available in TNA: PRO AIR 2/5352.

so far looked to Fighter and Coastal Command as the prototype models for OR, Tizard used Watson Watt's ORO at Bomber Command, A. O. Rankine, as his example of successful work already being done. He wrote that Rankine was "welcomed as an individual and given freedom to examine any data which he wishes to examine or which he is asked to examine by S.A.T. and others, and generally treated as an expert guest from whose activities something of interest may emerge." Tizard recommended that Rankine's post be enlarged into a staff, so that "the Commander-in-Chief should be naturally turning to them for analysis of his operations and for advice on any problem where scientific analysis can help him to guide operational policy or to formulate his needs with more certainty than at present." That example could be applied to the other Commands. He also tentatively suggested that the Deputy Chief of Air Staff (DCAS) could set up a mixed military-scientific committee to consider OR reports to see if they uncovered any points of higher command interest in their work.

Tizard's memorandum seems to have tapped into a latent need and so provoked a remarkable reaction: from then until October events proceeded rapidly. On July 23rd Rankine, citing disagreements with Watson Watt, quit his post.[91] On August 8th, the Commander-in-Chief of Bomber Command, Air Marshal Sir Richard Peirse, having discussed Tizard's memorandum with the Chief of Air Staff, demanded an Operational Research Section of his own. He submitted to Street a list of problems in need of elucidation for the improvement of night bombing and reducing casualty rates:

> (i) The method of operating enemy night fighters and searchlights.
> (ii) Location of enemy searchlights and A.A. defences.
> (iii) Location of enemy night fighter zones.
> (iv) Routeing of our own bombers to avoid enemy night fighters and A.A. defences.
> (v) Method of approach to the target in order to obtain a good bombing run with the minimum interference from searchlights and flak.

[91] Letter from Rankine to Tizard, 7/21/1941, *HTT* 352.

Note, consistent with our earlier discussion of military heuristics, that some of these problems could be handled through traditional avenues such as intelligence-gathering, and others, such as bomber routing and the methodology of the bombing approach, were heavily dependent on doctrinal theories that had to be carefully cross-checked against operational results. Even still: "All this," Peirse pointed out, "requires a large amount of research and the analysis of the data needs a trained scientific mind in order to draw from it the correct deductions." He observed that Bomber Command had already made progress, but felt "that we are still far from making the best use of the data that is available."[92] Tizard wasted no time in helping to build up a staff around a small core of analysts already at Bomber Command.[93] By the end of the month, he and David Pye had selected Basil Dickins, a PhD-level scientific civil servant then working in Pye's office, to lead the construction of the ORS—he remained there until the end of the war.[94]

Adding fuel to the fire, the prominent crystallographer J. D. Bernal, then doing research on bombing effects on England for the Ministry of Home Security, (according to a story he later told to C. P. Snow) drove Lord Cherwell in his "very dingy vehicle" to Bomber Command, where they bypassed the official reception that would ordinarily greet the Prime Minister's adviser. Here Bernal introduced him directly to the Command's photographic analysts, "a very lowly part of the organisation," and showed him first-hand the extreme inaccuracy—on the scale of miles—of nighttime bombing raids on

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[92] Letter from Peirse to Under Secretary of State, 8/8/1941, TNA: PRO AIR 2/5352; and Minute from DCAS to Tizard, 8/12/1941, TNA: PRO AIR 2/5352.

[93] "Dr. Roberts", from MAP's Director of Communication Development's office was culling information from wireless interceptions and radio activities. "Miss Goggin" of DSR David Pye's staff at Air Ministry Headquarters was working under Bomber Command's Chief Armaments Officer. See Minute from DCAS to Tizard, 8/12/1941, and Minute from DSR to DCAS, 8/27/1941; TNA: PRO AIR 2/5352.

[94] Minute from Tizard to DCAS, 8/21/1941, TNA: PRO AIR 2/5352. Dickins had been a practitioner of what was called "operational research" in the early days of RDF testing at Biggin Hill.

Germany.[95] Cherwell ordered his assistant, David Butt, to undertake an examination of Bomber Command's photographic reconnaissance. Butt's report, issued August 18th, is largely indistinguishable from an OR report. It detailed the statistics of bomb plots and likely causes of error, and called for further analysis of the problem as well as the establishment of a statistical section at the command. Butt noted, though, that his own abilities were limited by his lack of "skill at interpreting photographs and technical knowledge of bomb damage."[96]

The Air Ministry considered the report "fair," but added mitigating context to the statistics, pointing to the inexperience of bomber crews, the fact that crews told to photograph their missions were ones suspected of inadequate performance, and the exceptionally bad weather during the period the study examined. They also pointed out that, "on the authority of Sir Henry Tizard," the Germans exhibited a remarkably similar degree of inaccuracy in striking English urban districts.[97] Still, they were quick to add that these caveats were "not put forward in any spirit of self-satisfaction and complacency." The techniques of night bombing had been under study for years, and the recent challenges of the war "served to give new zest and determination to our studies of means to overcome them." They acknowledged Butt's call for "further operational research" (Butt himself did not use the words), and pointed to the establishment of their new ORS—the news of which Cherwell warmly welcomed.[98] The Ministry also promised to develop means of radar navigation, which is usually cited as the major

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[95] Letter from J. D Bernal to C. P. Snow, 4/11/1961, *JDB*, J.217. See also Crook, "Science and War," pp. 81-89.
[96] A copy of Butt's report, dated 8/18/1941 is available in TNA: PRO AIR 14/1218.
[97] Tizard apparently received this information from Bernal who was performing a survey of German bombing for the Ministry of Home Security. See Letter from Tizard to Medhurst, 9/4/1941 (2nd of 2), TNA: PRO AIR 2/5352.
[98] Minutes of the 22nd Meeting of the Air Fighting Committee, 8/28/1941, p. 2, TNA: PRO AIR 2/8653.

accomplishment of Butt's report, although they were worried about the Germans capturing the equipment and using it against them.[99]

Meanwhile, the RAF's Assistant Chief of Air Staff for Intelligence, Air Commodore C. E. H. Medhurst, informed high Ministry officials and Tizard that he had for some time been considering conducting a survey of the German defense system, and had felt that there must be a large amount of "uncorrelated" information already available, and that it was important to work out how the various elements of enemy defenses—searchlights, anti-aircraft guns, night fighters and radiolocations—impacted the effectiveness of their defense network. He believed only a "scientific brain" could best "pull together all the various threads and produce a comprehensive picture," and had already discussed the issue with the Assistant Director of Scientific Intelligence, R. V. Jones. Now he was anxious to ally Jones' efforts with the new Bomber Command ORS.[100] Tizard, of course, greeted the offer with open arms and immediately began concocting a scheme to coordinate the efforts of Jones, the various OR sections, the Air Warfare Analysis Section, and the Ministry of Home Security's bombing effects survey. Tizard wanted to promote Larnder to coordinate the effort, drawing swift complaints from Air Marshal Douglas and Larnder himself who wanted to stay at Fighter Command.[101] In the end, Larnder's title was changed to scientific adviser to Fighter

[99] See responses by the Senior Air Staff Officer (SASO) at Bomber Command Air Vice-Marshal Saundby, dated 8/21 and 8/29/1941; and John Baker to SASO, 9/5/1941, "Notes on Lord Cherwell's Paper dealing with the Analysis of Night Photographs taken by Bomber Command during their attacks in June and July 1941" which borrows heavily from Saundby's drafts; all from TNA: PRO AIR 14/1218.

[100] Letter from ACAS(I) to Tizard, 8/30/1941. Medhurst also apparently contacted Watson Watt to see about the availability of OR staff for handling secret intelligence, which prompted the latter to suggest a new scheme for OR organization, with him at the top in his role as SAT with Larnder as his assistant. This scheme is apparently what prompted Tizard's aforementioned brush-off. Minute from Watson Watt to ACAS(I), 9/1/1941, TNA: PRO AIR 2/5352.

[101] See three letters from (an excited) Tizard to Medhurst, dated 9/1/1941 and two from 9/4/1941, TNA: PRO AIR 2/5352.

Command, and it was decided to create an Operational Research Centre that would be under commissioned University College Southampton physicist Wing Commander A. C. Menzies and would be responsible for the correlation and distribution of OR reports. To facilitate coordination of the work, periodic meetings of an Operational Research Committee were to be held that would include OR personnel, military staff and other concerned scientific personnel. At some point in the fervor to build and coordinate OR, Watson Watt was included in this last category.[102]

When Watson Watt discovered that he "was added, as an afterthought, to the 'reserve team,'" he issued a "mild protest" to the DCAS, Air Vice-Marshal Norman Bottomley, and enclosed his plan that Freeman had supported in May and the January order from Joubert establishing his original ORO system as explanation for his surprise that the Air Ministry had "summarily disposed of a large block of staff still 'operationally under the command' of SAT." Bottomley replied, apologizing, "I am afraid I had no idea that the Operational Research Sections were 'operationally under your command.'" He wrote that he had been working with Tizard and Pye on the matter, pointed out that he

[102] Amusingly, Tizard's well-structured OR scheme seems to have wiped the board clean on OR's early history, prompting a letter to Joubert inquiring what an ORS might look like at Coastal Command. Joubert responded he already had an ORS under Blackett, and that it would be desirable to make this arrangement permanent. See A. C. Stevens to Joubert, 9/13/1941, and Joubert to Stevens, 9/18/1941, TNA: PRO AIR 2/5352. On the OR Centre and Committee see Memoranda from DCAS, 9/20/1941; from DCAS to Joubert, 10/2/1941; and from Pye to DCAS, 10/9/1941; all from TNA: PRO AIR 2/5352. The OR Committee held its first meeting on October 31st, 1941 and typically met once every one or two months between then and April 1943. It was chaired by the DCAS and the ACAS(Operations), and OR scientists actually constituted a minority of the participants. The meetings were primarily high level Air Ministry and MAP affairs, although representatives from the War Office, Ministry of Supply, Admiralty and Ministry of Home Security occasionally attended. Their usual purpose was to manage the transfer of personnel, to establish channels for information flow, to reinforce liaison between the OR Sections and branches of the military and research establishments who dealt with similar operational problems, and to consider the need for new operational research groups in the field, beginning with sending John Kendrew to Egypt to establish an OR presence in the Middle East. The committee also spawned subcommittees dedicated to coordination of effort between Commands on specific problems, such as technical aides to night bomber defense. Minutes of all OR Committee meetings can be found in TNA: PRO AIR 20/6172. The committee met again at similar intervals from January 1945 to January 1946 primarily to consider the problem of demobilization of the wartime sections and the conversion of OR Sections to peacetime duties.

could not be responsible for dealing with the "domestic relations of the various authorities" among the scientists, and suggested that if he was dissatisfied he should take the matter up with them.[103] Thus Watson Watt's scheme to keep Blackett off his territory and advance his own career backfired spectacularly, and thus ended his association with OR. Years later, in his war memoirs, he was gracious about it, giving Blackett all due credit, but could not help pointing out that it was he who had been responsible for coining the terms "operational research" and "Operational Research Section."[104] One might feel some sympathy for the gregarious Scottish engineer if he had not been so brazenly careerist in the matter. And, as we shall see, he was not yet finished.

Administrative Virtue, OR, and the Proliferation of Advisers, 1941-1945

At this point it is wise to pause from our stories of bureaucratic wrangling, and to return to the question of why OR came to have the significance it did in postwar memories of science and the war. Because OR's first historians were all Tizard's allies, it is perhaps not so strange that the statistical groups that Tizard's nemesis, Lord Cherwell, had created in the Admiralty as early as 1939, and later in the War Cabinet and Prime Minister's office, have never been incorporated into the history of OR.[105] Yet, one might well ask why they should not be. If the great revelation of 1941 for OR was that it should primarily aid military commanders, then surely the statistical sections' efforts to

[103] See note from Watson Watt to DCAS, 10/3/1941; and Bottomley to Watson Watt, 10/6/1941; TNA: PRO AIR 2/5352.

[104] Robert Watson-Watt, *Three Steps to Victory*, p. 203. While we can understand Watson-Watt's claims to refer to the events of 1941, Maurice Kirby, for what it is worth, has pointed out that most memoirs seem to point to A. P. Rowe as the coiner of the term; see Maurice Kirby, *Operational Research in War and Peace*, p. 76.

[105] See chapter one. There is a minor exception to this claim. In the Air Ministry's official history of OR, Cherwell's sections received one sentence, Air Ministry, *The Origins and Development of Operational Research in the Royal Air Force* (London: HMSO, 1963), p. xx.

collect, organize and present to authorities the vital information they needed to make proper decisions should be considered a vital precursor. Furthermore, when David Butt recommended that RAF Bomber Command set up a statistical section, the Command acknowledged the need for such "operational research"—they saw no clear difference. This history would even have parallels in the U.S. Air Force's Statistical Section and has frequently been associated with OR. On the other hand, given what we know about the way the military analyzed and improved its doctrines, and the fact that Tizard himself seemed to feel that OR was a "normal function of a well organized force," one might well ask whether any of the scientists in this chapter were simply scrambling to claim a piece of a concept that was not of their invention. Are these priority disputes even worth arbitrating? I argue that they are.

The political story we have been following is vital to understanding the significance seen in OR in Britain. Although a statistical report might, in many ways, look like an OR report, its construction was considered to be quite different. As Blackett later declared, "Collection of statistics for the sake of statistics is no more operational research than collecting beetles is biology."[106] Bernal's story about the origins of the Butt report bears reexamining. Bernal brought the newly-ennobled Cherwell unrecognized into Bomber Command in Bernal's own "dingy vehicle" and proceeded quickly, with no reception, to the "lowly" photograph room. Only through the Bernal's populism, was Cherwell able to get beyond the official statistics to where real research into operations can take place. Watson Watt was making a similar point when he conjured the image of his operational research officers doing "much vertical chasing

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[106] P.M.S. Blackett, "Operational Research," *Operational Research Quarterly* **1** (1950): 3-6, reprinted in *Studies of War*, 199-204, on p. 201.

through horizontal organisations" looking for "facts". OR was not simply a matter of collating and reporting statistics, it was, as I have argued in previous chapters, directly built on the epistemology of military planning. Whereas Butt's report had been limited by his lack of "skill in interpreting photographs and technical knowledge of bomb damage," it was precisely the duty of an OR scientist to find the people with these skills and to meld their expertise into a comprehensive and *nuanced* view of an operational problem. Note how when Butt submitted his report, military officers were, themselves, able to add important context to his statistics on account of their knowledge of meteorological conditions, why photography was used, and access to the Ministry of Home Security's survey of German efforts.

Bringing the views of various kinds of experts together to make military knowledge evolve more quickly into more sophisticated forms was a phenomenon explicitly linked in the minds of many scientists with Tizard himself. As a particularly clear example, take the statement Blackett made in a speech to the Association of Scientific Workers in 1947:

> One of the main origins I think of the brilliance and soundness of his own judgment on matters of warfare, and also of the immense esteem and respect with which Sir Henry Tizard was held by so many scientists and serving officers, was that he made it a policy to seek out the young men, in the establishments and in the squadrons, who were actually doing the real jobs. Few, for instance, who attended it, will forget a Conference in London, the day after a heavy blitz, where he assembled fighter pilots and Air Marshals, radar designers and administrators to thrash the problem of the defence of London.... Tizard had uniquely the quality of knowing what he could not know himself; and of going to the man on the job to find out.[107]

Here, succinctly, was Tizard's reputation: a dynamic, personable individual whose powers were in marshalling the skills and knowledge of others, regardless of whether or not they were scientists. There are direct conceptual links with OR. When Blackett, in

[107] P.M.S. Blackett, Presidential Address, 5/24/1947, *PMSB*, PB/5/1/4/19 (formerly E.22).

the fall of 1941, prepared an influential memorandum entitled "Scientists at the Operational Level," it included the revealing passage:

> The atmosphere required [of an OR Section] is that of a first class pure scientific research institution, and the calibre of the personnel should match this. All members of an O.R.S. should spend part of their time at operational stations in close touch with the flying personnel, and where possible should occasionally go on operational or training flights.[108]

Note the Tizardian administrative themes juxtaposed effortlessly with the requirements of "first class" scientific work, which would necessarily seek out the context of facts in order to understand their appropriate significance. As I have argued in more depth elsewhere, Blackett, as a cloud chamber physicist, had envisioned himself as a "Jack-of-All-Trades" who had to understand the diverse ways photographs of his finicky apparatus might present and misrepresent data. So, too, with the operational researcher, who had to understand the ways the various kinds of military expertise were integrated into a body of military knowledge might present and misrepresent what was going on in battle.[109] Epistemological and administrative virtue had melded in operational research.

However, OR also began to invoke Tizardian virtue in a way more akin to Watson Watt's original vision for his OROs. While the primary responsibility of OR sections was to local military commanders, they also provided higher level scientific advisers with an insider knowledge of military operations that, on account of the epistemological-administrative virtues of OR, competed with that possessed by the officers who directly oversaw them. Recall, though, that Tizard, at least initially, felt that technical advisers had no role in OR. Blackett felt differently. He believed that advisers (such as himself) who sat in on high level meetings were, on account of their scientific training, able to

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[108] P.M.S. Blackett, "Scientists at the Operational Level," reprinted in *Studies of War*, 171-176, on p. 175.
[109] See William Thomas, "The Heuristics of War: Scientific Method and the Founders of Operations Research," *BJHS* **40**/2 (2007): 1-24; the "jack-of-all-trades" quote is from P.M.S. Blackett, "The Craft of Experimental Physics," in *University Studies: Cambridge 1933*, edited by Harold Wright (London: Ivor Nicolson and Watson, 1933), pp. 67-96, on p. 67.

detect flaws or weaknesses in the body of knowledge backing military policies and then initiate research to clarify the issue. In line with their close knowledge of operational issues, advisers were also able to offer these officers better advice about technology implementation and development. While Blackett clearly perceived his work for the Commander-in-Chief of Coastal Command this way, this perception must have grown still deeper with his next and final move in the military.[110]

As America entered the war in December 1941, Blackett decamped to the Admiralty and assumed the curious hybrid title of Chief Adviser on Operational Research (CAOR).[111] The Admiralty's Director of Establishments (D of E) wrote that in bringing Blackett in, it would be necessary to create a definition of OR lest some confusion arise. "It might, for example, be taken to mean research into the conduct of operations," he warned with no hint of irony. Instead, the Admiralty defined OR as "the scientific investigation, in the light of results of actual operations, of the performance of existing weapons, and of the lines upon which they should develop."[112] The position itself reported to the Assistant Chief of Naval Staff for Weapons and the Controller, in their respective spheres of authority. While certainly a step back for Blackett, who was used to advising on operations, it did, at least, explicitly connect him with Admiralty R&D policy.

Compared to the rapidly expanding OR Group at the ADRDE, or the Fighter Command ORS, which now had fifty members, Blackett's group was extremely small.

[110] On the selection of problems by OR staff instead of by military authorities, see Blackett, "Operational Research," pp. 201-202. For Blackett's discussion of the role of OR in formulating operational requirements at Coastal Command, see "Scientists at the Operational Level," p. 174.
[111] Blackett's move finally brought about the end of his relationship with the Ministry of Aircraft Production.
[112] Minute from D of E, 12/14/1941; and Office Acquaint for Operational Research Staff from Henry Markham, 12/18/1941; TNA: PRO ADM 1/20113.

At first, it consisted solely of himself and Sir Ralph Fowler, an eminent but older physicist who was seriously ailing and only able to work part-time. They quickly acquired a few junior staff members from the Admiralty's own R&D branches, and ultimately brought on geophysicist Edward C. Bullard, and E. J. Williams, another physicist who had previously taken over Blackett's position at Coastal Command.[113] As Blackett expanded his staff, he also became a *de facto* scientific adviser to the Admiralty. Unsurprisingly, he soon pushed his field of inquiry beyond weapons effectiveness to tactics.[114] In October 1943, he requested that he report to the Vice Chief of Naval Staff (VCNS) through the topic-appropriate Assistant Chiefs in light of the facts that "the work of C.A.O.R. is developing more in the direction of operations than of weapons," and that the Admiralty had appointed Charles Goodeve, a Canadian academic chemist and scientist at *H.M.S. Vernon* mine warfare research establishment, to the new post of Assistant Controller for Research and Development, which made Blackett's connection to the Controller less necessary.[115] This request was granted.

It is unlikely, though, that the move disrupted the link between OR and R&D policy that had developed in the Admiralty. In fact, given Blackett's strained relationship with the Admiralty's Director of Scientific Research, it probably enhanced it. Although records of Goodeve's work as Assistant Controller are scarce, the simple fact that he became postwar Britain's top OR proponent should indicate that he had made extensive

[113] See D of E to T. Padmore, 3/11/1942, TNA: PRO ADM 1/20113. On the primacy of Bullard and Williams within the CAOR office, see Minute from Head of C. E. Branch 1, 5/15/1944, TNA: PRO ADM 1/20113.

