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Revolutionary prospects

Will there be a "Revolution in Military Affairs" (RMA)? Should there be? What truly is needed?[1]

Will O'Neil

The United States has not experienced a successful political revolution in more than 200 years. Most serious accounts of American political, social, economic, and technological change stress the long term continuity and consistency of the processes at work. But a great many of us nevertheless feel we live in "revolutionary" times. Asked to identify the forces of revolution, most people point to developments in technology and especially information and communications technology.

Naturally, this revolutionary spirit has affected military thought. Many ideas and concepts regarding predicted or suggested RMAs ("revolutions in military affairs") have come into circulation.

Different problems: different proposals

In this paper, I will review the most prominent RMA proposals, summarize the models they imply, and assess their realism. A great deal of what has been said is neither realistic nor responsive to some of the most important and easily-foreseeable problems facing our nation. At the end of the paper, I explore some of these problems and offer ideas about a program for defense change to meet them.

Revolutionary visions

In military matters the interest in revolution has been further reinforced by the influence of Soviet military writings, which of course were all imbued with the official Marxist-Leninist "revolutionary" eschatology. Soviet writers in the 1970s developed a theory of "military technical revolution" (MTR) which in the U.S. was transmuted into "revolution in military affairs" (RMA).

Interest in the RMA was heightened by the Gulf War, which seemed to many to represent a sharp break with the past (and to others, a break not sharp enough).

[1] This is the fourth version of this paper, differing from the third principally in editorial refinements and addition of a new chart relating to oil.

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Since then, a variety of somewhat different RMA views have developed, which I will summarize very briefly before examining some of their principal theses.

JV 2020: DoD's official revolution

In 1996, DoD "institutionalized" the RMA theory with publication of *Joint Vision 2010*. This has now been updated and elaborated (but not fundamentally reoriented) with the newly-issued (in June 2000) report, *Joint Vision 2020*.[2] In an effort to provide a summary that is quite compact without being altogether Delphic, I have prepared a summary of the *JV 2020 RMA* model (as I see it) which appears in a box at the end of this paper.

Other RMAs

Unsurprisingly, *JV 2020* doesn't go as far as many think it should. There are a variety of proposals for RMAs that are somewhat similar in general outlook, but go further in various ways. Another box in the appendix presents a synthesis of some of these in a generic RMA model.[3]

Owens' "American RMA"

A somewhat different RMA vision is provided by retired vice chairman of the Joint Chiefs William A. Owens (who was the vice chairman at the time that *JV 2010* was developed). This is most completely developed in a recent book written with the aid of a newspaper reporter.[4]

Owens' vision of the RMA—which he calls his "American RMA"—is close to that presented by *JV 2020* and that synthesized in my generic RMA model in many respects, but diverges in some important ways. It is a particularly interesting and important vision, both intrinsically and because of the background of the man who advances it. In another appendix box I present the Owens "American RMA" model as I understand it.

As can be seen, the Owens RMA incorporates many of the technological elements of the others. But he expects more of them (or at least describes his expectations in more breathless terms). Moreover, he adds a very different dimension of organizational and cultural reform, which he expects will save enough to permit massive investment in new systems. It can all bear fruit by 2010, he promises, if we act decisively; he counsels sternly against delay for consideration and analysis.

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[2] These and related materials are conveniently available from a DoD Web site at the following URL: http://www.dtic.mil/jv2020/. I've excepted *JV 2020* in such a way as to serve as a summary; this has been issued as a paper in this series.

[3] This draws a good deal on Michael E. O'Hanlon, *Technological Change and the Future of Warfare,* Washington: Brookings Institution P., 2000. The interpretations are my own, however.

[4] Bill Owens with Ed Offley, *Lifting the Fog of War.* New York: Farrar, Straus and Giroux, 2000. See also my summary issued as a paper in this series.

Do these RMA visions make sense?