[114] As Blackett noted in September 1942 (clearly espousing the oppositional view already) "the O.R.S. should attempt to infiltrate tactfully, using this and other problems as a foothold, into the more general study of tactics. One measure of success of an O.R.S. [is] the influence it could bring to bear on tactics." See minutes of the 7th informal meeting of "independent scientific advisers", 9/8/1942, *HTT* 298.

[115] Minute from CAOR, 10/5/1943, TNA: PRO ADM 1/20113.

use of Blackett's group's studies in his wartime post.[116] Meanwhile, by January 1944, it was apparent, according to the D of E, that "Operational Research has come to stay," and that there was "general agreement that the time has come to give the group enhanced status." That spring Blackett's office became the Naval Operational Research Directorate (NORD), a major promotion within the Admiralty bureaucracy, and Blackett became Director of Naval Operational Research (DNOR), still reporting to the VCNS.[117]

As we saw in chapter two, even as Blackett was getting settled at the Admiralty in early 1942, the War Office had come to the conclusion that they needed (according to one account "would tolerate"[118]) an office headed by a scientific adviser to handle long-term R&D planning. The Army consulted Henry Tizard who quickly informed them that the Ministry of Supply was best equipped to handle R&D problems, and attempted to redirect their attention to the need for the study of the effectiveness of existing equipment and suggested that their new adviser should have a close connection with their OR groups. By this time, the ADRDE OR Group had expanded rapidly, but it was still under the Ministry of Supply and dealt mainly with technical issues in the field. Tizard recommended that either that group's leader, Colonel Basil Schonland, a South African physicist, or John Cockroft, who was still Superintendent of the ADRDE, would be best

[116] See also Charles Goodeve, "Operational Research," *Nature* **161** (1948): 377-384. I base my description of Blackett's relationship with Admiralty R & D establishments on a letter from Blackett to Goodeve, 1/25/1943, TNA: PRO ADM 219/630. Responding to the Admiralty Director of Scientific Research's comments on an unknown docket, Blackett stated: "This strikes me as being a typical example of offensive non-cooperativeness of the sort I was discussing with you the other day. I am not supporting in any way the suggested review of Government Research programmes, but I can well believe that such a review might in fact be very valuable. D.S.R's attitude seems to me deplorable."

[117] Minute from D of E, 1/10/1944; Memorandum from CAOR to D of E, 4/20/1944; Minute from D. of E. 5/30/1944; TNA: PRO ADM 1/20113. The term "Naval" was used in the title to distinguish the DNOR from the Air Ministry's Director of Operational Requirements, with whom Blackett had frequent contact.

[118] Kenneth Hutchison, J. A. Gray, Harrie Massey, "Charles Drummond Ellis," *Biographical Memoirs of Fellows of the Royal Society* **27** (1981): 199-233, p. 215.

for the position.[119] Ultimately, though, the position that developed resembled Blackett's position at the Admiralty, and by the end of April Charles Darwin, the Director of the National Physical Laboratory, was appointed Scientific Adviser to the Army Council (SA/AC), and another well-respected physicist and Cavendish alumnus, Charles Ellis, was made his assistant shortly thereafter. Within months it became clear that Darwin could not balance his War Office duties with his duties at the NPL and Ellis was promoted to take his place in 1943.[120]

Figure 6.1. Charles Ellis, Patrick Blackett, and John Cockroft from a group photograph of Cavendish Laboratory research students in 1932. Source: Kenneth Hutchison, J. A. Gray, Harrie Massey, "Charles Drummond Ellis," *Biographical Memoirs of Fellows of the Royal Society* **27** (1981): 199-233. ©Cavendish Laboratory. Reproduced with Permission.

After the establishment of the SA/AC post, OR began to expand rapidly within the army into problems outside anti-aircraft defenses and radar applications. All OR studies on field operations, such as in Montgomery's No. 21 Army Group, and in groups in Asia and Australia, were performed largely by commissioned personnel under the auspices of the War Office, and they had a direct connection to the SA/AC; but Britain-based OR remained under the Ministry of Supply. This strange split structure did not prevent the SA/AC from establishing effective liaison with both sides of army OR. In

[119] Tizard memorandum, 2/25/1942, TNA: PRO WO 32/10330. Schonland would go on to become Scientific Adviser to Field Marshal Bernard Montgomery with the 21st Army Group, who also had their own OR personnel. See B. F. J. Schonland, "On Being a Scientific Adviser to a Commander-in-Chief," 2/1/1951, copy available in *ELB*, Box 44, Folder 7. See also Brian Austin, *Schonland: Scientist and Soldier* (Philadelphia: Institute of Physics Publishing, 2001).
[120] On the appointment of Darwin and the succession of Ellis see materials in TNA: PRO WO 32/10330.

fact, the work of all the AORGs provided SA/AC with the information he needed to perform his duties, which were "to suggest or examine from the scientific standpoint weapons or methods of waging war", to advise on development prioritization, and "to maintain close contact with research and development in the Ministry of Supply and to convey the views of the War Office regarding priority in research and development projects."[121]

So by the summer of 1942 a recognizable infrastructure of scientific advice had developed spanning all three services and with close connections to both OR and the research establishments. In June, Henry Tizard began calling a series of informal meetings at his office in the Ministry of Aircraft Production bringing the heads of the various scientific advisory organs together. These meetings were clearly gatherings of scientific allies. Darwin, Ellis, or both, attended every meeting. Blackett or Fowler attended frequently on behalf of the CAOR office. J. D. Bernal attended at first on behalf of the Ministry of Home Security, and later as a scientific adviser to the Chief of Combined Operations, Lord Mountbatten. MAP was well-represented, but research and development establishments from the Admiralty and the Ministry of Supply were not, except by John Cockroft, who attended less than half the meetings on behalf of the ADRDE. The meetings took place sporadically until January 1943, and typically discussed any question relating scientific effort to the progress of the war. So, pertinent questions of technological development came up, but operational research was especially high on the agenda. The first meeting, for instance, raised the question of diverting

[121] There are several good unpublished histories of the importance of the relationship between Army OR work and the SA/AC post. See "Reconstitution of the Operational Research Group, Ministry of Supply," 1/26/1943, TNA: PRO WO 291/1286; "The Department of the Scientific Adviser to the Army Council" 9/11/1946, TNA: PRO WO 291/1300 (which contains the terms of reference); "Operational Research in the British Army, 1939-1945," October 1947, WO TNA: PRO 291/1301.

personnel from the research establishments to OR sections, since they at that time saw it as crucial to raise the effectiveness of current weapons rather than push the production of new ones. These informal meetings were ultimately of minimal importance, but they represent a clear culmination of the new structure of scientific advice organs.[122]

In July 1943, around the time of his retirement to the Presidency of Magdalen College, Oxford, Tizard brought the OR-scientific adviser network full circle, recommending to the Chief of Air Staff that the Air Ministry establish a high level "full time operational research adviser" as a counterpart to the position of Blackett at the Admiralty and Ellis at the War Office, and essentially as an opposite number for MAP's Director of Scientific Research (now the up-and-coming civil servant Ben Lockspeiser, who had just replaced David Pye) in order to oversee operational research relating to overall air operations. He recommended that they obtain Bernal or his frequent partner the zoologist Solly Zuckerman, but, ultimately, Sir George Thomson, a professor of physics at Imperial College who had chaired the MAUD Committee, was brought in under the title of Scientific Adviser to the Air Ministry (SAAM), and had similar duties to Ellis in the War Office.[123] Wing Commander Menzies and his OR Centre, which became the Deputy Directorate of Science in 1944, were placed at his service.[124]

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[122] Minutes from these meetings can be found in *HTT* 298. During these meetings proposals were floated for the establishment of an OR circle or club, as well as a journal. These suggestions, of course, did not develop until after the war.

[123] See Memorandum from Tizard to Chief of Air Staff Air Chief Marshal Sir Charles Portal, 7/15/1943, TNA: PRO AIR 20/3411; Letter from Tizard to VCAS, 8/7/1943, TNA: PRO AIR 20/3145. In fact, the creation of the SAAM post caused significant administrative disruption after Tizard's retirement, as Lockspeiser insisted that all scientific advice should come from the Ministry of Aircraft Production. The Air Ministry, however, reserved its right to employ its own advisers. See materials in TNA: PRO AIR 30/3411, especially AUS(A) to VCAS, 11/10/1943, "Relations with M.A.P. on Questions of Operational Research and Scientific Advice".

[124] By the time Menzies became Deputy Director of Science (DDSc), he had been promoted to Group Captain. The DDSc was also considered to be the Deputy SAAM. See "Deputy Director of Science" memorandum, n.d. [1944], TNA: PRO AIR 30/2411.

Thomson was not anxious to stay too long with the Air Ministry, and as it became clear the war in Europe would be won soon, he returned to academic life in December 1944, leaving the SAAM position vacant. To fill the vacancy, attention turned to none other than Sir Robert Watson-Watt (as he was now called), still the Scientific Adviser on Telecommunications, whom Air Ministry officials were concerned would be unwilling to leave MAP. They need not have worried. Watson-Watt jumped at the opportunity, and was anxious to take on SAAM responsibilities *in addition* to some of his various responsibilities in telecommunications. In a January 1945 interview with Sir Archibald Sinclair, still the Secretary of State for Air, Watson-Watt "attached particular importance to the improvement of our O.R.S. system, and he thought that it had not been working smoothly in recent months." According to Sinclair, he was "not only willing but anxious to grapple with operational research." Ultimately, though, the Air Ministry grew uneasy with Watson-Watt's preoccupation with maintaining his status as a telecommunications expert and decided not to give him the position.[125] The post remained vacant until after the war ended when H. R. Hulme, who had just taken over the DNOR position from Blackett (who had returned to academics), was brought on.[126]

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[125] On Thomson's departure see AUS(A) to VCAS, 10/19/1944, TNA: PRO AIR 30/3411. Watson-Watt also made repeated demands for status. In his interview with Sinclair he asked for a position on the Air Council, which Tizard, unique among scientific advisers, had held while he was at MAP. He also asked to be "under VCAS working in close connection with DCAS" and for an office on the second floor of the Air Ministry building on King Charles Street, which would give him somewhat greater status than an ACAS. These demands made the Air Ministry uncomfortable. See especially memorandum from AHMS to PUS and VCAS, 1/13/1945, TNA: PRO AIR 20/3145; minute from D.C.S. Evill [VCAS] to Secretary of State, 3/13/1945, TNA: PRO AIR 30/3411; and memorandum from AUS(A) to PUS, 4/18/1945, TNA: PRO AIR 30/3411.

[126] Hulme was also a mathematical physicist who had been Chief Assistant at the Royal Observatory before the war. He "took charge of degaussing" at the Admiralty (magnetized ships would set off magnetic mines), and became Blackett's assistant in 1944. When he took over as SAAM, his duties included control over "operational, administrative and training research". Of files in the folder, see especially PAUS(G) to VCAS, 12/11/1945, TNA: PRO AIR 20/3145.

With the introduction of the Scientific Adviser to the Air Ministry position in 1943 channels for scientific advice on operations and operational requirements were established within all three military services. These channels remained until the 1964 reorganization of the Ministry of Defence when new one were created in their place. One might well ask whether the bureaucratic posts pushed by Tizard effectively eliminated the need for an *independent* scientific adviser such as himself.[127] His 1943 departure is certainly not generally seen this way. Instead, he is usually said to have left on bad terms owing to his loss of influence. The fact that Cherwell remained as Paymaster-General in the War Cabinet until Churchill's defeat in the general election of 1945 has only served to underscore this interpretation, as has a now legendary incident in the history of scientific advice in World War II, the debate between Tizard and Cherwell over the efficacy of area bombing.

In terms of the history of OR, though, that debate, reviewed extensively in many other works, is remarkable only for how remote it was from the OR-adviser network.[128] In fact, Blackett (who also took part in it) later lamented the lack of OR involvement, and brandished the results of the United States Strategic Bombing Survey as a sort of *post hoc* vindication of the lack of impact area bombing had had on the economy and morale of

[127] As Scientific Adviser on Telecommunications, Watson-Watt certainly far outstayed his usefulness. Already by 1945, though he was still too occupied with various telecommunications duties to take the SAAM post, he was rarely consulted by the Air Ministry, but he officially lingered there until 1950. See minutes in file TNA: PRO AIR 2/9688.

[128] Note David Edgerton's false claim, "The Tizard/Lindemann dispute was in effect a dispute involving 'operational research'…", Edgerton, "British Scientific Intellectuals," p. 21. The consequences of the oppositional view is most apparent when even its chief opponent confuses OR with more general scientific advice. He repeats the mistake in *Warfare State*, p. 204, when he claims that C. P. Snow's *Science and Government* was "concerned with operational research". Snow barely makes any mention of OR throughout the book.

Germany.[129] Most of the debate over Britain's bombing strategy centered around issues *other* than the perceived effectiveness of German bombing in England, and yet the Ministry of Home Security survey on this topic was the only study Tizard and Blackett ever actually marshaled to their defense. When they (sensibly) argued on other points under consideration, such as whether England should be on the offense or the defense, or how much destructive potential British industry could turn out, their outsider arguments were barely distinguishable from other insider military voices. This fact simply underlines how important OR had actually been in bringing scientific advisers inside the military heuristic process and making them effective and useful once they arrived.[130]

Part II

How might one best read the history presented in the first part of this chapter? In the past, it has not been read as a "domestic affair" between scientists, to use the Deputy Chief of Air Staff's term. It is read as a story about the path science itself carved in government. But whose notion of science? David Edgerton is quite right to observe that research establishment scientists have been largely ignored.[131] The standard line from the bureaucratic wranglers in Part I is that these government scientists were good at what they did, but that the value of their work was limited due to a lack of coordination. The

[129] See P.M.S. Blackett, "Recollections of Problems Studied," reprinted in *Studies of War*, 205-234, pp. 223-228; and especially Blackett, "Tizard and the Science of War," pp. 109-111.
[130] There are many accounts of the area bombing debate. See especially Crook, "Science and War" for an overview. Also see Kirby, *Operational Research in War and Peace*, chapter 5 on OR in Bomber Command, much of which is taken up by the strategic question, which, as Kirby points out, was not within the Bomber Command ORS' duties to consider. Even still, it is an indication of the strange weight the question has had on the historiography of OR. Primary materials relating to the debates are available in both the Tizard and Blackett papers, and at the Cherwell papers in Oxford, which I have not seen.
[131] Edgerton, *Warfare State*.

fault for this lack of coordination was often attributed to a government administrative infrastructure incapable of dealing effectively with technical issues.[132] Many government scientists, particularly in the Air Ministry, were receptive to the need to augment existing means of coordination, while others, particularly in the Admiralty, took these observations as ill-informed criticisms of their existing means of organization, and felt the burden was on the reformers to demonstrate why their proposals might prove superior. Outside scientists ultimately came to paint such resistance as a science versus military issue, rather than as an outsider versus insider issue among scientists.

Sir Henry Tizard, for the most part, stood outside of this oppositional discourse. His own bureaucratic reforms emphasized communication: between different laboratories, between laboratories and procurement officers, between officers and the field, and, finally, following Blackett's notion of OR, between scientific advisers and the field. If his schemes involved bringing scientists into decision making apparatuses, it was as much for their own benefit as for the benefit of policymakers.[133] He did not shrink from

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[132] In 1942, especially, praising the scientists, but damning their coordination had become a key facet of the political discourse of science. See for instance, Henry Tizard, Luncheon talk to the Parliamentary and Scientific Committee lobbying group [an organization that still exists], 2/3/1942, *HTT* 572 wherein he praised Britain's "tactical strength of science" but expressed concern about the "strategy of science"; J. D. Bernal, "Projected Pamphlet, Science and the War Effort," unpublished, available in *JDB*, B.4.48, n.d., [likely 1942], quoted Tizard's talk and observed that "whereas in detail most of the [scientific war] work is excellent, carried out quickly and to the point, there is no way of ensuring that the relative importance of different researches is kept constantly in view"; In A. V. Hill's letter to the *Times*, 7/1/1942, he claimed "The inferiority [of British tanks] is due not to bad workmanship, but to a system which has failed to anticipate future tactical requirements in guns, projectiles, armour, and performance, failed to collect, analyse, and profit by previous operational experience, failed sometimes even to obey the elementary rule that production must follow, not precede, development." On 3/15/1960, replying to a Navy researcher who felt he had disparaged Naval research in his Tizard memorial lecture, Blackett wrote, "I am sorry I implied any disrespect for the scientific attitude of the Navy—this was not my intention. However, I think it is generally agreed that until the challenge of the immediate pre-war period none of the Services really reacted fast enough to the technical changes in the methods of warfare." *PMSB*, PB/8/45/3/3 (formerly H.87).

[133] From later in Tizard's talk to the Parliamentary and Scientific Committee in the previous footnote: "The safest way of reaching the right decision [on the strategy of science] is to have scientists working side by side and in the closest collaboration with those who have the administrative and executive responsibility.

allowing the existing administrative infrastructure of military officers and advisory committees to continue as the primary policymakers, even on issues relating to science and technology, so long as these officers and committees were privy to the information and advice they needed to do their job well. However, as I began and ended the first part of this chapter by arguing, the eventual marginalization of Tizard as an independent adviser has never been interpreted as a sign of the military's policymaking infrastructure becoming increasingly secure in its own footing, partially thanks to Tizard's own efforts in improving the military's heuristic apparatus, not least with the establishment of OR. On the contrary, every development in the science-state relationship during the war, and, as we will see, after the war, continued to be measured by a yardstick monitoring the progress of science against the recalcitrant administrators. The task of this second part of this chapter is to survey the origins and consequences this oppositional view has had on interpretation of the postwar debate surrounding science and its relationship to society in Britain.

Sources of the Oppositional View

To a certain extent the oppositional view seems to have been ingrained in British academic scientists' consciousnesses for some time prior to the war. Individualist academics were sometimes seen—and so, they often saw themselves as being seen—as related to crank inventors, suspicious characters whose ideas were sure to prove too impractical to be of much use. Figure 6.2 is an extract from an uncompleted account written by A. V. Hill in 1918 detailing his experiences in the Anti-Aircraft Experimental

And the first thing that the scientist learns when he has the benefit and privilege of such collaboration is that he has a lot to learn."

Section during the just-completed war (as discussed in Chapter Two). The amount of self-deprecation crowded into this small excerpt is remarkable. Hill describes his own penchant for "inventing things" first as an "undesirable trait" and then, crossing that out, as an "unpleasant habit". Never mind that he was to be employed by the Munitions *Inventions* Department; only Horace Darwin (the son of a certified national scientific icon) could be depended on to introduce a character exhibiting such a "vice" into the government machinery. In a passage added in and then crossed out, Hill describes his previous work in the physiology of muscles as the work of an eccentric. His innovative work in mathematics, physiology and "to some extent" in physics, made him a "physiomatheologist," or, in his own words, a "Jack of All Trades & Master of None", but this odd mixture of skills, in the end, "proved useful in the work."[134] These same skills had already

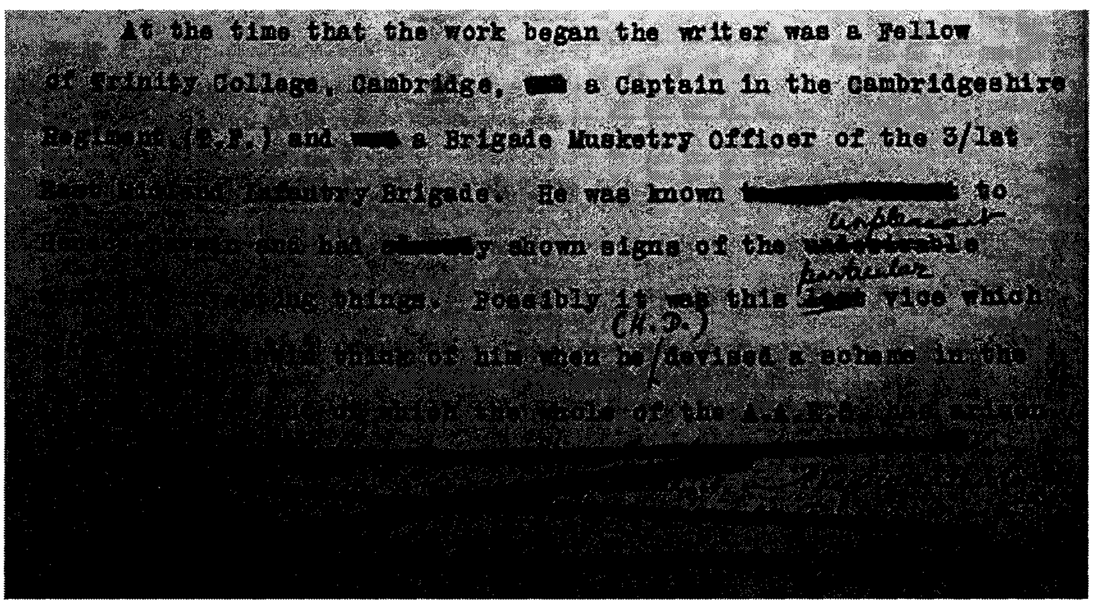
At the time that the work began the writer was a Fellow
of Trinity College, Cambridge, [redacted] a Captain in the Cambridgeshire
Regiment (T.F.) and [redacted] a Brigade Musketry Officer of the 3/1st
[illegible] Infantry Brigade. He was known [redacted] to
unpleasant
[illegible] and had [redacted]y shown signs of the [redacted]
particular
[illegible] things. Possibly it was this [redacted] vice which
(H.D.)
[illegible] think of him when he devised a scheme in the
[illegible] the whole of the A.A.E.S. [illegible]

Figure 6.2. An extract from A. V. Hill's uncompleted 1918 memoir of his work in the First World War. Source: "The Anti-Aircraft Experimental Section (1916-1918)" from *AVHL* I 1/37.

134 A. V. Hill, "The Anti-Aircraft Experimental Section (1916-1918)," *AVHL* I 1/37.

won him a fellowship at Trinity College, Cambridge, and would soon come to be recognized as laying important foundations for biophysics and would win him the1922 Nobel Prize in Medicine or Physiology.

Certainly Hill, in typical British fashion, was playing the wry humorist by discussing his strong multidisciplinary expertise in a tone that implicitly critiqued the government establishment's valuation of his particular talents. A man in his upwardly mobile position could afford a little self-deprecation. Still, as with so many jokes, one might legitimately ask if it did not reflect on his own position as a scientist working for the services. In so asking, though, it is important to contrast Hill's position as an academic outsider to his position as a scientist. After all, the military services were undeniably technical organizations, and as such were beset by well-meaning but unhelpful suggestions from outsiders. In fact, Hill was a member of the committee tasked with reviewing suggestions for anti-aircraft related inventions, which were almost invariably dismissed as not worthwhile.[135] That Hill and the other scientists in the AAES had to prove their own worth to a skeptical military is entirely plausible, but the point should not be overburdened. Anti-aircraft technology was, itself, both quite novel and unreliable at that time. Significantly, though, it *was* an accepted military specialty. It would not be surprising if the military harbored some doubts that outsiders could improve upon it, and it should not reflect on the military's attitude toward science and invention in general.

A more direct and rather more satisfying explanation of the prevalence of the oppositional view in World War II and after can be found in the rhetoric employed by a

[135] See for instance, Munitions Inventions Department, Gunsights and Rangefinders Committee, Meeting No. 40 minutes, 12/11/1918, *AVHL* I 1/24.

loose affiliation of concerned scientists who comprised what has been called the social relations of science movement of the 1930s and '40s. This movement w as the product of a fairly select group of scientific elites who spoke and wrote primarily under the aegis of the British Association for the Advancement of Science (BAAS; and particularly its Division for the Social and International Relations of Science, which was established in 1938), and the leftist scientists' union, the Association of Scientific Workers (AScW).[136] Movement members were concerned about the way science and technology were employed relative to national and international needs, and about the self-imposed isolation of academic scientists from questions of political concern. Although the movement featured prominent communists and socialists, including many featured in this chapter such as J. D. Bernal, Patrick Blackett, and C. H. Waddington; it was also supported by the more centrist journal *Nature* and its editor Sir Richard Gregory, and conservatively-identified scientists, such as Hill (who repeatedly attacked the leftists' claims that science belonged naturally on the side of socialism). This movement, however, was not representative of the general population of scientists. Indeed, movement scientists frequently complained that scientists who worked in government and industry were blindly pursuing whatever goals their employers set before them. The government machine itself was allegedly ignorant of the effect science and technology had on popular welfare. This group's media presence was dedicated entirely toward goading scientists to invest themselves in social problems, and educating policymakers about the potential of science. The oppositional view was thus a constituent element of its identity.

[136] McGucken, *Scientists, Society and State* is the key source on the movement.

It so happened that many of those heavily involved with the movement had a significant hand in the development of operational research in Britain during the war. The socialist Patrick Blackett, as we have seen, was a driver in the creation of three OR groups. In writing "Scientists at the Operational Level," and the later "Methodology of Operational Research," he became the most identifiable face of OR.[137] The communist J. D. Bernal and political glad-hander Solly Zuckerman (who joined the movement quite late by resuscitating his Tots and Quots dining club) are also commonly linked with OR. Their joint work for the Ministry of Home Security was at least qualitatively similar to a great deal of OR, and both served as advisers to Lord Mountbatten; but the importance of their relationship to OR has certainly been magnified, probably on account of their prolific publication.[138]

It is not completely inappropriate to see the events in light of the movement—Bernal and Blackett clearly saw their part in the war against fascism as an extension of their respective communist and socialist politics.[139] However, as demonstrated in Part I of this chapter, the significance of their part was to be found more in their mastery of an insider's game than in any use they may have made of the military as a test case for their program of interrelating science and society's needs.[140] Wartime publications by elite

[137] Both documents are reprinted in Blackett, *Studies of War*, pp. 171-198.

[138] For one brief description of the relationship of their work in the Ministry of Home Security in the context OR, see Air Ministry, *Origins and Development*, pp. 41-42; and Kirby, *Operational Research in War and Peace*, pp. 126-127. Probably the best available descriptions of the work of Bernal and Zuckerman during the war are in Zuckerman, *From Apes to Warlords*; and Andrew Brown, *J. D. Bernal: The Sage of Science* (Oxford: Oxford University Press, 2005).

[139] Their popular lectures during and just after the war attest to this point. See J. D. Bernal, "The Function of the Scientist in Government Policy and Administration," *The Advancement of Science* **2** (1942): 14-17; or Blackett, "The Planning of Science – Series in the Chinese Service" draft [1946], PB/8/7 (formerly H.15).

[140] Others, without political motivations, such as the industrial scientist Harry Larnder, or the academic physicist Basil Schonland, adapted themselves similarly. According to Maurice Wilkes, who served with Schonland in the AORG during the war, Schonland "gave vent to a certain mistrust of eminent scientists who made pronouncements on subjects outside their scientific specialties. He used to call them 'public

scientists, such as in *Nature* or the BAAS' *The Advancement of Science* and particularly the 1940 Penguin special *Science in War*, were seen by politicians and civil servants (including scientists) as ill-informed and irrelevant.[141] These outsider scientists were to be pacified, not heeded. Yet these same politicians and civil servants were usually sympathetic to scientists' plans to tweak the government's scientific infrastructure to meet the demands of the war. Even ambitious plans of questionable practicality, such as those of the Royal Society and Watson Watt's May 1941 scheme, were taken seriously in many high quarters. In the military, high officers were almost invariably keen to take advantage of laboratory-trained scientists in assisting their own teams of specialists in assembling tactics and plans. Although it is doubtless true that scientists were met with resistance in some quarters (probably for the most part local units not used to dealing regularly with outsiders), the oppositional view certainly overstates its case. We will now examine three instances of how the creation of the oppositional view has done a disservice to historiography: first, in the relationship between OR and the scientific left; second, in the reception of two postwar committees run by Tizard; and third, in Blackett's postwar relationship with OR. We will then conclude with an examination of alternative views of OR, science and society that have flown under the proverbial radar.

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scientists' and I detected a slightly malicious pleasure in the way he said it", Maurice V. Wilkes, "Foreword" in *Schonland: Scientist and Soldier*, by Brian Austin (Philadelphia: Institute of Physics Publishing, 2001). Although Tizard occasionally spoke publicly, his inclusion in this story as anything but an experienced insider is suspect; likewise Watson Watt (at least in the prewar years). Yet Rau, "Technological Systems," p. 226-227, includes them in the social relations of science movement. This tendency can be traced to the loose definition of the movement given in McGucken, *Scientists, Society and State*, which seems to include any bureaucratic reformers with a BAAS membership in the movement's numbers.

[141] See for instance *The Advancement of Science* **2** (1942): 4-24, dedicated to problems of "science and government"; *Science in War* (Harmondsworth: Penguin, 1940).

Operational Research and the Scientific Left

Our knowledge of the relationship between operational research and the British scientific left suffers from having been the subject of many passing observations, but few in depth examinations. Usually, OR has been seen as suffering in the postwar years for its association with the radical left. Most notably, the OR practitioner Jonathan Rosenhead has directed historical attention to the abortive efforts of the communist biologist Cecil Gordon, the force behind Coastal Command's Planned Flying and Planned Maintenance scheme, to instill OR throughout postwar civil government. Rosenhead sees OR as having stood at a "crossroads" after the war, and its subsequent failure to be explicitly adopted in government offices indicates to him the presence of a "chill wind" of anticommunism that blew against the spread of OR and the ambitions of the scientific left.[142] Philip Mirowski likewise suggests that what he calls "British OR" was destroyed by its affiliation with the left's politics of science planning.[143] Solly Zuckerman confirmed in his memoirs a certain air of suspicion to the left in how his own efforts to offer scientific advice within government departments after the war were sometimes met with suspicions of "socialist meddling", but it is questionable whether OR and leftist politics were really so bound in their fates.[144]

How, after all, was OR supposed to have been at the service of socialism? Its scattered associations with scientific planning and even scientific advice in general may have left the impression that OR had strong connections to Soviet-tinged centralized, bureaucratized planning, an impression that can only have been strengthened by the

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[142] Rosenhead, "Crossroads".

[143] Philip Mirowski, *Machine Dreams: Economics becomes a cyborg science* (New York: Cambridge University Press, 2002), pp. 182-183. He appears to be unaware of Rosenhead.

[144] Solly Zuckerman, *Monkeys, Men and Missiles: An autobiography, 1946-1988* (London: Collins, 1988), p. 98.

subsequent association of OR with optimization techniques such as linear programming, and planning techniques such as cost-benefit analysis and the Planning Programming Budgeting Systems of Robert McNamara's Pentagon. After all, what serves one country's efficiency-optimized military-industrial complex serves another country's socialist Five Year Plan equally well. It is even misleading that Gordon, one of the postwar era's reddest and probably the most ambitious advocate for the extension of OR, was responsible for one of the very few wartime OR projects that bore any sort of resemblance to what would be recognizable as an optimization-driven OR twenty years hence. The left's regard for OR, however, did not, in general, resemble this portrait at all.[145]

The central tenet of the scientific left was that scientists should be politically engaged with the problems of society, and Bernal, Blackett and other radical leftist scientists harshly criticized the *status quo* of academic science culture as a source of a peculiar "frustration of science" that enforced their isolation from the R&D activities that most affected society's welfare.[146] In Bernal's view (much as in Tizard's), there was no linear model of scientific and technological advance. Pure and applied scientific advances were mutually dependent. The need for abstract knowledge arose haphazardly

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145 Although Rosenhead, "Operational Research at the Crossroads" discusses OR as a means of contact with the user and an inspiration for new consumer research, the bulk of the paper seems to relate OR to its later association with methods of economics and optimization, rather than Bernal and Waddington's broader war-inspired vision. In any case, he concludes, OR, whatever its expression, was subjected to the "chill wind" of anticommunism (p. 24). Fortun and Schweber follow along accordingly, "Scientists and the Legacy of World War II," pp. 614-615. The question of economic planning is clearly on Mirowski's mind as well. See *Machine Dreams*, pp. 182-183, and especially the footnotes on p. 183.

146 There are many sources on the frustration of science. Most obviously, see Daniel Hall, J. G. Crowther, et al., *The Frustration of Science* (New York: W. W. Norton, 1935), and especially the chapter by Blackett entitled, "The Frustration of Science." Another young leftist, Bernard Lovell, did not find his own "frustration" alleviated by contact with the world of the Air Ministry research establishments. He derided their parochial research practices, writing in an angry letter to his Manchester mentor Blackett,: "You may remember from my book [*Science and Civilization*, 1939] how keenly I felt the disillusionment and frustration in science. But when even frustration is being frustrated by inefficiency, it is very hard to sit under!" Letter from Lovell to Blackett, 10/14/1939, *HTT* 32.

as practical problems demanded it, regardless of whether the practical problem was of an academic or industrial nature. Therefore, as Bernal wrote in his prewar masterwork *The Social Function of Science*, academic scientists, in their insistence on isolation from practical goals, unnecessarily and destructively treated science as an "amusing pastime," an escape from the problems that their society faced. Science was a "game against the unknown where one wins and no one loses" or a race against other scientists to win a "prize from nature." He observed, "It has all the qualities which make millions of people addicts of the crossword puzzle or the detective story." Without socialist or communist politics, he argued that the advances of academic science would always be exploited to capitalistic ends, which, in turn, sparked a danger of a fascistic and anti-scientific backlash.[147]

The *Social Function of Science* was supposed to chart a way forward for a more socially sensible scientific culture, a "planned" approach to scientific advance, which meant selectively choosing problems to pursue in depth, *as they arose*, according to social goals rather than the deliberately haphazard approach favored by university culture. This call for planned science prompted a long and fruitless debate against a group, led primarily by scientists Michael Polanyi and John Baker, called the Society for Freedom in Science, over whether or not science could be planned at all. For what it was worth, the radical left clearly won this debate given Polanyi and his allies' consistent refusal to engage with what the radicals meant by planning, taking it to mean that fundamental scientific advances could be anticipated and rationally pursued. To Polanyi and his allies, planning seemed similar to the idea of planned society then being carried out in the

[147] See J. D. Bernal, *The Social Function of Science* (London: G. Routledge and Sons, 1939), p. 97. On "Bernalism" see Werskey, *The Visible College*, chapter 6.

Soviet Union. They feared that the despotism of Soviet politics would extend to the scientific world.[148]

For his part, Bernal insisted that planned science entailed nothing of the sort. After the war ended, he was able to point to the effort of wartime research, and the contributions of operational research in particular, as an example of how scientific effort could organize itself around a set of stated goals. While OR had entailed the comparison of research establishments' scientific effort and the use of equipment in the field in light of the operational requirements of the war, in peace, Bernal noted condescendingly in 1945, "[these requirements] are less easy to see but are beginning to be recognized as the removal of want and fear."[149] However, with the exception of Gordon, Bernal's and other leftists' notion of OR *itself* held no special significance for the postwar era—witness Waddington's letter to *Nature* at the beginning of this chapter. It was more of a handy historical reference point for how socially-minded scientists could research social needs and select pressing problems on which to work. For instance, according to the 1947 AScW pamphlet *Science and the Nation*, OR veterans, familiar as they were with the Tizardian act of culling knowledge from the military hierarchy, could prove useful in the field of consumer research.[150] For the scientific left, OR was a rhetorical stand-in for

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[148] On the Society for Freedom in Science, see William McGucken, "On Freedom and Planning in Science: The Society for Freedom in Science, 1940-46" *Minerva* **16** (1978): 42-72; or McGucken, *Scientists, Society and State*, ch. 9, and parts of Werskey, *Visible College*, chapter 8.

[149] See especially J. D. Bernal, "Lessons of the War for Science," originally printed in *Reports in Progress in Physics*, 1945; reprinted in *Proceedings of the Royal Society of London, Series A* **342** (1975): 555-574, quote on p. 560; and the particularly illuminating J. D. Bernal, "Science and the War Effort".

[150] Association of Scientific Workers, *Science and the Nation* (Harmondsworth: Penguin, 1947), Ch. 10. Blackett was president of the AScW when the pamphlet was written, and penned its preface. From 1945 to 1951 the AScW linked OR with consumer research. T. E. Easterfield, who was involved with the AScW in this respect moved to Gordon's Special Research Unit at the Board of Trade, which also took up the question of consumer research. Rosenhead, "Crossroads," pp. 11-13, calls this suggestion for the "potential" of OR "seemingly curious," but it was, as we can see, precisely in line with the leftist notion of the significance of the wartime OR experience. Recall from the previous chapter the American Samuel Wilks' similar observation of the parallel between OR and consumer research.

scientific representation on policy planning committees, for sociological research, for surveys of scientific work, and for better means of organizing and disseminating scientific information. If the radicals' schemes never came into being through the adoption of a grand scheme, it would be difficult not to argue that over the following decades many of the things for which Bernal argued came to pass in some form or another. Radical political transformation, on the other hand, did not prove so necessary, and so Bernalism has been seen as quashed rather than fulfilled.[151]

In reacting to the left's rhetorical deployment of OR, David Edgerton, I believe, extrapolates too far in concentrating on the rhetorical use of OR by scientific intellectuals as a demonstration of novel uses of science in the war, as a means of overstating the wartime contribution of elite leftist academics, and, most damningly, as a means of sanitizing leftist scientists' contribution to the war by discussing OR's contributions to defensive (but not offensive) military planning.[152] By concentrating laboratory scientists' attentions on problems of what we now call technology transfer, the left simply considered OR a vital example of how putting scientists in more direct contact with the problems they were supposed to solve could increase the efficacy of technology. Their attention to it had more to do with finding lessons for the future than with charting the history of science in the war—little wonder, then, that they became "anti-historians," as Edgerton puts it. Their primary rhetorical crime, in my mind, was not in misrepresenting the history of science and the war, but in concentrating on their traditional oppositional

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[151] Solly Zuckerman, "The Social Function of Science" [Rickman Godlee Lecture, University College ,London, 1960], printed in Solly Zuckerman, *Scientists and War: The impact of science on military and civil affairs* (New York: Harper & Row, 1967), 140-160, esp. pp. 140-142, is insightful on the intellectual history of the competing movements, and recognizes that Bernal's call for planned science largely came to pass. See also the critique of C. P. Snow's "two cultures" on p. 145. For a radical interpretation of the events, see Werskey, *Visible College*, p. 303.
[152] See Edgerton, "British Scientific Intellectuals," pp. 21-22; and Edgerton, *Warfare State*, pp. 204-205.

emphasis on what policymakers could gain from scientists, and not enough on the crucial Tizardian theme of what scientists could gain from operational personnel and policymakers. Due to rhetorical carelessness, the impression has been left that OR and the introduction of scientists into policymaking were the same thing. As we have demonstrated, it was OR's epistemological dependency on military heuristics that made it an essential complement to scientific advice from 1941 onward.

Sir Henry Tizard and Postwar Science Policy Committees

Even as the left began extolling the wartime developments in scientific organization as a lesson to be applied in peace, the first opportunities to connect these lessons with contemporary developments came swiftly with the 1947 creation of the Defense Research Policy Committee (DRPC) and the Advisory Council on Science Policy (ACSP) under Clement Attlee's Labour Government. The choice of none other than Sir Henry Tizard to chair both bodies virtually ensured that they would become a locus of rhetorical attention. In fact, we can see the lasting impact of this rhetorical attention in Jon Agar and Brian Balmer's historical study of the DRPC's work. In searching for a "science interest" that introduced a scientific position into government in the committee's work, they conclude that no such interest was identifiable, since the DRPC was constituted of both military and scientific representatives from separate military services. Rather, they persuasively argue, the DRPC served as "a gatekeeper because of its position at a bureaucratic crossroads: it was both a *sink* and a *source* of information, a site at which both science and defence policy were within a single community's terms of reference." In other words, it was a typical Tizardian organization.[153] But, if such was the case, why did Agar and Balmer ever even expect to find a unique "science interest" in the DRPC?

As we might now suspect, the historiographical reasons run deep. The wartime experience was of great importance in interpreting the importance of the committees, and not just by the left. Frederick Brundrett—who had advanced through the Admiralty research establishments to become the Chief of the new Royal Naval Scientific Service and, *ex officio*, a member of the DRPC—was fond of recapping the story told in this chapter as an ascent of scientists through the establishment of DSR posts in the Admiralty and Air Ministry, Tizard's CSSAD, operational research, the establishment scientific advisory positions and, ultimately, the establishment of the postwar committee on which he served under Tizard.[154] Tizard himself seems to have had a similar appraisal of the history. He lauded the "little but significant revolution," bringing scientists into the realm of "organisation, training, tactics and strategy," and added that it "did not occur without some resistance." He praised the military in particular, saying, "The system has come to stay, I hope. Scientists are concerned with all problems of war, at all levels; not merely with the technical problems of particular equipment." However, he acknowledged there was a long way yet to go in the area of civil policy.[155] It is

[153] Jon Agar and Brian Balmer, "British scientists and the Cold War: The Defence Research Policy Committee and information networks, 1947-1963" *Historical Studies in the Physical and Biological Sciences* **28** (1998): 209-252.
[154] See Frederick Brundrett, "The Place of Science in the Machinery of Government," 2/21/1951, TNA: PRO DEFE 19/24; or lectures from 1955 and 1957 in TNA: PRO DEFE 19/2. Tizard served as chair of the DRPC from 1947 to 1952, when John Cockroft, who was the head of the Atomic Energy Research Establishment at Harwell, took over. Cockroft served for only two years, and Brundrett took over in 1954. Brundrett remained until 1959 when Solly Zuckerman assumed the post from 1960 until its dissolution in 1963. He became the Government's science advisor in 1964.
[155] Henry Tizard, "Science and the Machinery of Government," 11/24/1951, *HTT* 611.

ambiguous whether Brundrett's and Tizard's comments reinforced an oppositional view.[156] At least one prominent scientist was far more clear about it.