Some people have expressed visions that have moved the world to wonderful advances. I'll call these *visions of the first kind*. On the other hand, there's a jest that I've heard in various forms from several mental health care professionals: "In my business, people with visions get medicated and restrained." *Visions of the second kind*. What about these various RMA visions? Which side to they fall toward? Naturally, it's a mixed story. I will discuss several aspects of it. This is only a very brief summary of analyses that have been (or will be) presented in fuller detail in other papers of this series.

Digital microelectronics—vastly powerful, but not omnipotent

The development of digital microelectronics technology is a remarkable story, one likely to be celebrated by economic and technological historians to the end of time. It has impressed and delighted the engineers and scientists who have been involved in it and have understood exactly the physical principles and processes that have made it possible. It has been equally impressive to economists who have never seen a clearer example of a virtuous circle in operation in a market, and who finally have been able to identify significant effects on general economic growth stemming from it.[5] To most people, it has seemed more than a bit like magic.

Having become accustomed to the idea of radical and rapid change in computational power and the products and processes that depend on it, we often are tempted to make projections of further or correlate change without thorough and specific consideration of the underlying physics or economics. That is surely the case with many of the RMA proposals now in circulation.[6]

Technological factors

In technological terms, there has unquestionably been a revolution in processing of digital electronic signals, and it unquestionably is going to continue for some time to come. The principal technological factors are:

- Since about 1970, the capacity to process digital electronic signals has doubled about every one to three years (the precise doubling period depending on exactly what index one uses). Technological and economic analyses suggest that this rapid progress will continue for at least another decade and perhaps longer.

[5] It is not until the 1990s that such growth effects can be identified on a broad scale—technological revolutions characteristically take quite a while to generate significant economic effects. For an easy summary see Pam Woodall, "Untangling e-conomics," *The Economist,* September 23, 2000.

[6] This is not to say that those who do take the physics and economics into consideration can never be surprised (through faulty calculations or inadequate understanding, for instance), but their odds are much better.

- To the extent that data can be represented as digital electronic signals, the capacity to process data also has increased very rapidly. To the extent that information can be cast in the form of data, its processing too has benefited. But the difficulties of representing data as signals and information as data have been limiting in many cases.
- Electronic communications, in which the information and data are already represented electronically, have benefited greatly also from digital electronics advances, making more channel capacity available at lower costs.
- Sensor systems also have been transformed the digital microelectronics advances, not only in processing but in making sensing elements.

These advances in digital information processing are especially interesting because our brains also deal with information encoded as digital signals, albeit of an electrochemical nature rather than electronic. The digital microelectronics revolution has enabled machines to take over many of the information processing tasks that humans formerly had to perform, and has led to a sense that machines soon will rival humans in this regard. This is greatly oversimplified.

Much of the processing power of digital microelectronics stems from the speed with which electronic signals move—roughly ten million (10^7) times faster than in the brain. The complexity of the brain, however, in terms of number and complexity of computing elements and interconnections, is astronomically greater than that of today's most powerful computers.[7] Speed can substitute for complexity in certain tasks, but only to a limited extent in those most critical for information processing.[8] There is no foreseeable prospect that machines can closely approximate human information processing capabilities in many areas, such as perceiving and understanding the relationships among objects and surfaces in space based on information collected by sensors (as would be necessary, for instance, in driving a car or tank). Even if the present very rapid pace of improvement in computing capabilities can be maintained for many decades into the future, it is clear that machine capabilities will not begin to approach those of the brain before 2050, and it's likely to take a great deal longer.[9]

Application factors

Humans are notably the "brainiest" of animals, yet our brains comprise a small portion of our body mass and get relatively little of the body's resources. The rea-

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[7] The difference is so vast that no one at this time can determine it very precisely, but on the whole it seems that the brain is at very least a quadrillion (10^{15}) times as complex as today's most complex microprocessor.

[8] The brain also makes up for some of its slower signal transmission through smaller components and better packing density.

[9] Some much more optimistic estimates have been published. As specialists in the brain have pointed out, however, these rest in grossly inaccurate models of how the brain functions. In particular, they generally virtually ignore the cellular level, at which much of the brain's processing takes place, and treat the brain as if it were a simply-connected electrical network.

son of course is that it takes more than a brain to make a functioning, capable animal—much more.