After the war ended, Patrick Blackett returned to academic work, but as the president of the AScW at that time, his reaction to the establishment of the DRPC and the ACSP rather recklessly mixed the oppositional view of the social relations of science movement with the Tizardian view he acquired during the war. In the speech he gave to the AScW quoted earlier, Blackett's praise of Tizard's approach to research administration was made in the context of announcing his hope that the postwar committees would bring a much-needed spirit of collaboration to Britain's scientific needs. Earlier in the speech, he had chastised the Ministry of Fuel and Power, relating how he had been in an interview with Ministry officials—"at which, incidentally, no scientist or engineer from the Ministry was present"—wherein he was assured that an adequate scientific and technical staff would be appointed. Blackett felt that nothing was, in the end, done, and asked:

> Is it likely, to take but one example, that with a technologically informed Ministry with an Operational Research Section, this country should have been so short sighted as to take off purchase tax from electrical appliances, and make an unnecessary increase in train services, a few months before a winter during which it was certain that coal stocks would be the lowest ever?

Intriguingly, Blackett decided, this (apparently) clear lack of a healthy attitude toward science was a result of what he (admittedly "flippantly") called the "counter offensive of

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[156] Edgerton argues that Brundrett and Tizard were clear in their respect for administrative authority, *Warfare State*, p. 185; but a careful reading of Bernal and Blackett's writings indicate that they, also, did not want the scientist to be "on top" as the saying goes. It is important to keep in mind that the problems were are discussing are more questions of rhetoric than of actual belief. As we will see Blackett was always willing to retreat to a clear Tizardian stance when pressed.

the administrative civil service" against the incursion of "technologically educated but sometimes rather uncouth outsiders."[157]

With the appointment of Tizard to the ACSP, though, Blackett felt that the "defence" had begun to stop the counteroffensive. Here Blackett recalled Tizard's aforementioned round table exploits "to thrash the problem of the defence of London. This," Blackett insisted, "is the spirit in which the tasks of peace should, but are not always, being tackled." He finished by reiterating the central tenets of Tizard's philosophy of administration as well as his own operational research: "Much that concerns the future development of science in this country and its application to our present needs, will depend on the scientific judgment, practical understanding and social responsibility" of the members of the ACSP. He continued, "To tackle the numerous tasks in front of them, we will expect them to draw, through the machinery of advisory panels, etc., of all the relevant ability to be found in this country."[158]

One must be careful at this point. As Brundrett's and Tizard's observations suggest, British scientists had indeed come to play a greater role in policymaking. But Blackett's rhetoric of a scientific defense arrayed against an administrative counteroffensive more closely echoed A. V. Hill's vocalization of the oppositional view. While embracing Tizardian administrative virtues in principle, Blackett's and Hill's rhetoric simultaneously subverted them by conflating the issue of scientists'—and particularly outsider scientists'—participation in policymaking with the introduction of a spurious scientific viewpoint, to which government administrators were somehow ill-advisedly opposed. In Hill's wartime critique of British tanks in Libya and in Blackett's

[157] Patrick Blackett, Presidential Address, 5/24/1947.
[158] *Ibid.*

critique of Britain's postwar policy on appliance taxes and train schedules, the missing element was always implied to be the scientific perspective. If responsible scientists (and hence science) entered the deliberation, different conclusions would surely have been reached. All this despite the fact that what was called for was usually not a sophisticated scientific analysis, but good administrative and technical liaison and informed executive judgment.[159]

Blackett and the Legacy of OR

Blackett and other radical scientists' shifting criticisms of government science, their occasional implications that if only government would listen to them things would be better, their criticisms of militarism, their continual invocation of the role scientists had played in the war, the swiftly spreading definition of OR as the application of scientific method to military problems very quickly conspired to create the impression that "science" offered the answers to Britain's postwar economic doldrums, with the miracles performed by OR scientists for the inept military as evidence. The *Nature* editorial at the beginning of this chapter is an excellent example of this misimpression. Blackett, who had almost nothing to do with OR after the war, but was universally associated with it, soon found himself in an awkward position. He, rightfully, did not want to distance himself from the very real contributions of the wartime OR work or the notion that the work was scientific, but he also had to dispel the idea that OR and

[159] R. V. Jones may have had something of this sort in mind when he commented: "It is obviously dangerous, as Sir Charles [Snow] says, 'to have a scientist in a position of isolated power, the only scientist among non-scientists', but the danger lies much more in the isolation than in the power; *and this observation is not peculiar to scientists*." Emphasis added. R. V. Jones, "Judgement," p. 285.

scientific advice in general was about scientists setting military personnel straight, even though that is just what most scientific intellectuals seemed to see themselves as doing.

Blackett made his first real attempt to rectify the problem in the inaugural issue of *Operational Research Quarterly* in 1950 by offering a long and convoluted definition of scientific method that was so vague as to encompass most any problem-solving activity. He felt obliged to point out that OR had not really been unique, and he devoted the bulk of his article to his most explicit discussion yet of the centrality of Tizardian virtues to OR practice.[160] In a 1953 talk to the Institute of Physics, he took on the mythology surrounding OR more explicitly. "There is now a kind of deceptive simplicity," he said,

> about the results of these [wartime OR] investigations which tends to make previous tactics employed seem rather stupid and ill advised. Actually, it wasn't at all like that. It is extremely hard to see through complicated things to a simple solution and in many cases it needed a large background of service knowledge to be sure that a simple argument was correct. Operational research is not, as some people think, a case of the bright scientist suddenly intervening and telling the experts what to do. It is very much more the slow and careful enquiry into extremely complicated matters by scientists who have soaked themselves in the atmosphere of an operational command.[161]

His corrective led him away from the sociopolitical rise of the scientists championed by Bernal and himself, and back to the Tizardian administrative innovation that put scientists into contact with the "atmosphere" of the "operational command" where real operational knowledge resided.

Isolated segments of isolated speeches do not have the myth-destroying qualities to take on the notoriety of Blackett's own book-length critiques of Britain's nuclear policies.[162] Further, the lavish media attention given to scientists in the wake of the war

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[160] Blackett, "Operational Research".
[161] "Operational Research, Notes on a lecture given by PMSB to Institute of Physics," 3/17/1953, *PMSB*, PB/4/7/2/10 (formerly D.106).
[162] The most notable example is P. M. S. Blackett, *Military and Political Consequences of Atomic Energy* (London: Turnstile Press, 1948); published in America with the main title, *Fear, War and the Bomb* (New York: Whittlesey House, 1949). See also P. M. S. Blackett, *Atomic Weapons and East-West Relations* (Cambridge, UK: Cambridge University Press, 1956).

and the atomic bomb helped thrust scientific analysis into the public spotlight, predominantly through the RAND Corporation's sprawling systems analysis studies and its analysts' publications on nuclear strategy. The methodological ins and outs that RAND employees debated, however, were not so well understood as the concept of the "think tank" which essentially connoted a group of brainy scientists who told the military what to do by virtue of science, whatever that actually entailed.[163] Blackett's wartime work and his postwar critiques fit too easily (if mistakenly) into this (false) picture. RAND, for its part, took no great pains to combat the public image. Blackett, meanwhile, must have chafed at RAND's unabashed association with militarism and his own association with the rise of RAND by virtue of his association with OR. He felt certain that RAND did not possess the same epistemological legitimacy which his embrace of Tizardian administration had given his wartime OR work.

Then, in 1958, Charles Hitch reiterated his 1952 sub-optimization argument for a symposium on "economics and operations research" published in the *Review of Economics and Statistics*. Sidney Dell, who had worked under Blackett at the Admiralty, and was then working for the United Nations in America, saw Hitch's article and was incensed by a segment that criticized Philip Morse and George Kimball's choice of optimization criteria (ratio of U-boats to merchant vessels sunk) in their treatment of the problem of convoy size in *Methods of Operations Research*. Of course, the convoy size problem had initially been studied by Blackett's Admiralty group, and their conclusion that larger convoy sizes were preferable had since gained notoriety as an example of successful and important work of OR scientists. Dell wrote to Blackett, drawing his

[163] Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, Mass.: Harvard University Press, 2005), chapter 2, is probably the best source on the postwar image of RAND.

attention to Hitch's suggestion "that the criteria used by 'operations researchers' have frequently been inferior or misleading and," Dell went on acidly, "[he] adds, with some smugness, that 'the really bad ones involve errors that I think would not have been made by a person with good economic intuition.'"[164]

Hitch's language inadvertently pushed buttons. Economists were often scorned, then as now, for sporting an overly-theoretical view of the world, and as the head of the economic staff at RAND, of all places, Hitch was in a particularly unsympathetic position. Of course, had Hitch's commitment to a more practical and usable systems analysis based on his sub-optimization principle been more widely known, he might not have provoked the reaction from Dell and Blackett that he did. Blackett replied to Dell that he was alarmed by a "dangerously abstract approach to [...] practical problems," pointing out that, given the data available during the war, it had not been possible to suggest an upper limit on convoy size. To have attempted to do so would have been an abdication of scientific responsibility. "I fear," he wrote, "that a great deal of Operational Research done by clever but rather conceited young men, who know very little about the subject about which they are talking, could easily discredit Operational Research seriously."[165]

Dell and Hitch published dueling follow-ups in the *Review*, in which Hitch soundly clipped Dell's wings (even neatly including a line from one of Blackett's publications suggesting that the problems of OR were closer to "biology and economics" than physics).[166] Blackett and Hitch also exchanged a brief correspondence several

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[164] See Charles Hitch, "Economics and Military Operations Research," *The Review of Economics and Statistics* **40** (1958): 199-209; Letter from Sidney Dell to Blackett, 12/31/1958, *PMSB*, PB/4/7/3/8 (formerly D.115).

[165] Letter from Blackett to Dell, 1/20/1959, *PMSB*, PB/4/7/3/8. Hitch, incidentally, had just turned 49; Blackett was 61.

[166] See S. Dell, "Economics and Military Operations Research," *The Review of Economics and Statistics* **42** (1960): 219-222; and Charles Hitch, "A Further Comment on Economics and Military Operations

months later, in which Blackett came to understand that Hitch had not been taking aim at the wartime study, but Blackett insisted, "I still think that there is a considerable difference of opinion between us on what can or cannot usefully be predicted."[167] He must have been intent on letting off steam at RAND rather than engaging with Hitch, because Hitch's argument had basically amounted to a critique of Morse and Kimball's *own* over-abstraction of the convoy problem, specifically their failure to include factors subject to alternative criteria of analysis (shipping efficiency) that would have suggested a cap on convoy size if, indeed, it did not reverse the conclusion altogether. For Hitch, such omissions, not a failure to give precise answers, were the true abdication of the analyst's responsibility.[168] Nowhere did he imply, as Blackett claimed, that the Admiralty should have actually designated a precise optimal convoy size—though the original wartime paper "The Case for Large Convoys," in fact *did* suggest a 50% increase in average convoy size (without claiming optimality), *and* explicitly included sections analyzing the difficulties of handling large convoys and the question of fixing an upper limit on convoy size.[169] These were *precisely* the things Hitch had found wanting in Morse and Kimball's treatment. Blackett was clearly out of line.

Nevertheless, Blackett was not finished criticizing RAND and intellectual strategists from other American institutions. In 1961 he viciously attacked them in a speech entitled "Operational Research and Nuclear Weapons" to the Royal United

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Research," *The Review of Economics and Statistics* **42** (1960): 222-223. Blackett's quote was from his 1943 memorandum, "A Note on Certain Aspects of the Methodology of Operational Research," reproduced in *Studies of War*, quote on p. 177. Hitch saw a version reproduced in *The Advancement of Science* in 1948, which he considered an excellent introduction to OR.

[167] Letter from Blackett to Hitch, 9/17/1959, PB/4/7/3/8. Blackett was "in the process of digging out of the Admiralty our original papers on the convoy problem…"

[168] It is another matter as to whether Hitch was being too hard on Morse and Kimball for what amounted to a textbook example. Morse and Kimball, after all, shared Blackett's commitment to studying problems in a practical forum, and explained as much in their book.

[169] See C.A.O.R., "The Case for Large Convoys" n.d. [1943], TNA: PRO ADM 219/19.

Services Institute that was published in the Institute's journal.[170] In a draft of the speech, he once again criticized Hitch's article, although by this time he seems to have read it carefully and considered it "otherwise wise".[171] His real targets now were game theorists and nuclear strategists, and especially Albert Wohlstetter, who had argued against the stability of nuclear stockpiles as a deterrent. As a pundit on nuclear strategy himself, Blackett was eager to poke holes in Wohlstetter's argument that conditions might arise that would make a nuclear first strike seem like a plausible strategy for the Soviets.[172]

What concerns us most immediately about this speech is the connection Blackett saw with his own work in OR. In his speech, he recalled an encounter he had had with "a great friend" who was "an American scientist of great distinction and wisdom.... After a long discussion of some of the errors of this academic literature [on nuclear strategy] he said: Blackett, are you not ashamed of yourself for having developed operational analysis?"[173] Blackett was not, and he took great pains to highlight the differences between what he had done in the war and what RAND analysts were doing. He pointed out that it was impossible to conjure theoretically any useful conclusions where operational data was unavailable, thereby limiting the range of possible conclusions that scientists could legitimately make—in cases of uncertainty, he said, "they should keep

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170 P.M.S. Blackett, "Operational Research and Nuclear Weapons," *Journal of the Royal United Services Institute* **106** (1961): 202-214. The lecture was delivered on 3/22/1961. It was altered and published under the title "A Critique of Some Contemporary Defence Thinking" in *Encounter*, April 1961. This version was reprinted in P.M.S Blackett, *Studies of War*, 128-146. A draft of the introduction to the speech that resides only in Blackett's papers is most illuminating, *PMSB*, PB/4/7/2/13 (formerly D.109) and will be quoted from here and cited as "draft".

171 The Hitch critique appears only in the draft, p. 9.

172 Blackett quoted Wohlstetter (*Studies of War* reprint, p. 135): "...the risks of not striking might at some juncture appear very great to the Soviets, involving, for example, disastrous defeat in a peripheral war; loss of key satellites with danger of revolt spreading possibly to Russia itself; or fear of attack by ourselves." This speech was given only a year and a half prior to the Cuban Missile Crisis. Blackett was adamant (p. 136), "In the list of imaginary circumstances which are depicted above as likely to provoke a Soviet strike, there is only one, in my opinion, which has any semblance of reality; this is the fear of an immediate strike by America."

173 Blackett, "Operational Research and Nuclear Weapons," p. 343.

silent: never should they fall into the trap of decking out what is essentially only a hunch with a pseudo-scientific backing."

Continuing a theme we have seen repeatedly, he (suddenly) began invoking the clear division between the planning role of the executive and the field research responsibility of the scientist. He averred "that the work of the operational research worker was an addition to, and not a substitute for, the exercise by the trained staffs of conventional military wisdom."[174] As for his own strategic theorizing, he emphasized:

> I do not think that my experience of four years of active operational analysis during the War has given me any reason to suppose that my views on the present [strategic] situation are any more likely to be reliable than those of any other academic who has studied the subject deeply, except in one respect: my experience has given me some personal acquaintance with how major decisions in war are in fact taken.

In criticizing Wohlstetter and others, he thus insisted, "I do so, not as an ex-operational researcher, or even as an ex-professional fighting man, but as one who is deeply concerned with making the right choice of policy..."[175] Yet, strategizing, planning, and research had all, by the early 1960s, become a part of the notion of scientific analysis. As we saw in the last chapter, the same freedom Blackett claimed as an outside critic was claimed in the postwar era by RAND analysts in their role as independent developers of robust policy options. The philosophical differences between what Blackett was doing as an outside pundit and what RAND was doing as a contract research organization were actually razor thin.

Alternative Views in Industry and Government

Postwar opinions of OR among scientists who left the civil service but remained interested in the wartime experience were not always encumbered by the broader

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[174] Blackett, *Studies of War* reprint, p. 129.
[175] Blackett, draft, p. 15.

question of the relationship between scientists and policymakers—even though these scientists might had an interest in the problem. Especially among those who fostered an interest in the continuing fate of OR in industry and the military, OR could still be seen as an administrative accomplishment that was only marginally connected to the problems of coordinated scientific effort that had surrounded its rise in 1941. Maurice Kirby has already documented the spread of OR in British industry quite well, identifying the two newly-created organizations, the British Iron and Steel Research Association (BISRA) and the National Coal Board, as the most important sites of development.[176] Significantly, OR teams were introduced into each location by scientists with major responsibilities for coordination of scientific effort in the war, but who had arrived *after* the 1941 rise of OR, and were not otherwise major political players. These were the chemist Charles Goodeve, the Deputy Controller for Research and Development at the Admiralty, who became the Director of BISRA; and the physicist Charles Ellis, who had replaced Charles Darwin as Scientific Adviser to the Army Council, and became the "scientific member" of the National Coal Board.

Ellis cannot really be considered either an activist for science or even a booster for OR.[177] Accordingly, we know very little about what significance he attributed to OR, except that he felt it was prudent to establish it in the British coal industry. Yet, in its own way, Ellis' case is revealing. As I am arguing, the stories told about OR during the

[176] Kirby, *Operational Research in War and Peace*, chapters 7 and 8. On the history of OR at the National Coal Board, also see Rolfe C. Tomlinson, ed., *OR Comes of Age: A Review of the Work of the Operational Research Branch of the National Coal Board, 1948-1969* (London: Tavistock Publications, 1971).

[177] On the other hand, Ellis' successor as SA/AC, Owen Wansbrough-Jones, became the first president of the Operational Research Society in Britain. According to Kirby, *Operational Research in War and Peace*, p. 382, "In the 1950s, the elected presidents of the national society had been established public figures in the worlds of science and Whitehall and all of them had been persuaded to stand for office in the hope and expectation that they would prove to be active ambassadors on behalf of the operational research community."

war ascribe a missionary spirit to it, stating that it brought science into the deliberations of policymakers. However, in reality, science, as OR's British proponents understood it, was already a part of their deliberations; OR simply augmented an existing heuristic process. The same was true of the NCB, which had decided to include Ellis in its numbers. It is because Ellis seems to have lacked any sort of missionary pretensions that we can clearly see how he translated his War Office experience in the industrial sector.

From his new position, Ellis played a crucial role in coordinating the role of science in the continuing modernization of the British coal industry. OR, the work performed by what was initially called the NCB's Field Investigation Group, which Ellis established, was a constituent part, but *only* a constituent part, of this effort. Upon arriving at the NCB, Ellis had called for the establishments of a number of new research directorates to handle "fundamental laboratory work, operational research on coal-getting and coal preparation, [and] research into matters involving human aspects, e. g. health of the miner."[178] While included, OR seems to have held no emblematic significance for him, even though his staff was just as aware as more politically sensitive scientists of "the tendency to relegate scientists to the back room (the 'boffin' complex)," but there was no need to wheel out the history of OR against some sort of anti-scientific backlash. They knew "impeccable administration and staff duties will help to overcome it".[179]

Charles Goodeve, like Ellis, also incorporated OR into a much larger apparatus for the management of industrial science. Unlike Ellis, though, after he joined BISRA, Goodeve became a prolific speaker and writer on OR, extolling both its wartime successes and its industrial applications. His historical treatments more-or-less mirrored

[178] "The Organisation of the Scientific Department of the National Coal Board: Memorandum by the Scientific Member" n.d. (dated 3/18/1946 from other sources), TNA: PRO COAL 33/21.
[179] Memorandum from G. Essame to Ellis, 1/28/1947, TNA: PRO COAL 33/21.

those of the intellectuals, but, while he lauded OR as an application of scientific method to military problems, he actively resisted mystifying it, offering the definition that it was "quantitative common-sense," and observing that it was "not new to industry". He identified its pre-existing manifestations in "market problems, cost accounting, quality control and works efficiency; in other words the operations of industry."[180] This point is all the more significant because Goodeve had plenty of opportunity to invoke OR in a politically emblematic way. As a proponent of industrial cooperation "between separate firms and between firms and the suppliers of their raw materials and equipment and the users of their products," Goodeve insisted, "We are not forgetting that of equal importance is co-operation between the research worker and the other parts of the mechanism of industry."[181] Yet, unlike Bernal, Blackett or other more political scientists who expressed a similar vision, Goodeve's interest in OR, like Ellis', was not as an historical exemplar, but as a current phenomenon.

In a 1946 speech to the Sheffield Society of Engineers and Metallurgists, he invoked a deeper, more localized history than the wartime one as a precedent in order to push his efforts to improve scientific and administrative collaboration within the iron and steel industry. This history began with the foundation of the Iron and Steel Institute and various "local Metallurgical and Engineering Societies" in the previous century. These institutions "provided means by which people got together and discussed their experiences." In the First World War committees were created under the Iron and Steel Institute "to investigate certain problems which were troubling the industry as a whole."

[180] See especially Goodeve, "Operational Research,"; "quantitative common-sense" from p. 377, and "Operational Research: A paper based on recent lectures given by Sir Charles Goodeve, F.R.S.," December 1947, *GOEV* 5/1, which is the source of the quotes given here.
[181] Charles Goodeve, "The Future of Co-operative Research in the Iron and Steel Industry," a speech given at the Sheffield Society of Engineers and Metallurgists, 2/18/1946, *GOEV* 5/1.

After those ephemeral organizations dissolved, "the executives in the steel industry got together and set up the National Federation of Iron and Steel Manufacturers to tackle some of their common problems." After that, "those concerned realised that the firms' common commercial problems were intimately related to their common technical problems." So, "They set up a Fuel Economy Committee and research group." The group, which later became the Technical Department of the renamed British Iron and Steel Federation under the Iron and Steel Industrial Research Council, collaborated with the Iron and Steel Institute during the Second World War "and made important contributions to the [...] war effort."[182]

Finally, as the war was coming to its close, the industry established BISRA. Goodeve observed, "Some ill-informed people look upon the lateness in the setting-up of a Research Association for iron and steel as an example of conservatism on the part of this industry." He was not such an employer of the oppositional view, though. The Technical Department and the joint wartime committees, he countered, had effectively performed the same role as a research association, and he defended the research facilities and funding in the industry as second-to-none. "The iron and steel industry," he declared, "has always been and remains in the forefront in organised research and in addition has a healthy attitude of self-criticism coupled with a desire to do more."[183] Goodeve gave this speech shortly after leaving the Admiralty, but its understanding of the history of scientific collaboration within the industry and its forceful defense of industry research practices marked him as an insider. Like E. V. Appleton at the DSIR in 1939, he could have been called "more royalist than the king," but that accepts the dubious idea that an

[182] *Ibid.*
[183] *Ibid.*

entrenched counteroffensive against outside scientific participation actually existed. Within intellectual circles, the ideal kind of research structures that they wanted to promote were only expressible in vague terms referring to the introduction of scientific viewpoints and through a limited set of recent historical icons such as OR. In more localized contexts these same sorts of collaborative structures were equally desirable, but were understandable as developments within the traditions of that context. It was not necessary to draw upon the historical discourse of the war—even when the iconography was readily available to an OR booster such as Goodeve.[184]

Solly Zuckerman represents an intriguing case of a hybrid of intellectual and localized perspective. Zuckerman was a zoologist and socialite whose lengthy memoirs are filled with the dropped names of famous scientists, powerful politicians and major cultural figures. He was the proud impresario of the aforementioned Tots and Quots dining club, members of which wrote the *Science in War* Penguin special. During the war, though, as we have seen, he began to burnish his insider credentials, working at the Ministry of Home Security and under Lord Mountbatten and Air Marshal Tedder. After the war, he became the deputy chair of the ACSP and in 1960 he became a full time civil

[184] Another instance of Goodeve's discussion of coordination within a localized context is after a 1966 trip to the Massachusetts Institute of Technology, he used its system of spin-off academic projects funded by venture capital as another example of a healthy research atmosphere, and caused him to call for a "Route 128" for Britain. See Charles Goodeve, "A 'Route 128' for Britain?" *New Scientist* (9 Feb. 1967): 346-348, and especially his confidential notes from the trip, August 1966, *GOEV* 5/8. It is worth noting that Goodeve did come to see OR, especially after his retirement, as possibly playing a significant role in society, but his conception did not resemble scientific leftists' invocation of OR as a crucial cog in the science coordination machine. Rather he suggested OR could entail such things as a study of the nature of conflict. An early example is Charles F. Goodeve, "Operations Research – A new approach to complex social problems," a speech given at the Case Institute of Technology, 4/11/1953, *GOEV* 5/4. See also Charles Goodeve, "Science and Social Organization," *Nature* **181** (1960): 180-181; and Charles F. Goodeve, "Science and Social Conflict," *Journal of the Operational Research Society* **29** (1978): 289-298.

servant as the chair of the DRPC, which also made him science advisor to the Ministry of Defence. In 1964 he became the British Government's first official science advisor.[185]

"An Uneasy Alliance," the first of Zuckerman's 1965 Lees Knowles Lectures on Science and Military Affairs at Trinity College, Cambridge, is a thoroughly typical example of the oppositional view right down to its title. In the talk Zuckerman identified a centuries-long alliance between scientists and technicians and military officers (name checking Archimedes and Leonardo da Vinci), and went on to suggest (rather oddly) that a nineteenth century divergence of the science and the military on account of the coming of the "age of specialization, ushered in by the industrial revolution," in which the "soldier cocooned himself in an isolated and proud professionalism, whose ritual was the admiration of all," while "scientists started to develop as a race apart," and "became enemies of the conventional and established," thereby establishing a clear "contrast of the scientific and military minds". Zuckerman's history leaves something to be desired, of course, but, continuing apace, he observed that the First World War and especially the Second tore down the nineteenth century wall, and that scientists became more tightly bound to the military than ever through operational research. However, like Blackett, he was wary of some postwar analytical work, and warned that advances in OR, cost-benefit and scenario analyses, had their limits. He concluded on the more Tizardian note that analyses always had to be based on facts, "And the facts, *the real facts*," he insisted, "will not come to him of their own accord. It is for the military scientist to go out and seek

[185] Solly Zuckerman's memoirs, Zuckerman, *From Apes to Warlords* and Zuckerman, *Monkeys, Men and Missiles*, still provide the best information about him. See also a recent (much shorter) biography by John Peyton, *Solly Zuckerman: A scientist out of the ordinary* (London: John Murray, 2001).

them—in the laboratory, through field tests and by direct observation in theaters where our forces might be engaged in active operations."[186]

When not speaking to a general audience at Trinity College, though, like Ellis and Goodeve, Zuckerman was capable of couching his arguments in more localized terms. A memorandum for defense science reform he wrote in 1963 seamlessly merged the oppositional view with the Tizardian language of bureaucratic reform. Portions of the memo he excerpted in his memoirs illustrate how, while his own position was the most striking example of the continuing administrative developments for science policymaking, he still believed that research establishment scientists were

> men who could contribute materially in the taking of major decisions" but they tended "to be regarded as 'technicians'". They took no direct part in the discussions where the decisions were taken. In this way, so I wrote, "narrowness and exclusion become perpetuated", and scientists become isolated in departments within their specialties. This, I pointed out, also conduces to isolation from the very thriving world of science in the universities....

Using that timeless (and often factually questionable) spur to action, Zuckerman pointed out that in other countries scientists "are not only concerned with the business of bringing science and technology to bear on defence, but also in the administration of R&D, and in the determination of policy." In addition, in defense there was also a bigger gap between the "decision-maker" and the "scientists and technologists" than in industry. He framed his ideas for reform, though, not as a lost history of the war beat back by a "counteroffensive", but as an addition to the still tangible legacy of that history. OR, as in the National Coal Board and BISRA, was a component of his broader science policy. Zuckerman recalled, "To help promote a coherent defence policy, the separate operational research groups of the three Services also had to be brought together into one

[186] Solly Zuckerman, "An Uneasy Alliance," October 1965, printed in Zuckerman, *Scientists and War*, pp. 3-28. Quotes on pp. 6-8, 28, emphasis added.

integrated inter-Service organization."[187] Zuckerman's memorandum is about only instance I have found connecting the oppositional story in which OR has been such a prominent rhetorical fixture with the actual administrative postwar developments of military OR presented in chapter three.

Conclusion

In 1972 Sir Henry Tizard's son, Peter, wrote to an aging A. V. Hill that the writings about his father had "all been influenced by that deplorable (although exceptionally well written) book by Snow in which the author is the real hero!"[188] Indeed, although C. P. Snow lionized Tizard, *Science and Government* was not really a story about science and government in Britain the way Tizard perceived it. Snow concluded the book by saying he greatly admired government administrators, but that they reminded him of a line from an Icelandic saga: "Snorri was the wisest man in Iceland who had not the gift of foresight."[189] Foresight, representing in his mind an ability to understand the consequences of technological and social progress, was a gift reserved primarily for scientists. But Tizard himself never really saw so much difference between Snow's "two cultures" of scientists and administrators. Each side simply had knowledge the other did not. The important thing was to make sure they could speak with each other fluently and help each other effectively. As we have seen in previous chapters, most of the thinkers who moved OR forward during and beyond the war shared this view.

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[187] Zuckerman, *Monkeys, Men and Missiles*, pp. 361-363.
[188] Letter from Peter Tizard to A. V. Hill, 8/2/1972, *AVHL* II 4/80.
[189] Snow, *Science and Government*, p. 83, and the entirety of the last chapter. He also wrote (p. 80), "I suppose most scientists possess nothing of this foresight. But, if they have any trace of capability, then their experience, more than any experience at present open to us, gives them the chance to bring it out." On the question of Snow's views of scientists and progress see Ortolano, "Human Science or a Human Face?"

For all their superficial similarities, the oppositional view and the Tizardian virtue of administration were not the same thing. For Tizard, OR entailed an exchange of expertise, between specialists and other field personnel, officers, and scientists. The oppositional view, on the other hand, with its strong links to the social relations of science movement, emphasized the importance of bringing scientists into military and government deliberations because otherwise the products of science would be misused by non-scientists who did not properly understand them. By fitting OR within an oppositional framework, scientists such as Bernal and Blackett, while making valid points about the ways scientists and policymakers could interact to enhance policy, always risked losing track of the major force behind OR's wartime success. The impression left by the rhetoric they used to push their own political agendas forward was that if only the scientific perspective could find the respect it deserved, Britain would be able to rise to the challenges presented to it in the postwar era. Yet, when Blackett confronted the products of RAND Corporation, he was suddenly forced to retreat from his strong rhetoric about the power of scientists to inform decision making and to begin emphasizing the more Tizardian point that the policymaker's input was essential for the scientist's statements to have any real quality—without really comprehending or caring how much contact with the military RAND actually had or how its analysts perceived their role with respect to the military.

David Edgerton has shown that views such as Snow's have had a pervasive impact on the historiography of British science, and, indeed, on Britain as a nation in decline. In this chapter, we have limited our scope to this view's impact on the historiography of OR, but it is worth repeating Edgerton's call to move beyond Snow's view in the examination of the history of science in relationship to the British state. This dissertation as a whole has been an attempt to retell stories about OR and related areas in light of the notion that decision makers and scientists are not so epistemologically or heuristically different. Previously, the way this history has been told was based on the presumption that the entry of scientists into policymaking represented a new epistemological phenomenon in government, military and industry, creating shockwaves in the depths to be followed upward and outward. Blackett later ran into problems with this story, and so, I argue, have more dedicated historians. The history I have chosen to follow is one of new epistemological alliances between different communities of rational actors, not the explosion of an emergent culture of rationality amid a culture of intuition and politics. As much as this approach has benefits for studying the history of OR, and the history of science and the British state, I believe it is also a worthwhile approach to the history of the relationship between science and policy in general.

CONCLUSION

Some days we are convinced no one is listening; on others, we are, like the ants riding downstream on a log, convinced we are steering. We are doubtless wrong both times; for our prestige usually ensures us a hearing, but our views do not dominate in decision-making. However, it does seem that a lot of things go, eventually, the way we pull, whether it is a field of mathematics, of economics, or of operations analysis; a flood of computers, of ICBM's, or of satellites; a command-and-control philosophy, a civil defense concept, or some other facet of a war-fighting capability. We cannot prove that anything happens that would not happen anyway, but things may at least happen sooner when we start the discussion early, at a level of discourse that requires attention and thought by others. Moreover, it is inevitable that we do influence the future—when Merlin tells Guinevere that a dark, handsome stranger will enter her life, that changes the probabilities; he does not have to invent Lancelot too.

John Williams, "An Overview of RAND", May 14, 1962[1]

Leaving Colonel Blimp and Dr. Strangelove Behind

Two mythical figures have long dominated our view of the relationship between science and policy: let us call them Colonel Blimp and Dr. Strangelove. Colonel Blimp was a character created by the British cartoonist David Low, and was a frequent feature in the *Evening Standard* beginning in the 1930s. Almost always to be found in the Turkish baths, clad, at most, in a towel wrapped around his substantial belly, Blimp was famous for spouting kernels of daft wisdom, habitually prefacing his proclamation with his indignant catch phrase, "Gad, sir!" and his agreement with some notable, usually ennobled figure, such as the fictional Lord Flop, or the real Lord Beaverbrook. Although martial in name, he was willing to expound on any topic, domestic or foreign, and could be counted on to make an unwitting parody of a crusty, antiquated point of view reminiscent of upper class politicians (see Figure 7.1). Blimp's wisdom was as implacable as it was recognizable. As Colin Seymore-Ure points out in his introduction

[1] J. D. Williams , "An Overview of RAND," D-10053, 5/14/1962, RAND Corporation archives, John Williams Papers, Box 1.

to a collection of Blimp cartoons, "[Low critic] Harold Nicolson doubted whether he could find seven or eight people who really held Blimp's opinions; while the American journalist Ray Daniell, also in 1942 and in the same city, reported that 'It's impossible to live in London long without encountering the Old Colonel.'"[2]

Figure 7.1. Colonel Blimp makes a rare clothed appearance outside the Turkish bath to fight the Nazis and discourse with his doppelgangers on military strategy. This cartoon appeared shortly after Germany invaded the Soviet Union thereby opening up an eastern front while Britain remained mostly disengaged in the west and America remained, for the moment, neutral. David Low / "We Must Have Their Undivided Attention", September 22, 1941; British Cartoon Archive, University of Kent, catalogue record LSE3007 at <http://opal.kent.ac.uk/cartoonx-cgi/ccc.py?>, reproduced by permission.

While Blimp was a well-known character in Britain in his time, Dr. Strangelove has been an even more enduring name, and is more familiar to American audiences than

[2] Colin Seymore-Ure, "Introduction," in *The Complete Colonel Blimp*, edited by Mark Bryant (London: Bellew Publishing, 1991), pp. 13-29, on p. 21.

the old colonel. Strangelove, a wheelchair-bound German scientist and defense intellectual, was the title character in Stanley Kubrick's 1963 dark comedy, *Dr. Strangelove, or: How I Learned to Stop Worrying and Love the Bomb*. He was played by the British comedian Peter Sellers, and his ideas were based on those of Herman Kahn, who had then recently written *On Thermonuclear War*.[3] To be sure, the film is not short on Colonel Blimp characters, although they appear more akin to the younger Curtis LeMay than the crusty upper class British Blimp type. The film begins as one of them, Strategic Air Command General Jack D. Ripper, launches a full-scale nuclear assault on the Soviet Union, having convinced himself that the fluoridation of water represents an insidious Soviet plot. Strangelove does not appear until later in the story, after it is learned that the Soviets have secretly armed a "Doomsday Device" that will destroy all life on earth if either a nuclear weapon is exploded on Soviet soil, or any attempt is made to disarm it. Strangelove (who had commissioned a study of such a device from the "BLAND Corporation" and found it an impractical deterrent) chastises the Soviet ambassador for the Soviets' lack of logic: such a device is pointless if kept a secret. The ambassador replies that the Soviet premier, a drunk who "loves surprises," was due to announce it at the Party Congress the following Monday. As it happens, despite heroic efforts to recall the bombers, one does get through and triggers the Doomsday Device. As I described in chapter five, Strangelove consoles the distraught President (also played by Sellers) by pointing out that the nation's leaders can go into hiding in mineshafts deep beneath the dead surface of the planet. At a ratio of ten physically alluring women to each man, life—and strategic logic—will continue. Soon, the war room is abuzz as the

[3] Strangelove's German identity is likely a reference to German rocket scientist Werner von Braun, who was acquired by the United States after World War II, and was then working for the National Aeronautics and Space Administration.

assembled generals and politicians begin to worry whether the Soviets might stash away a bomb for the day a century off when their progeny would emerge from their mineshafts to repopulate the nations.

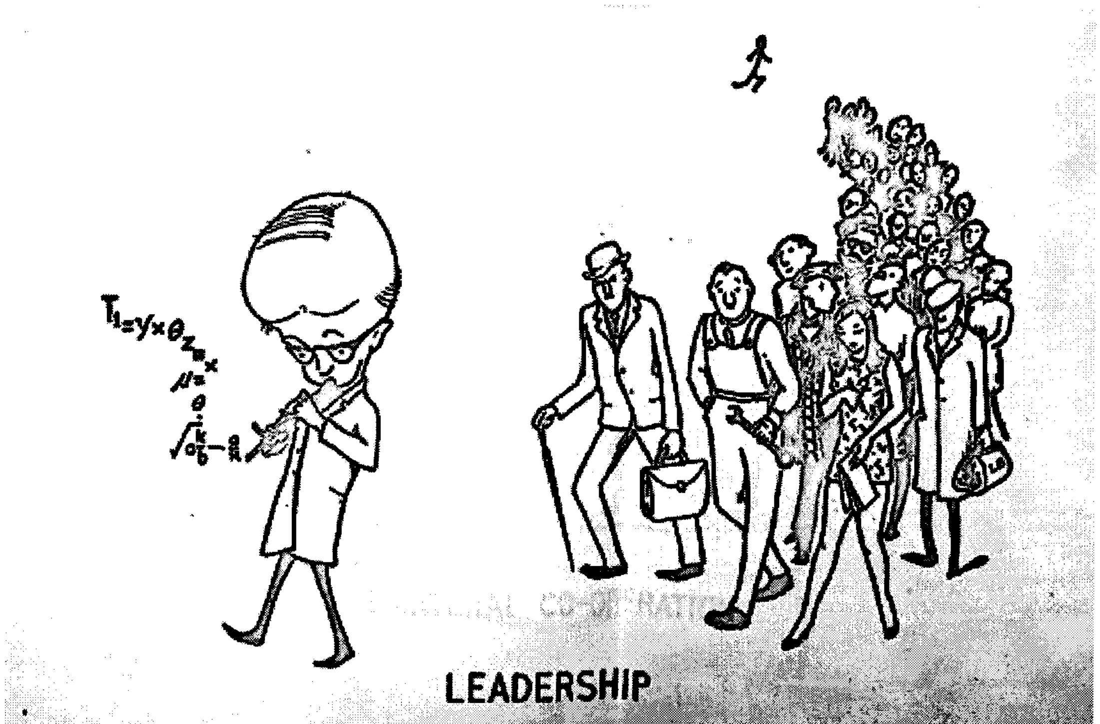

Figure 7.2. While not Dr. Strangelove, this figure, pictured in a slide from a lecture given by British OR booster Sir Charles Goodeve, demonstrates the same commonly expressed angst that mathematical and scientific experts would entrance policymakers with their strange but authoritative logic and lead society down an uncertain path. Source: Charles Goodeve, "The Future of Co-operative Research in the Steel Industry," 10/10/1968, *GOEV* 8/1.

Colonel Blimp and Dr. Strangelove are both extraordinarily compelling critical devices, but they are not especially useful from an analytical standpoint. Seymore-Ure's juxtaposition of the claims that Blimp cannot be found and that he is seen everywhere is apt. It is not difficult to cast historical actors in the tradition of these archetypes. Which archetypes get deployed in the history of the policy sciences and who is portrayed as a more reasonable lot usually depends on what direction one reads the story. For example, Sharon Ghamari-Tabrizi, in her excellent biography of Kahn; and Philip Mirowski, in his critique of the use of game theory in neoclassical economics, both look *backwards* at wartime OR and see a source of the Strangelovian tendencies they are tracing. Ghamari-Tabrizi sees a Blackett whose "smugness was intolerable to the services. Even in an age of wizards' war," she explains, "martial prowess still presided over the military, however scientifically enhanced it might be. Sir John Slessor protested that the instruments of war didn't win battles; human valor did."[4] Meanwhile, for Mirowski, in World War II,

> the crusading aura of the operations researcher was based on the assumption that he would bring the tonic of quantification to fields and questions which had previously been sorely wanting in that dimension; and as such, he helped to foster the mistaken belief that quantification and precision measurement were methodologically stabilizing influences bestowing scientific credibility in their own right, entirely removed from the larger social dimension of science.[5]

In his narrative, it was not a large step from the rationalization of equipment and tactics to the proposal of hyper-idealized models of the marketplace lacking a firm ontology (see Appendix A).

As I pointed out in chapter six, David Edgerton has also critiqued wartime operations researchers, but for quite different reasons. He objects to British OR proponents' portrayal of the military as a non-scientific entity. By making out the history of science in World War II to be a history of operational research and radar, they have systematically diverted attention away from more realistic portrayals of the longstanding relationship between science and the military—a practice he calls "anti-history". The military was not the Blimpish organization it was made out by scientists to be. Following history *forward* from wartime OR has led to a view of British history as removed from

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[4] Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear Warfare*, Cambridge, Mass.: Harvard University Press, 2005, pp. 46-47. Note the inverted Whig argument. According to Waddington, *OR in World War 2*, p. 12, under Slessor's command the RAF Coastal Command OR Section became better integrated into the Coastal Command organization.