This is true no less of the spheres of commerce, industry, and war. While some companies prosper by selling information and only that, they are enabled to do so only because their customers (and they themselves) have other sources for non-information needs.

In business, it has been shown that thoughtful application of information technology can bring substantial gains in output per unit of input—productivity. When one looks at a particular company or product line in isolation, these gains can sometimes be startling. But looked at over a larger scale—a larger firm, an industry, or an entire economy—the gains from information loom less huge. In the U.S. economy, information technology appears to have accelerated growth in productivity significantly in the 1990s. Yet most growth still came from factors other than IT.

It is rarely possible to make dramatic improvements in any complex process through information alone. Where it is possible, it's usually a sign that someone erred badly in constructing the process to begin with.

Machines are human too

Error is human, perfection, mechanical. We all have that image in our minds. "Human error" and "mechanical perfection" are set phrases in our language. But the contrast of mechanical perfection and human fallibility is illusory and dangerously misleading.

The chemical components of which humans are composed are highly reliable and perfect in their functioning, just as the individual electronic components in a computer are very reliable and perfect in theirs. The difference is that the human is astronomically more complex as measured by the numbers of components and their interconnections. Engineers today would have no idea how to build a computer of similar complexity that functioned at all, let alone one that even approached human standards of reliability and perfection.

In practice, we often observe that the more complex of today's information systems are a good deal less than perfect in their functioning. We all tend to respond to this with disparaging comments about the engineers and companies who built them, for we all *know* that electronic systems are *supposed* to be perfect in their operation. In hard engineering fact, however, complexity sets real limits on perfection of functioning. Engineers seek ways to relax these limits, often studying nature's biological systems for clues and ideas. Eventually, they may learn to make systems that approach nature's.

But the complexity of any system tends to grow as the square of the number of components (because the number of possible interactions grows as the square). So while the number of components in digital electronics systems doubles in 18 to 24 months, their complexity quadruples. For the most part, the engineering of reliable complex systems is not succeeding in staying ahead of this very rapid growth in complexity.

On the whole, it seems reasonably certain that very large, very complex systems will inevitably be very erratic in their functioning. The situation may improve with continued engineering effort, but not rapidly. Experienced system engineers generally counsel against trying to make systems that are too much more complex and closely integrated than those for which there is relevant experience. Those who fail to follow this advice frequently regret it.

Hard limits

For the most part, mechanical engineers long ago convinced their customers to stop asking for perpetual motion machines, but computer software engineers have yet to accomplish a comparable feat in their area. There are many problems for which it is impractical or impossible to compute a solution, no matter how fast the computer.[10] Unfortunately, this includes many scheduling and assignment problems that are of importance for military operations. Often it is possible to use computers to provide approximate solutions that are satisfactory for practical purposes. But in some complex cases it can be surprisingly difficult for the computer to beat experienced human schedulers.

Final output

The digital microelectronics revolution is an ongoing multi-decade transformation. It's made a great deal of difference in industry, commerce, everyday life, and war in the half century since its inception. Very likely, it will have equally great effects over the next half century as well. But there is little prospect that it can supply the means for a sudden and dramatic change in military affairs, because:

- Many aspects of war are inherently not susceptible to transformation on the basis of information processing, communications, or sensor information alone.
- Ideas that machines will soon come to rival human abilities in a variety of complex tasks are founded in a misunderstanding of the relevant technical factors.
- Ideas of the degree to which extremely large and complex information, communication, or sensor systems can be made to function reliably also are seriously affected by lack of technical understanding.

Thus most of the specific advances projected by the RMA theorists as a result of information technology in such matters as "transparent" battlespace, conclusive target verification, and greatly increased strategic and tactical mobility are not soundly rooted in rational calculation and are unlikely to be achievable, at least not in anything like the degree they project, nor with such rapidity.

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[10] For a non-technical summary, see David Harel, *Computers Ltd.: What They* Really *Can't Do.* Oxford: OUP, 2000.