[5] Philip Mirowski, "Introduction: Cracks, Hidden Passageways, and False Bottoms: The Economics of Science and Social Studies of Economics," in *idem*, *The Effortless Economy of Science?* (Durham: Duke University Press, 2004), p. 16.

technology until we soon find ourselves at Harold Wilson's 1964 election victory on the back of his ineffectual promise to exploit the "white heat" of science and technology, and to put Britain back on the right track.

However, moving forward from the war *and* crossing the Atlantic brings about a critical disjuncture. At some point some Americans—and here we are expected to turn our gaze directly to RAND—apparently took science too far, and gained quite a lot of power by doing so. Historians have often remarked on Blackett's critique of RAND in the early 1960s. Even Ghamari-Tabrizi and Mirowski, who point to Blackett as a nascent Strangelove, nod at his knowing recognition that wartime OR (which must have had some legitimacy because it tackled small, present problems) had been perverted into scientistic prognostication on the basis of questionable mathematics. For Mirowski, Blackett's criticism represents the protest of a decimated "British OR" that had been subsumed by a mathematically tricked-out "American" OR, dealing in the fundamentalist language of game theory, not the humble "string and sealing wax" of the British approach to science that retained some links to humanistic inquiry.[6]

The use of these critical devices has tended to create categories of winners and losers; or, more precisely, people historiographically identified as critics rather than people to be criticized, and vice versa. Blackett, especially later in his career, tends to be seen sympathetically as a critic of the inept use of science in British policy, especially with respect to nuclear weapons, and, in the early 1960s, of the inappropriate use of science at RAND. Similarly, such historians as Thomas Hughes and Stephen Waring have viewed Russell Ackoff as a critic of a humanism-deficient OR profession. Edwin

[6] Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (New York: Cambridge University Press, 2002), p. 328.

Paxson, on the other hand, is never viewed as a critic of arbitrary methods of equipment design, but always as someone to be criticized for his overambitious attempts to create an austere "science of warfare".

Our choices about whom we allow to speak as critics and whom we single out as targets of criticism often smack of an historiographical attempt to triangulate our way to a critical or even a moral high ground. By marshalling prior critical efforts into historical analyses, we succeed in pointing out how problematic the relationship between science and policy was and the present need to look beyond the distinction between them, and in so doing perpetuate the idea that they have heretofore been treated as separate, even after so many institutions have been built, and so much ink has been spilled because of the fact that so many people understood at the time that they were not.[7]

In my mind, what we historians of this subject need to do is to reevaluate our role as analysts. Rather than attempt to stand outside of these debates, we need to get inside them, and understand the motivations behind individuals' actions. Once we have evaluated what our historical actors were trying to do and the means they proposed to accomplish their ends, we can become much more cogent critics of both their means and their ends. Paxson, for instance, becomes a much more forceful intellect once we realize what design methodologies he was critiquing in his brief time at the Naval Ordnance Test Station, a military organization, and we can begin to understand why RAND thought it

[7] At this point, it is worth recommending an essay: Albert Wohlstetter, "Strategy and the Natural Scientists," in *Scientists and National Policy-Making*, edited by Robert Gilpin and Christopher Wright (New York: Columbia University Press, 1964), pp. 174-239. The entire volume, and particularly Wohlstetter's essay, represents a strong and early critique of C. P. Snow's essays, "The Two Cultures" and "Science and Government". Wohlstetter tends to be seen as a criticized figure rather than a critic, but his writing is frequently insightful, and should be taken seriously, even if his positions on defense matters are not always agreeable. The volume itself may be best known for the provocatively titled essay by Robert C. Wood, "The Rise of an Apolitical Elite," pp. 41-72, which is actually much more insightful than one might presume.

was a good idea to let him take three years and a significant fraction of their resources to put together a massive set of equations surrounding a preferred bomber design. We can also begin to understand in what ways his original systems analysis technique was useful to aircraft designers, and in what ways his analysis was inappropriate for time-staged projects and for the advisory work of the independent RAND Corporation.

Similarly, we can begin to see why Ackoff was not actually a very effective critic of OR. It did not have so much to do with the inward-looking model-centric nature of OR professionals preventing them from heeding his call to study the ethical elements of systems, as it had to do with the rationales underlying the way OR had divided itself from more traditional managerial activities. This is not to say that OR *theorists* were *not* inward-looking and model-centric, but rather that it made no sense for them to be any other way. To have expanded into the less rigorously quantitative areas of management was, effectively, to proclaim one's superiority to the entire managerial profession. More than anyone at RAND, Ackoff was guilty of conflating science with OR, and the two together with good managerial practice, and using this conflation to legitimize his simple consulting work by adopting an aura of forward thinking. When for this purpose his "science" horse became exhausted, he switched over to the more humanistic "systems thinking" horse that was gaining in popularity in the 1960s.

Ackoff's frustrations with OR as a science only demonstrates the shrewdness of the *professional* convergence of management consulting and operations research at Arthur D. Little. Using the firm as an intellectual venue wherein the real benefits of OR's new technical methodology could be combined with the more traditional authority of the consultant to interfere in *all* aspects of management, its analysts were able to offer a full range of advice to managers without the need for constant hand-wringing over professional boundaries. Indeed, it was not long before other consulting firms such as Booz, Allen, and Hamilton began to hire operations researchers as well.[8] However, the necessity of such professional alignments did not bode well for maintaining the identity of the practical side of the profession, but there had never been much reason to believe that the stability OR achieved in the 1950s (thanks to its adoption of a mathematical canon) could be easily preserved in the long run. Of course, it is worth pointing out that it *has* been preserved, despite a persistent struggle to maintain the identity of the profession.[9]

As I argue throughout this dissertation, the first twenty years of the existence of the term operations research were marked by continual shifts in OR's identity, all of which served to relate the activities of self-identified operations researchers to the concerns of differing intellectual and institutional policymaking environments. For this reason, its historical development defies any simple notions we might have about a patron-expert duality. It thrived by its ability to engage with policymaking cultures, not by its ability to replace them with a scientific epistemology. In 1940 operational research was a term used sporadically to indicate the study of technology in use in operations. Acting as the British Ministry of Aircraft Production's Scientific Advisor on Telecommunications, Robert Watson Watt identified OR as a means of tailoring his own

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[8] See J. W. Pocock, "Operations Research and the Management Consultant," *Journal of the Operations Research Society of America* **1** (1953): 137-144. Pocock was a consultant with Booz, Allen, and Hamilton. His article goes over many of the same points as Magee did, as described in chapter five.

[9] See Randall S. Robinson, "The Operations Research Profession: Westward, Look, the Land is Bright," in *Perspectives in Operations Research: Papers in Honor of Saul Gass' 80th Birthday*, edited by Francis B. Alt., Michael C. Fu, and Bruce L. Golden (New York: Springer, 2006), pp. 135-152. Within the past few years the Institute for Operations Research and Management Sciences has started a branding campaign (OR is now the "Science of Better"; see <www.scienceofbetter.org>) to identify OR more with managers' concerns.

advice on radar to the most pressing operational concerns, by deploying three Operational Research Officers to the RAF commands. In the summer of 1941, however, Patrick Blackett and Sir Henry Tizard redefined OR to mean any act of research bearing on operational planning. OR was no longer just a matter of equipment design and the improvement of its tactical use, but a means of studying operations in general, closer to intelligence analysis and general tactical development. Throughout the remainder of the war, OR spanned these two notions.

First and foremost, OR was an aid to ordinary military heuristic processes. It is unusual to think of military planning as an intellectually rigorous process, but many of the very best military leaders, such as General Curtis LeMay, whom we examined in some detail, were committed to developing their ability to understand the missions they conducted, perceive changes in them, and respond by altering doctrines and training. They fostered communication between crewmembers, planners, and technical specialists; and they built committees and special experimental units to help develop the scholarship informing mission planning. Operations research groups helped them in this process. Similarly, equipment designers maintained regular liaison with testing facilities and training schools, and prior to the war were beginning to relate the technical aspects of their designs to the results expected of their tactical use. As new equipment came into use without adequate development work and without a chance to establish doctrines concerning its use, the military and its laboratories began to establish new tactical development units and to employ scientific advisors, and laboratories began to send some of their scientists into the field to make recommendations on tactics and training, and also to develop various intermediary technologies—simple add-on devices, slide-rules, or even new sets of operating instructions—to improve the use of technology. Sometimes scientific personnel working on field problems used the term OR to describe their work, and oftentimes they did not. The Expert Consultant to the Secretary of War Edward Bowles used the term only sporadically to describe the work of the field consultants working under him, even though he was deeply committed to blurring the boundary between technical and tactical development. Although Warren Weaver was a great proponent of OR, he was even more interested in another British import, mathematical warfare analysis, as a means of improving decisions made about equipment designs with respect to their ultimate use.

The wartime history of OR, I would claim, is not really an origin story, but an adjunct of *other* military histories. The sense that OR actually represented a new concept has much more to do with the perspective of the personnel who performed it, and the new bureaucratic arrangements that accompanied its instantiation. The use of civilian scientists and the development of networks of advisors and OR groups was indeed in large part the result of pressure from scientists, especially Tizard, and it made existing military practices more focused and effective. Intellectually, however, we should remember that Tizard himself felt that OR was a "normal function of a well organized force." The history of these normal functions has never been told, and new histories might profitably focus on the interaction of military training, doctrine building, tactical development, and equipment design, with the growth of wartime OR as a part of this history rather than as the central focus.

OR survived the war's end, but without real operations to study it became identified more around the fact that it was performed by independent civilian analysts than the idea that it was the study of doctrines and plans in action. As such, OR had to be integrated effectively into military policymaking traditions, evaluating plans and doctrines to see to what extent they made sense in light of tests, exercises, intelligence reports, and other sources of military policy knowledge. Some groups kept their studies rather tightly confined to studies of weapons and tactics, but others, such as the British and especially the American army groups, incorporated the social sciences into their ideas about OR as well.

Meanwhile, a new set of organizations was established to evaluate equipment procurement decisions, the foremost of which was the Air Force contractor, the RAND Corporation. When Edward Bowles first envisioned RAND, it was as a place where military tacticians and equipment designers could come together to settle problems relating designs to their intended uses. Edwin Paxson derived the new techniques of systems analysis from warfare analysis and game theory to work out just such questions. However, when RAND became established as a source of independent civilian policy analysis, Paxson's comprehensive studies did not integrate easily into the work of the Air Force's and its contractors' own workshops, even as its strongest recommendations ran against the Air Force's policy preference for jet engines.

Ultimately, RAND ameliorated its problems with the Air Force by renegotiating its intellectual and institutional relationship with it. By the mid-1950s systems analysis had come to focus on policy instead of detailed design specifications, turning RAND analysts' attentions toward the development of policies that they figured would remain robust under broad sets of arbitrary assumptions. These new systems analyses served more as informed policy guides than as specific prescriptions, and RAND even initiated a series of short courses on the "appreciation" of its systems analyses, so that Air Force planners could learn how RAND analyses were and were not supposed to be used to inform planning. By positing robust policy frameworks, systems analysis came to resemble military OR, which traditionally focused on policy rather than design. On account of its staff of civilian analysts, RAND itself was already closely associated with the military's OR groups.

However, RAND was also at least partially responsible for bringing OR into the more mathematically sophisticated tradition of systems analysis. Because systems analyses had always incorporated logistical as well as operational elements into their evaluations of equipment designs, they hired mathematicians such as George Dantzig to study logistical problems. Within the services, logistical problems were quite separate from operational problems, but when proponents of moving OR into industry began to escalate their efforts in the late 1940s and early 1950s, the study of logistical problems such as inventory control became a central feature of industrial OR. On account of RAND's association with OR, and the relationship between wartime OR practitioners and respected mathematicians such as Samuel Wilks, RAND's mathematicians also began to associate their work with the OR profession and soon began to publish in its journals. Thus Dantzig's linear programming technique, although initially completely removed from OR, became a major component of OR's mathematical canon, and classes of OR models, such as those on inventory control, supported a thriving link with the nascent field of decision theory, and fostered contact with high level theoreticians of social behavior such as Kenneth Arrow and Herbert Simon.

In many ways, the development of a mathematical canon was a boon to the professionalization of OR. Because industrial management was a fairly well-developed activity, and because consultants already filled the role of independent analyst, mathematical sophistication gave OR an important edge in making contributions to business policies. However, it also tended to remove OR intellectually from the immediate problems of policy, and forced it toward the more abstract problems of mathematical modeling. Once OR established its own intellectual content rather than relying on analysis of the intellectual theories generated by policymakers, it developed practitioner and theoretical wings. Although there was some idea that OR theory and practice might be separated professionally between OR and what was called "management science", in reality the tensions created by the divide were negotiated far more locally. I have already noted how Russell Ackoff, with his mentor West Churchman, viewed managerial problems as a whole as a problem for scientific methodology, and how consultants at Arthur D. Little incorporated OR into a larger management consulting activity. In addition, I have shown how the teaching of OR at Ackoff and Churchman's program at the Case Institute of Technology, and at the Massachusetts Institute of Technology emphasized application as strongly as mathematical methodology. Dissertation advising and course content stressed using models in real policy situations, the OR Group at Case actually performed consulting work, and both institutes held summer programs to build liaison with managers. These efforts were, in large part, built from the veteran emphasis on applicability, but, in a history extending beyond the time frame of this study, became established professionally in prizes awarded for important applications, and the foundation of a journal dedicated to OR in practice, appropriately called *Interfaces*.

It is, I argue, the interfaces that matter most in the history of operations research. Once we have understood their nature, we can begin to understand why OR was considered important, by Strangeloves, Blimps, and, most importantly, by all of the real characters who exist somewhere in between them. It is not easy. As the epigraph to this conclusion shows, fifteen years into RAND's existence, John Williams could scarcely grasp the mechanisms by which RAND had any impact at all, even though, at the same time, its impact seemed pervasive. His statement that "things may at least happen sooner when we start the discussion early, at a level of discourse that requires attention and thought by others" is, I believe, a clue leading to a larger problem of understanding the flow of ideas between communities who understood their work as somehow related, but who remained intellectually separate.

Policies beget policies, practices beget practices, and theories beget theories, but different intellectual realms do intersect, in some contexts regularly, and in some contexts rarely; in some cases quite directly, but more obliquely in others. The transition of knowledge between different social groups is hardly seamless, but the fact that there even is a seam marking their interaction frequently escapes notice.[10] Instead, historians have tended to resort to hoary ideas related to supposedly pure knowledge and its problematic

[10] Readers might find Peter Galison's discussion of the "intercalation" of theory, experiment, and instrumentation useful here; see the conclusion of Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997). But, whereas Galison posits a replacement for a straightforward intellectual relationship between experiment and theory offered by positivists and, in reverse order, by anti-positivists; in the case of policy and science, the two realms are considered to operate in totally separate realms of fact and value, with any interaction between them representing the intermingling of knowledge and power, not knowledge production.

applications to real problems, and about patrons financing experts who back their policies through the application of a scientific veneer to the inevitably political.[11]

I suggest instead that we look to the exchange of insights. The history of OR is replete with them: between designers of technology and designers of tactics; between doctrine builders, intelligence analysts, technical specialists, and field personnel; between independent civilian analysts and military planners; between RAND, Air Force procurement officials, and aircraft designers; between mathematicians and OR proponents; between OR theorists and OR practitioners; between OR theorists and decision theorists; between OR practitioners and industrial policymakers; between operations researchers and systems analysts; and so on.

Unfortunately, historians do not yet have a good sense of how insights are exchanged in the mutual pursuit of robust theories and policies. We do not even have a good sense of what robustness means, except that it is something that is recognized through a process of social agreement. However, scholars of scientific activity have tended to use the socially constructed nature of science more as a weapon against some long dead straw man representing the supposed context-independence of knowledge, and have not often studied what it is about scientific knowledge, or really any knowledge, that fosters agreement between individuals and groups willing to be persuaded of points made by others. The preoccupations of these scholars have deep roots in their concerns about the relationship between knowledge and polity, and before bringing this

[11] Robert K. Merton, "The Role of Applied Social Science in the Formation of Policy: A Research Memorandum," *Philosophy of Science* **16** (1949): 161-181 suggested the need for research to come to a deeper understanding about just these issues. It is now déclassé to cite Merton on account of the perceived primitiveness of his sociology of science compared to the SSK program,, but I think the paper stands up well.

dissertation to a close, it will make sense to offer a survey of these preoccupations in order to suggest new and compelling ways forward.

Notes on Methodology and Historiography

In the introduction to this dissertation, I suggested that David Edgerton's concept of "inverted Whig" historiography has shaped the stories that we tell about the relationship between science and polity, and that these stories vary far too starkly depending on whether one is working within the British or the American historiography. Further, I have argued that an overabundance of historiographical attention to the tensions between science and policymaking aimed at illustrating how problematic it is to consider them as operating in separate realms of facts and values has only served to underline *historical* distinctions between them. Historiographical priorities have systematically diverted attention away from the many strategies employed to treat science and policymaking as engaged in related rational tasks, because the primary concern has been to show just how value-filled science actually is.

The central problem driving the historiography is the idea of science as a means of producing truth or "matters of fact" rather than insights. The idea of inquiring into the *social* processes by which a matter of fact is produced dates back to the origins of the sociology of scientific knowledge (SSK) program in the 1970s.[12] However, the most influential text in this tradition is clearly Steven Shapin and Simon Schaffer's 1985 work *Leviathan and the Air-Pump*, which explores the relationship between experimental results and their perceived legitimacy. They argue that the ascendance of the

[12] The social dimension of science had, of course, been long recognized, but the more specific emphasis on the social nature of the validated matter of fact was the quarry (or perhaps the red herring) of SSK.

seventeenth-century Royal Society's experimental program over deductive philosophy was not predetermined by any sort of innate epistemological superiority offered by experiment. In reality, the experimental program involved assembling a society of disinterested gentlemen to witness experiments and to vouch for the results. The philosopher Thomas Hobbes opposed the validity of such a program, because he felt it was politically dangerous to allow knowledge to rest on social agreement. Instead, to ensure order, knowledge like power was supposed to be non-contestable, which meant that only philosophy could be relied upon to offer factual pronouncements. As Shapin and Schaffer point out, Hobbes' critique was on target, and was never answered by Robert Boyle or any other advocate for experiment.[13]

Subsequent discussions of the relationship between knowledge and political power have followed a similar line of reasoning in exploring the consequences of the fact that scientific knowledge is rooted in social agreement. In particular, an overwhelming burden has been placed on the study of scientific controversies. Since controversies can only be quelled through agreement, and if science is essentially only another form of agreement, then clearly science cannot be an answer to political controversy, or stand separate from politics. In studies of twentieth-century science and technology, Donald MacKenzie's research on the perceived adequacy of knowledge about nuclear missile accuracy rising and falling with political tides, and Sheila Jasanoff's many studies of the use of scientific results in government regulation and as evidence in the courtroom, are

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[13] See Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985); see also Steven Shapin, *The Social History of Truth: A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994).

premier examples of this argument.[14] Their conclusions are surely correct—the ability of science to settle political controversies definitively is limited by politics, and scientists do well to recognize this fact—but we should ask to what extent addressing the question of science and politics through the study of controversy is like looking at the tip of an iceberg.

Historians have been interested in studying scientific controversy because it so visibly destroys illusions that scientific research proceeds along lines generated exclusively by the intellectual path of their work free from political interference. If science cannot stand independently from its cultural context, scholars can usefully examine just how controversies are resolved or avoided through the negotiation of the contents of research programs within their social contexts. Crucially, the study of controversy indicates that if in many cases science has not been controversial, it very well might have been had cultural and political conditions been different, which means that matters of politics can be hidden within accepted scientific truths.[15]

Typically, scholars will point out two avenues through which politics invade even the most banal research. First, the selection of research topics may be influenced by external factors, most often available patronage, impacting scientists' ability to conduct

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[14] Donald MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge, Mass.: The MIT Press, 1990); Sheila Jasanoff, *The Fifth Branch: Science Advisers as Policymakers* (Cambridge, Mass.: Harvard University Press, 1990); and Sheila Jasanoff, *Science at the Bar: Law, Science, and Technology in America* (Cambridge, Mass.: Harvard University Press, 1995).

[15] Steve Fuller, *Thomas Kuhn: A Philosophical History of Our Times* (Chicago: University of Chicago Press, 2000) has tracked the idea that *no* kind of science is actually illegitimate back to Thomas Kuhn's "paradigmatic" model of science, which he argues was part and parcel of an unquestioning Cold War acquiescence toward the scientific enterprise. Fuller would like to see a more "Tory" history of science that is willing to criticize past advances. He points to Mirowski as an example of such a bold scholar. However, see Mirowski's insightful discussion of Fuller, Philip Mirowski, "What's Kuhn Got to Do With It?" [2001] in *idem*, *The Effortless Economy of Science?* (Durham: Duke University Press, 2004). Incidentally, one might also inquire as to the difference between a Tory and an Inverted Whig.

research on any given topic.[16] Second, a more subtle array of influences are seen making their mark on how scientists actually construct facts from evidence, and knowledge from facts. A particularly vibrant branch of the history of science has shown how scientists employ discourses composed of philosophical ideas, metaphorical language and visual representation in their work. Often these arguments show how science shares its discourse with the political and social culture surrounding it, and hence cannot make a claim to pure, objective rationality.[17] Most clearly, medicine and psychology have been seen as reinforcing socially-determined concepts such as "normality", "sexuality", "health" and "intelligence," but other scholars have been eager to extend discursive arguments to areas of expert policymaking much closer to OR. Historians drawing attention to human-machine "cyborg" sciences have noted how they are marked by military patronage and computer metaphors. Paul Edwards, in particular, has argued that the use of computers (or computer metaphors) to do everything from coordinate warfare to model the brain in mid-twentieth-century America was not only consonant materially with the economic interests of the military-industrial complex, but consonant discursively with the "closed world" discourse inhabiting the bipolar political order of capitalism

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[16] Key works bearing on the era in question are Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *HSPS* **18** (1987): 149-229; and Stuart Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993). One might, however, just as easily go back to J. D. Bernal, *The Social Function of Science* (London: G. Routledge and Sons, 1939), or even earlier.
[17] From the science studies tradition, see the classic Paul Forman, "Weimar Culture, Causality and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment," *HSPS* **3** (1971): 1-115. From the critical theory tradition, Michel Foucault has been especially influential; see especially Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences* [1970] (Chicago: University of Chicago Press, 1971).

versus communism and the frightening logic of such strategic doctrines as mutually assured destruction and the domino theory of nations.[18]

Although historians such as Edwards have been careful not to claim that the use of such metaphors somehow makes for illegitimate science, it is not difficult to detect an underlying critique of the *limitations* of scientific inquiry on account of the innate limits of its operative metaphors, which are shaped by its material and discursive surroundings. Effectively, this line of argument boils down to an injunction not to take the validity of dominant scientific traditions for granted, and to question the politics of their inevitably artificial origins. The point is certainly fair enough, but it leaves the acceptance of dominant scientific programs as an issue of political power: scientific ideas are accepted because their proponents are backed by funding, and because they resonate with a dominant discourses. Yet, we have no other sense of why ideas might prove compelling. Because we are so intent on showing how scientific models *can* lead to controversy and policy failure, we do not know what it is that makes them powerful and capable of fostering agreement.

The question might be framed as a matter of why anybody agrees on anything in politics or in science. As I pointed out in the introduction, all policies are, to some extent, based on some rationale: politics is rational. Yet, when one speaks of rationality in the context of politics, and especially in the context of science and politics, for some reason one is inclined to speak of rationality in some absolute sense, and thus to proclaim politics inevitably irrational. Thus, a strange thing happens in historical accounts: policymaking is stripped of its rationality altogether, and this rationality is given over to a

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[18] Paul Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, Mass.: The MIT Press, 1996).

science that promises to make objective truth claims for their patrons the politicians. Historians of science proclaim the impossibility of such a notion, and, our work being done, we head home, safe in the knowledge that society still needs us. Meanwhile, the policymakers are left behind. Nobody has bothered to restore their rationality to them, because it has been sufficient to view them merely as a source of patronage, and therefore human values, which science cannot escape. They are non-scientific, and thus not of interest to historians of science.[19]

At this point many scholars might recognize the parallels between the way historians of science treat society's supposed perception of the science-politics relationship and certain strands of feminist theory, with science clearly cast in the rationalizing masculine role and politics in the more naturalistic feminine role. Sure enough, we find a familiar critical strategy: science studies informs us it is not the purportedly pure (masculine) science that should be protected against impure (feminine) politics. Rather, it is a story of science dominating politics, dispossessing political and social classes whose more intuitive knowledge does not fit into "objective" science's proclamations. However, among various poststructuralist feminist critiques of the gendered identification of artificial rationality and natural intuition, we should especially remember Donna Haraway's "Manifesto for Cyborgs". Using the post-World War II scientific culture as a springboard, she argues that not only are we all, male and female, currently conglomerations of the rational and the natural (i.e., cyborgs), but that it makes no sense to argue nostalgically for a past that probably never was. Her critique declares

[19] Science studies scholars have, however, made a small industry of restoring the rationality to forms of knowledge seen as displaced by scientific inquiry: astrology, alchemy, herb lore, midwifery, and so forth. For some reason, studies of areas of knowledge not seen as having been displaced by science, such as policymaking or management, have not attracted similar attention.

that we should accept our status as cyborgs, and move beyond the military connotations of cybernetics[20]. Ironically, not only has science studies never considered the corresponding argument that politics is rational with or without the intervention of science, scholars such as Paul Edwards and Andy Pickering have actually seized on Haraway's use of the term "cyborg" to repeat the familiar critical arguments about the alliance of science and politics, pointing to the rise of a "cyborg discourse" and "World War II regime" that was not there previously.[21]

Instead of resorting to cyborg discourses, I have preferred to discuss politics and science in terms of rationality and arbitrariness, rather than in terms of rationality and intuition, or rationality and values. In fact, I would go so far as to say that science itself operates along lines similar to what we have discussed in terms of policy knowledge. If recent science studies has taught us nothing else, it is that science is also full of speculation, simplistic models, estimates and outright guesses. Thus, when we discuss scientists pressing for policies to be made more scientifically, or more rationally, it should not be taken to mean they advocated for the rationalization of politics by introducing the epistemology of the universities, but rather that they advocated for a more rigorous use of the political standards of rationality already in place.

What we historians of science should now begin to ask ourselves when we are discussing a mix of arbitrariness and rationality in *either* science or policymaking is whether we are discussing the *arbitrarily rational* (as SSK would have it) or the *rationally arbitrary*. The question has especially significant implications for studying the

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[20] Donna Haraway, "A Cyborg Manifesto: Science, Technology, and Socialist-Feminism in the Late Twentieth Century," [1985] in *idem, Simians, Cyborgs and Women: The Reinvention of Nature* (New York: Routledge, 1991), pp. 149-181.

[21] Edwards, *Closed World*; and Andy Pickering, "Cyborg History and the World War II Regime," *Perspectives on Science* **3** (1995): 1-48.

relationship between science and polity. The first phrase tells us that what we take to have authority on account of its purported rationality may not, in fact, be entitled to such authority because it is actually the product of an arbitrary agreement. It should be questioned vigorously, and the roots of the social agreements surrounding it probed. The second phrase tells us that what is enforced by means of political authority is being justified by a clearly-stated rationale. It does not mean that the rationale is necessarily correct, validated, or even complete, but it does mean that those subject to the policy are assured that the policy has a motivation consonant with general interests it purports to represent, it tells explicitly whether the policy is intended to benefit certain groups more than others, and it lays the grounds for future debates over whether the rationale is just, correct, or if it might be improved in either respect. It also holds the makers of the policy responsible for consulting experts to ensure the integrity of the rationale, and holds them responsible for the more arbitrary elements of the rationale.[22]

The idea of the arbitrarily rational is consonant with common critiques of an Enlightenment view of polity, which holds that political action should be taken based on an appeal to universal truths. In such a polity, science as the recognized producer of truths is relied upon to endow politics with the authority of knowledge. Ted Porter's work on the use of quantification to build political trust through claims to objectivity, and the limitations of these means is exemplary of this tradition. Similarly, Ken Alder's work on the foundations of the metric system, and the painstaking efforts to create a measure

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[22] For an intriguing contrast with the American case, see Peter Caldwell, *Dictatorship, State Planning, and Social Theory in the German Democratic Republic* (New York: Cambridge University Press, 2003) on the German Democratic Republic's party leadership's obsession with scientific socialist theory that eventually degraded into discredited official party pronouncements; pay particular note to chapter four on cybernetics. On this note, also see Slava Gerovitch, *From Newspeak to Cyberspeak: A History of Soviet Cybernetics* (Cambridge, Mass.: The MIT Press, 2002).

for all people in all times by precisely measuring the distance from Barcelona to Dunkirk is a potent reminder of the ends that one might take to dehumanize polity. As Alder points out, however, choices of what constitutes a rational measure inevitably involve arbitrary choice, and should not require going to such lengths.[23]

Such scholars as Yaron Ezrahi, Bruno Latour, and Sheila Jasanoff have continually questioned the sustainability of holding an Enlightenment view of polity in light of what Ezrahi refers to as the late-twentieth century decline in the authority of scientific representation. Latour, even declares it is time to formulate a new constitution between science and politics that does not insist upon their separation.[24] I tend to take a far less dramatic view of things simply because I do not feel we are at any late-twentieth (or, now, early-twenty-first) century moment of crisis, and my reason largely has to do with a lack of belief in the catechism of science studies: the historical dominance of an Enlightenment view of polity, what Latour considers to be the old constitution. The title of this dissertation declares that it is an examination of "Anglo-American" scientific cultures. While my use of the term refers mostly to the transatlantic history of OR, it becomes especially pertinent here at the end, because the Anglo-American political experience, while still based upon reason rather than notions of natural authority, does not precisely replicate the French Enlightenment notion of polity on which so many

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[23] Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton: Princeton University Press, 1995). Ken Alder, *The Measure of All Things: The Seven-Year Odyssey and Hidden Error That Transformed the World* (New York: Free Press, 2002); also see Ken Alder, "A Revolution to Measure: The Political Economy of the Metric System in France," in *The Values of Precision*, edited by M. Norton Wise, (Princeton: Princeton University Press, 1997), pp. 39-71.

[24] Yaron Ezrahi, *The Descent of Icarus: Science and the Transformation of Contemporary Democracy* (Cambridge, Mass.: Harvard University Press, 1990); Jasanoff, *Fifth Branch*; Jasanoff; *Science at the Bar*; Bruno Latour, *We Have Never Been Modern*, translated by Catherine Porter (Cambridge, Mass.: Harvard University Press, 1993); Bruno Latour, *The Politics of Nature: How to Bring the Sciences into Democracy*, translated by Catherine Porter (Cambridge, Mass.: Harvard University Press, 2004).

critiques of the limitations of the objectivity of science are based. Anglo-American polity is rationally arbitrary.[25]

In an Enlightenment view of polity, objective viewpoints can be called upon to demonstrate a correct course of action. On the other hand, in the Anglo-American view of polity, demonstrable correctness is not so much the issue as is demonstrable superiority. The burden is shifted away from authority onto the critic to demonstrate not only the flaws or the limitations of the system at it stands, but how an alternative course would be better. In the event that no option can be said to be demonstrably superior, an arbitrary choice between the best options may legitimately be made by the legally appointed authority. Under this system, policymakers have strong incentives to eliminate arbitrary assumptions in their policies whenever circumstances permit, because citizens give their allegiance to a government, or stockholders put their faith in management in the expectation that the policymakers will make better use of available advice than readily-obtainable alternative governments and managers.[26]

In non-controversial circumstances, policymakers will tend to make use of advice, scientific or otherwise, because, on the balance, rationally considered policies will have better results than wholly arbitrary ones. Policymakers can find no better way to gain the public's faith than by fulfilling the rationales behind their policies. On points where advisors disagree, however, policymakers must choose arbitrarily between expert

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[25] Philip Mirowski, *More Heat Than Light: Economics as Social Physics: Physics as Nature's Economics* (New York: Cambridge University Press, 1989), chapter 7, esp. note 1, seems to concur that the distinction of an Anglo-American notion of polity from Continental versions matters in determining attitudes toward "scientism".

[26] In the case of the military, soldiers put their faith in officers' use of rational plans in the knowledge that they stand the best chance of survival. Soldiers do not have the authority to question their orders, but morale, I would argue, is dependent on the perceived merit of the orders soldiers receive. Literary and cinematic references abound. I would recommend Joseph Heller's *Catch-22* and Stanley Kubrick's film *Paths of Glory* as exemplars.

opinions, and may do so because the legal nature of their authority permits them to act arbitrarily, by guess or (more palatably) by "principle", when no clear rationale for policy exists.[27] In all cases of action, policymakers will not be held accountable for not solving problems, which, it is presumed, no one else could have solved except by chance.[28] Thus, in politically controversial matters, politicians actually have an incentive to exploit controversy among experts, or even to create the illusion of controversy, because it reduces the burden of responsibility they might carry for taking politically convenient action that they can claim was necessarily arbitrary. Of course, in certain controversial circumstances, particularly those where pertinent information is subject to secrecy restrictions, politicians may be in a position to stifle expert controversy, or even to mask agreement contrary to policy choice, and present the illusion of united expert opinion to legitimate their policies, but they do so at the peril of policy failure, exposure, and accusations of dishonesty.

These views do not differ markedly from Yaron Ezrahi's ideas about the relationship between polity and science, which he spells out using the concepts of "pragmatic rationalism" and "utopian rationalism".[29] His notions of pragmatic rationalism map rather nicely onto the concept of Anglo-American polity deployed here,

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[27] Of course, lack of action is as much a policy choice as action. Famously, many economists have strenuously argued that the inherent virtues of market forces provide a sound, or at least arguable rationale for lack of government action in most circumstances. Incidentally, the need to justify necessarily arbitrary action in some way does seem to suggest a lingering notion of Enlightenment polity along the lines Ezrahi suggests, but I would also suggest that the idea of confidence in leadership is still more important than rationalization of policy in Anglo-American politics.

[28] In Federalist paper 63, James Madison argued that frequent elections ensured "a due responsibility in the government of the people," and observed that "responsibility, in order to be reasonable, must be limited to objects within the power of the responsible party." Thomas Haskell quotes Madison here as the first ever instance of the use of the word "responsibility," which is a concept he endows with particular intellectual significance. Thomas L. Haskell, "Responsibility, Convention, and the Role of Ideas in History," in *idem*, *Objectivity Is Not Neutrality: Explanatory Schemes in History* (Baltimore: Johns Hopkins University Press, 1998), pp. 280-306, on p. 282. The volume, a collection of Haskell's essays, is well worthwhile.

[29] Yaron Ezrahi, "Utopian and Pragmatic Rationalism: The Political Context of Scientific Advice," *Minerva* **18** (1980): 111-131.

and he uses much the same language of arbitrariness and rationality that I do. But Ezrahi seems to style himself as an advocate for pragmatic rationalism, rather than an historian thereof, and he argues that utopian rationalism, which he believes has traditionally been dominant, is now in decline. He argues that this descent has created the danger of the rejection of rationalism altogether. I reject this view on empirical grounds. In general, Ezrahi does not rely heavily on empiricism, and where he does, he tends to share the usual science studies preoccupation with controversy studies.[30]

Looking back to that part of the iceberg below the surface, we must concede that, yes, any scientific result *could* have been controversial, but there are many, many cases in which there is actually no real reason for science or policy to be controversial, because disputes revolve primarily around uncertainty rather than conflicts of interest. In these cases, which I hypothesize constitute a large majority, factions which hold differing inclinations will be willing to be persuaded through demonstration of an alternative point of view, if for no other reason than out of fear of being demonstrably wrong. Science, politics, and science and politics in tandem, I would claim, all tend to operate based on this principle. This dissertation should have amply demonstrated that in every instance where OR thrived, whether in military planning, in academic contests over abstract theories, or in the application of models to industrial policies, it did so because it was able to bring something new to existing debates and foster (not dictate) consensus, not because

[30] Ezrahi, *Descent*, p. 239 admits, "Any attempt to discern or explain general trends in the relations between science, technology, liberal-democratic ideology, and politics in late-twentieth century America must remain for the time being largely speculative. Combine with the inherent complexity of the subject, the absence of an historical perspective limits our ability to interpret recent experience and distinguish significant from marginal developments." My suspicion is that Ezrahi, in subscribing to the controversy studies viewpoint, has locked in on marginal developments, and ignores the more general fabric of polity, especially in his more recent writings. In Ezrahi, "Utopian and Pragmatic Rationalism," he uses a noted controversy from the history of OR over Anti-Ballistic Missile technology, which I intend to address in future work.

its results were scientific, but because it was able to bring stronger, more nuanced rationales to policymaking mechanisms.

As I have argued repeatedly, what is useful about science, in general, and the policy sciences and OR, in particular, is not that they provide a clear basis for action, but that they create a language that permits policy knowledge to become sharper by solidifying what is understood, isolating what is controversial on account of ignorance, and more clearly defining what is controversial on account of differences in values, thereby setting the table for bargaining. When the time comes to make a policy, no scientific result will ever guarantee that it is the correct decision to be made. Rather, it creates a framework wherein one can more easily identify what is wrong—remember Curtis LeMay's attitude toward doctrine. By solidifying rationales through investigation, these rationales are not so much endowed with authority as they are placed in the open as the *sine qua non* of authority. They are a gauntlet thrown down to critics as a challenge to demonstrate how they might be mistaken, and what stronger claim to authority might exist. It is, remarkably, just the same with the theoretical side of OR and the other policy sciences. A mathematical theory is not deemed correct because of its formalized nature. It is recognized at the outset that it is surely wrong in some sense, but it, too, is a gauntlet thrown down to others demanding that they demonstrate how it is wrong.[31] Occasionally, these worlds of policy and mathematical theory might even have some relevance to each other, and it is in these spaces that OR anchored its postwar stability.

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[31] The discussion in S. M. Amadae, *Rationalizing Capitalist Democracy: The Cold War Origins of Rational Choice Liberalism* (Chicago: University of Chicago Press, 2003) of theoretical developments in political science stemming from Kenneth Arrow's impossibility theorem is an extremely enlightening discussion of this process.

Even as controversies seem to proliferate, the terms of the debate become defined with greater clarity, and the nature of the problems being pursued by experts becomes increasingly challenging. This is not a threat to polity. If we often seem to be running in place or even retreating, if policy experts multiply and disagree, I do not think these are signs of the limitations of policymakers or policy experts, so much as it is a reminder of how much we need their debates to keep our thinking fresh in an increasingly complex world.[32] The more we discuss, debate, and berate each other about the rationales behind policies, the more ways we find of talking about them, the better defined the terms of the debate become, the better off we will be. Although we will surely deny it, hopefully we will always listen to the arguments of our opponents and appropriate whatever of their insights we find expedient to our own, and in so doing bring our political and scientific discourses into new territories.

[32] Brian Balogh, *Chain Reaction: Expert Debate and Public Participation in American Commercial Nuclear Power, 1945-1975* (New York: Cambridge University Press, 1991) waxes similarly optimistic, although I tend to disagree with his emphasis on controversy as the breeding ground of expert culture.

Appendix A: A Short Essay on Philip Mirowski

Speaking of opponents, although I have made some references to him, I have found myself continually pushing an inevitably esoteric discussion of Philip Mirowski back from the introduction, into the chapters, to the conclusion, and ultimately out the back of the dissertation, and so it has landed here. Because Mirowski is one of the most creative scholars working in and around science studies today, and because of his recent references to operations research in relationship to his work on neoclassical economics and the methodology of science studies, I would be remiss if I did not discuss his work in at least some detail,

Although Mirowski addresses many audiences, much of his work, and particularly his sprawling 2002 book *Machine Dreams: Economics Becomes a Cyborg Science*, serves as a critique of recent developments in neoclassical economics that base game theoretic models of market functions on the foundation of information processing rational agents who compute and maximize the utility of their choices on the marketplace based upon a fixed set of preferences.[33] A key to Mirowski's argument is the idea of OR as a free-standing theory of rational behavior—what he terms "American" OR—with a special emphasis on its embrace of game theory and the mathematically-related linear programming. According to Mirowski, mid-twentieth century convergences between economics and OR (to which the current trends can be traced) are themselves a remnant of the work of the nineteenth century French economist Leon Walras, who posited a "scientific" (which, he explicitly stated, meant philosophically coherent) version of

[33] Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (New York: Cambridge University Press, 2002); the most relevant chapters of his book to OR are 4 to 6.

economics, in which he envisioned the market as a great auction calculating optimal allocations of goods. Recall from chapter four, when George Dantzig developed his linear programming technique, that the Cowles Commission economist Tjalling Koopmans took it up as a new and powerful way of formulating economic ideas. As Mirowski relates the story, when Dantzig came to Koopmans seeking an algorithm to solve a linear programming formulation, Koopmans was not interested in actually solving the programs, for "he saw only Walras."[34] Theoretical economists do not calculate; they model. The point is certainly fair enough. The difficulty lies in the legitimacy of this act.

In Mirowski's reckoning the event was unfortunate, because it represented a means Koopmans and the Cowles Commission could use to perpetuate an intellectually bankrupt Walrasian economics program. Although Mirowski believes that Walras' methodology was never exactly above suspicion, following Kurt Gödel's famous proof, which entailed that rationality could not be defined in any fundamental and self-referential way, it should have been absolutely clear to Koopmans and his collaborators from the outset that any model making exercise along those lines could not have been anything but vacuous. Mirowski derides such detached mathematical model-making as "Bourbakism".[35] He notes, "Because Koopmans had avoided algorithmic questions as beyond the pale, waxed agnostic on questions epistemological, and showed no interest in any historical market structure, [his portrayal of the "neoclassical agent"] was based on little more than the sheerest bravado." Indeed, he continues, "Economists of all stripes, above all else, should have been wary of the smell of snake oil and the whining of

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[34] *Ibid.*, p. 258.
[35] *Ibid.*, pp. 390-394. The term derives from the French structuralist mathematics movement, "Bourbaki", which, Mirowski argues, heavily influenced the economists at Cowles. On Walras, see Philip Mirowski, *More Heat Than Light: Economics as Social Physics: Physics as Nature's Economics* (New York: Cambridge University Press, 1989).

perpetual motion machines that surrounded this early approach to 'information' at Cowles…" But they were not: "bedazzled and in awe of the nascent computer, no one saw fit to complain."[36] The idea that the market's internal calculus could be explicitly formulated might have smacked of prior socialist ambitions to do the same, but, Mirowski argues, the program was protected by its close relationship to the use of linear programs and game theory being developed for the military in the guise of OR and systems analysis, and so received lavish military funding, and so survived.[37]

The connection to "American" operations research and decision theory was direct, and Mirowski is quite correct to point it out. At the same time as the Cowles program was being formulated, OR was taking its turn toward building a mathematical canon by adopting the abstract reasoning of subjects such as inventory theory. Some of the same characters, most notably Kenneth Arrow, were involved in both the Cowles program and in the new mathematics of OR. However, Mirowski has never gone so far as to examine the mathematics of OR in detail (inventory theory receives no mention), and he certainly has made no efforts to examine the institutional arrangements that OR was developing to regulate the intellectual exchanges between theorists and practitioners. For Mirowski an operations researcher is an operations researcher, which, for him, is a term that simply connotes a bogeyman responsible for bringing the rationalism of anti-aircraft gunnery algorithms and linear programming solutions simultaneously to military problems and to neoclassical economics.

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[36] *Ibid.*, p. 259.
[37] *Ibid.*, pp. 255-256; see also Philip Mirowski, "The Scientific Dimensions of Social Knowledge and Their Distant echoes in 20th-Century American Philosophy of Science," *Studies in the History and Philosophy of Science* **35** (2004): 283-326, on p. 308. Mirowski has made similar statements in several sources, and so here I will employ the version that seems to convey his argument the clearest.

Because Mirowski needs a source of rationalistic thinking, and because RAND is a recognized source of just such thinking, RAND becomes *the* source of OR thinking. However, according to my argument in chapters four and five, RAND would not have considered itself properly in the OR business until at least 1953, when it began to become clear that industrial OR was going to be characterized mathematical optimizations: four years into the rapidly proliferating Cowles program. This span of time is crucial, because for Mirowski it is not just the mathematics of OR that is important, but its *status* as a scientific intervention in military policy. For Mirowski, the status of RAND as a rationalistic intervener in military policy is directly linked to wartime OR. Mirowski has argued repeatedly that it was the wartime operations researchers who originally

> believed that they were far smarter than your average lieutenant colonel, and should be allowed to run things as they saw fit; in effect, they wanted to exist simultaneously *within* but *apart from* the military chain of command. They wanted to be paid by the military but not really be in the military; as physicists, they wanted to do social research for the military but not become confused with social scientists; they wanted to tell others what to do, but not be held responsible for the commands given. After the war, they wanted to return to their university posts without having to relinquish their military ties. To be granted these extraordinary dispensations, they had to innovate new roles that embodied this delicate amalgam of engagement and aloofness.[38]

This view contrasts markedly with my view of wartime OR scientists as people who learned about military policymaking, and worked within its logical structures. On the contrary, for Mirowski, "The construct of the 'operations researcher' was the professional device which fostered the reconciliation of these conflicting demands,"[39] and this device worked because of its access to a "generic expertise in models of command and control," which served operations researchers' "arrogation of special status for themselves as

[38] Mirowski, "Scientific Dimensions," p. 300; but Mirowski has rehearsed some version these lines on at least four occasions overall. They are clearly central to his thinking on the matter. The first instance was in Philip Mirowski, "Cyborg Agonistes: Economics Meets Operations Research in Mid-Century," *Social Studies of Science* **29** (1999): 685-718, p. 690.
[39] Mirowski, "Scientific Dimensions," p. 300.

scientists situated beyond the reach of conventional social hierarchies and external demands to make their own activities accountable."[40]

Operations researchers' ability to formulate the mathematics for an effective fire control device or an efficient allocation of resources, and the claims they could make to scientific objectivity, gave them the authority to apply their models more broadly—notably in economics. Mirowski claims:

> OR was the anvil upon which the postwar relationship between scientists and the American state was hammered out; once successful, the blade was then turned to carve out a new model of society that could be amenable to the rapprochement of science and the military. OR provided much of the metallic durability and intellectual firepower for postwar American social sciences such as decision theory, organization theory, management theory, and neoclassical economics...[41]

In Mirowski's "American" OR there is never a time when the growing mathematical canon of OR threatens to distance OR from policymaking, because, according to him, it was always the mathematics that gave OR its impersonal authority. The rise of mathematics, in his view, was an unquestioned source of scientific authority. This authority was transferred directly from OR to the RAND Corporation, and then transferred through the relevant mathematics to the neoclassical economic program of the Cowles Commission. Never mind that RAND was primarily involved in design work at the time, with some economics on the side. Mirowski's argument, in short, is this: if efficient machines and command-and-control policies can be built through mathematics, then it was thought to be unproblematic to use these same mathematics to describe human economic behavior, hence: "machine dreams".

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[40] Philip Mirowski, "Introduction: Cracks, Hidden Passageways, and False Bottoms: The Economics of Science and Social Studies of Economics," in *idem*, *The Effortless Economy of Science?* (Durham: Duke University Press, 2004), p. 17.
[41] Mirowski, "Scientific Dimensions," p. 301.

It is at this point that Mirowski's lack of engagement with OR brings difficulties. Efficient policies were never built *through* mathematics, but, at most, with the *aid* of mathematics. During World War II mathematics played barely any role in OR at all. OR was actually the means through which mathematical equipment designs (which Mirowski conflates with "American" OR) could be compared to operational realities. Later, when OR actually did become mathematical, it was more through the transfer of insights between OR theories and OR practice than any rote application of a theory that made policies more effective. By separating mathematical American OR from rough and ready "British" OR, Mirowski discounts the intellectual and institutional processes through which theory and practice communicated with each other and with policies as a source of OR's legitimacy in America as well as in Britain. If one concentrates one's view entirely on the self-referential mathematical theories of OR, one misses crucial elements of the identity of the operations research practitioner.

Of course, none of this discussion of OR necessarily negates Mirowski's point about *economics*. After all, while he indicts the Cowles program as a "machine dream," he does admit that "dreams can sometimes be salutary, so long as they are never confused with conscious reality."[42] Here is my point about OR theory and OR practice. OR theory was salutary precisely because it was not confused with actual policies, but Mirowski never recognizes this point—only the British could be so concerned. We are thus forced to ask whether the neoclassical economists might not have had similar intellectual and institutional relationships with policymakers such as the ones I have outlined in this

[42] *Machine Dreams*, p. 414.

dissertation in the case of OR.[43] No doubt neoclassical economic theory does not ontologically map onto actual economies in any particularly elegant way, but what we have to ask is what *insights* we (or, more accurately, policy-minded economists) derive from it versus from other theoretical programs. What kinds of knowledge claims do the mathematical theories make? How do they build on or argue with prior knowledge claims? Are the knowledge claims in OR theory different from knowledge claims in decision theory or neoclassical economic theory? In the post-"matter of fact" era in science studies, these are the basic sorts of questions that must fuel further historical inquiry on this topic.

[43] It is especially important to ask the question since Kenneth Arrow, one of Mirowski's neoclassical *bêtes noirs*, was fully aware of the oblique relationship between theory and practice in OR, so surely he would might have felt similarly with respect to the neoclassical program he was building at the exact same time. See Kenneth J. Arrow, "Decision Theory and Operations Research," *Operations Research* **5** (1957): 765-774.

Appendix B: Archival Locations of Some Military OR Reports

THE NATIONAL ARCHIVES OF THE UK: PUBLIC RECORD OFFICE
British Royal Air Force
Bomber Command, TNA: PRO AIR 14 scattered in separate files listed by report title
Coastal Command, TNA: PRO AIR 15/731-734, AVIA 7/1004-1005
Fighter Command, TNA: PRO AIR 16/1043, 1045, 1161, and scattered

British Admiralty
TNA: PRO ADM 219, scattered in separate files listed by report title

British War Office/Army
TNA: PRO WO 291, scattered in separate files listed by report title

British Air Warfare Analysis Section
TNA: PRO AIR 20, scattered

UNITED STATES NATIONAL ARCHIVES AND RECORDS ADMINISTRATION
U. S. Army Operations Research Office (including correspondence re: reports)
RG 319: Records of the Army Staff
See Finding Aid: Volume 10, G-2 thru G-3, tab Declassified G-3/DCSOPS Entry 95 (040, ORO)
A1: 137-A: General Decimal File {Security Classified Correspondence, 1950-55}(Boxes 1-609)
1950-51: (020, ORO) 40-42
1952: (040, AFSA-ORO) 89; (040, ORO) 90-93
1953: (040, Defense-ORO) 12; (040, ORO) 13-21
1954: (040, Defense-ORO) 12; (040, ORO) 13-25
*1955 See A1: 137-B below
1956: (032-040) 13; (040) 14-15; (040-060) 16
1957: (032-040, ORO) 11; (040, ORO-060) 12
1958: (032-040, ORO) 7; (040, ORO-062) 8
1959: (040-091, Australia) 4
A1: 137-B: General Decimal File {Security Classified Correspondence, 1955} (Boxes 2-372)
1955: (040, Defense-ORO) 15-16; (040, ORO-045) 17-26

U. S. Air Force Headquarters, Operations Analysis
RG 341: Records of Headquarters U. S. Air Force (Air Staff)
NM-15: 208: Technical Operations Analysis Reports 1945-57 (Boxes 11-14)

Weapons Systems Evaluation Group (files consulted; large groups remain classified as of 2005)

RG 218: Records of the U. S. Joint Chiefs of Staff

See Finding Aid: 1948-1961 Files

UD: 5: Central Decimal File, 1948-50 (Boxes 1-253)

334 (WSEG) (2-4-48) Establishment of Weapons Systems Evaluation Group Sec. 1-2

384.8 (2-10-50) Sec., BP WSEG Study of Airborne Operations

UD: 10: Central Decimal File, 1951-53 (Boxes 1-179)

322 Opns Research Groups (6-16-50) Sec. Operations Research Group

UD: 16: Central Decimal File, 1954-56 (Boxes 1-149)

334 (WSEG) (2-4-48) Establishment of Weapons Systems Evaluation Group Sec. 3 [in box 29]

UD: 22: Central Decimal File, 1957 (Boxes 1-54)

334 (WSEG) (2-4-48) Establishment of Weapons Systems Evaluation Group Sec. 4-7 [in boxes 29-30]

UD: 28: Central Decimal File, 1958 (Boxes 1-89)

334 (2-4-48) Weapons Systems Evaluation Group Sec. 8-9 [in box 53]

UD: 92: Joint New Weapons Committee, Subject File, May 1942-1945 (Boxes 1-87) *formerly Entry 343

A1: 1-A: Central Decimal File 1959 {Central File} (Boxes 1-153)

3201 Operations Research (30 June 1959)

RG 330: Records of the Office of the Secretary of Defense

See Finding Aid: Entry 341, Research & Development Board (3 volumes)

NM-12: 341-A: Records Concerning Organization, Budget, and the Allocation of Research and Development, 1946-1953 (Boxes 1-609) [Research and Development Board]

Box (Folder) Title [Folder Contents]

505 (1) Air Force Project "RAND"

512 (4) Operations Evaluation Group [studies of, 1947-53]

519 (1) Weapon Systems Evaluation Group

520 (1-3) WSEG Report #7 [Evaluation of the Offensive & Defensive Cap, abilities of fast carrier task forces in 1951].

520 (4) WSEG Report #9 [The Continental Air Defense System, March 1952]

521 (1-3) WSEG Organization

1. Lectures, Handbook, Correspondence between Karl Compton, V. Bush and others, January 1949-January 1931 [sic 1951]
2. Proposed studies, briefings by WSEG Group, etc. April 1949-February 1951
3. Establishment of WSEG, reports, etc., January 1947-December 1945

Appendix C: Abbreviations in Text

Key:
WW2=World War II usage only
PW=Postwar usage only
If the name of a person is given, that person is the only person ever to hold that specific title

AA	Anti-Aircraft
AAES	Anti-Aircraft Experimental Section (UK, Munitions Inventions Department, World War I)
AAF	Army Air Forces (USA, WW2)
AC-92	Contract number for AMP research project on the B-29 bomber
ACSP	Advisory Council on Scientific Policy (UK, PW)
ADRDE	Air Defence Research and Development Establishment (UK, under the Ministry of Supply)
AFDU	Air Fighting Development Unit (UK, RAF Fighter Command)
AirORG	Air Operations Research Group (US Navy, WW2)
AMG-C	Applied Mathematics Group at Columbia University (USA, WW2 under AMP)
AMP	Applied Mathematics Panel (USA, WW2, under NDRC)
AORE	Army Operational Research Establishment (UK)
AORG	Army Operational Research Group (UK)
AScW	Association of Scientific Workers (UK)
ASV	Anti-Surface Vessel (radar)
ASW	Anti-Submarine Warfare
ASWORG	Anti-Submarine Warfare Operations Research Group (US Navy, WW2)
AWA	Air Warfare Analysis
AWAS	Air Warfare Analysis Section (UK)
BAAS	British Association for the Advancement of Science
BBRL	British Branch of the Radiation Laboratory (USA, WW2 under the Rad Lab)
BISRA	British Iron and Steel Research Association (PW)
BDU	Bomber/Bombing Development Unit (UK, RAF Bomber Command)
CAOR	Chief Adviser on Operational Research (UK, WW2 Admiralty, Patrick Blackett)
CAS	Chief of Air Staff (UK, Royal Air Force)
Case	Case Institute of Technology (USA, now Case Western Reserve University)
CSSAD	Committee for the Scientific Survey of Air Defence (UK)
CSSAW	Committee for the Scientific Survey of Air Warfare (UK)
DCAS	Deputy Chief of Air Staff (UK, Royal Air Force)
DCS/R&D	Deputy Chief of Staff for Research and Development (USAF, PW, Curtis LeMay)
D of E	Director of Establishments (UK, Admiralty)
DNOR	Director of Naval Operational Research (UK, WW2, Admiralty)
DOR	Director/Department of Operational Research (UK, PW, Admiralty)

DRPC	Defence Research Policy Committee (UK, PW)
DSIR	Department of Scientific and Industrial Research (UK)
DSR	Director of Scientific Research (UK)
H_2X	Navigational radar that produced an image of surrounding terrain on its scope (USA)
HBR	*Harvard Business Review*
IDA	Institute for Defense Analyses
INFORMS	Institute for Operations Research and the Management Sciences (USA)
LRASV	Long Range Anti-Surface Vessel (type of radar)
MAUD	Code name for British committee to investigate potential for atomic weaponry
MAP	Ministry of Aircraft Production (UK)
MIT	Massachusetts Institute of Technology
NACA	National Advisory Committee for Aeronautics (USA)
NCB	National Coal Board (UK)
NDRC	National Defense Research Committee (USA, WW2, under OSRD)
NORD	Naval Operational Research Directorate (UK, WW2, Admiralty)
NPL	National Physical Laboratory (UK)
NRC	National Research Council (USA)
OEG	Operations Evaluation Group (US Navy, PW)
OFS	Office of Field Services (USA, WW2, under the OSRD)
OR	Operations Research (USA), Operational Research (UK)
ORG	Operations/Operational Research Group (various contexts)
ORO	Operational Research Officer (UK, WW2, under SAT), Operations Research Office (US Army, PW)
ORQ	*Operational Research Quarterly*
ORS	Operational Research Section (British Air Ministry, WW2)
ORSA	Operations Research Society of America
OSRD	Office of Scientific Research and Development (USA, WW2)
Penn	University of Pennsylvania
PhibORG	Amphibious Warfare Operations Research Group (US Navy, WW2)
R&D	Research and Development
Rad Lab	MIT Radiation Laboratory (USA, WW2, under NDRC Division 14)
RAE	Royal Aircraft Establishment at Farnborough (UK)
RAF	Royal Air Force (UK)
RAND	Project RAND/RAND Corporation (acronym for "research and development")
RDB	Research and Development Board (USA, PW, Department of Defense)
RDF	Early British term for "radar", meant to connote Radio Direction Finding
SA/AC	Scientific Adviser to the Army Council (UK)
SAAM	Scientific Adviser to the Air Ministry (UK)
SAC	Strategic Air Command (US Air Force)
SADU	Sea Search Attack Development Unit (US Army Air Forces, WW2)
SAT	Scientific Adviser on Telecommunications (UK, under MAP, Robert Watson-Watt)

SIM	School of Industrial Management at MIT (USA, now Sloan School of Management)
SORG	Submarine Operations Research Group (US Navy, WW2)
SRG	Statistical Research Group at Columbia University (USA, WW2, under AMP)
TIMS	The Institute of Management Sciences
TMH	Initials of Tom Hill, professor of accounting at MIT
TRE	Telecommunications Research Establishment (UK, under MAP)
TSP	Traveling Salesman Problem
USAF	United States Air Force
VCAS	Vice Chief of Air Staff (UK, Royal Air Force)
VCNS	Vice Chief of Naval Staff (UK, Admiralty)
WSEG	Weapons Systems Evaluation Group (US Department of Defense, PW)

Appendix D: A Brief Primer on Military Bureaucracy

In the timeframe of this dissertation, the British military was composed of three separate service branches, and each branch is governed by a political organization: a service ministry headed by a politician and run by the (non-military) civil service. Thus, the Royal Air Force (RAF) was under the Air Ministry; the Royal Navy was under the Admiralty; and the Army was under the War Office. Additionally, the RAF was divided into three home commands: Fighter Command, which was primarily responsible for home defense; Coastal Command, which was primarily responsible for patrolling the seas; and Bomber Command, which was responsible for bombing. The RAF also ran overseas commands, such as in the Middle East.

The Admiralty always ran its own R&D and production facilities; but at the beginning of World War II the Ministry of Aircraft Production (MAP) broke off from the Air Ministry; and the Ministry of Supply broke off from the War Office. After the war, MAP and the Ministry of Supply combined until 1959, when the War Office reclaimed its R&D and production, and the Ministry of Supply became the Ministry of Aviation.

Over the course of the 1920s and 1930s, each service ministry took on a central scientist called a Director of Scientific Research (with scope of powers varying from ministry to ministry) who oversaw R&D activities in the various laboratories and workshops; during World War II they worked for the Admiralty, MAP, and the Ministry of Supply. The Scientific Advisers, instituted during the war, worked for the Air Ministry and the War Office—not the supply ministries. This situation did not exist at the Admiralty. R&D and production facilities were staffed by scientists and engineers working for the civil service.

On radar: RAF radar was developed by the Telecommunications Research Establishment (TRE) under MAP; Army radar was developed by the Air Defense Research and Development Establishment (ADRDE) under the Ministry of Supply

In America during the war, the Air Forces (numbered Eighth Air Force, XX Bomber Command, etc…), were a semi-autonomous part of the Army, and both came under the Secretary of War; while the Navy (with its Marines) was completely separate, and under the Secretary of the Navy. In 1947, the post of Secretary of Defense was created, and the military institution of the Joint Chiefs of Staff (a wartime innovation) was perpetuated. The unified organization was at first called the National Military Establishment, but in1949 became the Department of Defense. At this same time the Air Force became fully independent of the Army within this organization. The Secretary of Defense was a relatively weak position until the 1960s.

As with the British, the American services had their own R&D and production facilities. These changed drastically so I will not outline them in detail here, but Wright Field was the Army Air Forces' foremost facility; the Navy was served by various bureaus, such as the Bureau of Ships, the Bureau of Ordnance, etc.

Military organizations are headed by a central figure (Chief of Staff, Chief of Naval Operations, First Sea Lord, etc.), but create high-level policy through a board or council of high ranking officers (often "deputies") with different responsibilities. These responsibilities vary from service to service, but will typically revolve around topics such as operations, personnel, logistics, intelligence, procurement, and so forth. Having science or R&D (as distinct from procurement) represented at this level was widely considered an important step in having the importance of ongoing innovation in strategy discussions recognized.

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Imperial War Museum, Department of Documents, London, UK
Papers of Sir Henry Tizard

The National Archives of the UK: Public Record Office, Kew, London, UK
ADM: Records of the Admiralty
AIR: Records of the Air Ministry and the Royal Air Force
AVIA: Records of the Ministry of Aircraft Production
CAB: Records of the Cabinet Office
COAL: Records of the National Coal Board
DEFE: Records of the Ministry of Defense
WO: Records of the War Office

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