Other areas of technology—no easy victories

While not a prominent part of the "official" *JV2020* RMA vision, projections of major technological changes in other areas play a significant part in the visions of many RMA advocates and enthusiasts. The principal claimed advances include:

- Land vehicles (including armored vehicles), ships, aircraft, and rockets that will be drastically lighter, more fuel efficient, faster, and stealthier than current models—all at lower cost.
- Much more powerful and compact non-nuclear weapons.
- Wholesale replacement of manned vehicles with unmanned, not only avoiding the casualty problem but bringing great savings in costs and mass.
- Development of new kinds of biological weapons posing tremendous dangers.[11]
- Development of directed energy weapons that will permit terrestrial targets to be destroyed from orbit.

None of these promises or threats is altogether lacking in foundation, but each is very greatly overblown. Each could and should be examined at length, but I'll very briefly review the main points here.

Vehicles

First, a point of engineering terminology: *Any machine that moves under the power provided by an onboard source is a **vehicle**.* A bicycle and a Saturn space rocket may not look very similar, but at the most fundamental level their performance and characteristics can be analyzed in the same framework, justifying the use of a common term to describe them, and "vehicle" is the term that has been adopted.

It can be shown mathematically that most of the economically and militarily valuable qualities of any vehicle depend on three factors:

- *Propulsive efficiency,* representing the efficiency with which the vehicle's propulsion system is able to convert fuel energy to effective power for propulsion and lift.
- *Kinematic efficiency,* meaning the power that is required to move a vehicle of a given mass at its operating speed.
- *Mass efficiency,* or the proportion of a vehicle's operating mass that is available for carriage of fuel and payload, after account is taken the mass of structure, operating equipment, and operating crew.

The engineering considerations involved in these factors differ among vehicles, but have some fundamental links that have tended to result in considerable correlation in the rates at which the economic and military performance of vehicles advance.

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[11] Biological weapons are invariably warned of as a threat, not advocated as an area of development.

It is useful to gain some perspective through reflection on common experience. If we think back 35 years to the year 1965, we can see that the transport aircraft, automobiles, and rockets of that time were not drastically inferior to those we are familiar with today. Looking back a further 35 years, to 1930, we see that the major vehicles of that time were dramatically worse than those of 1965. That is, the rate of gross change in vehicle performance slowed greatly in the last third of the 20^{th} century, relative to that of the middle third.

Why this should be so, its implications, and what should be done about it make up a fascinating and important story that I cannot begin to do justice to here. But analysis of the underlying technological developments shows little reason to expect much acceleration in the general rate of progress. Upon examination, expectations of major improvements have generally turned out to be founded on a combination of enthusiasm for technologies that are either (a) far from realization or (b) offer only marginal improvement over existing technologies, combined with a faith that the designers of the future will prove smarter than those of the past.

It is worth noting that there is room for changes in performance by making different choices among available technological options. The designer of racing cars draws on the same technology base as the designer of family sedans, but gets much greater speed and cornering performance by making tradeoffs with other performance and cost factors. The same thing can be seen in many other kinds of vehicles. But most of the possible tradeoffs have been explored fairly thoroughly, since the technology advances slowly enough that examples remain reasonably relevant for decades. People often advance proposals for "new" vehicles that in fact have been studied and even prototyped previously.

As in any other technological field, the rate of advance in vehicles is affected by economic factors. Engineers have ideas for development that are not pursued, or pursued only very slowly, because the prospects for gain do not seem bright enough (whether rightly or wrongly) to attract the commercial or governmental investment that is necessary.

One factor that tends to discourage development of vehicle technologies is the duration, risk, and expense involved in getting from a clear concept to an applicable system, and the uncertainties about how great the payoffs will be and when they can be reaped. The supersonic-combustion ramjet (scramjet) is one good example. Analyses show clearly that the scramjet offers a key to efficient flight at speeds of more than a mile a second and altitudes above 100,000 feet. A scramjet airliner might carry passengers halfway around the Earth in three hours or less, with only modest fuel expenditure, flying at altitudes too great for sonic boom to reach the ground. With scramjet propulsion, satellites might be boosted to orbit at a fraction of current costs. Yet after more than 40 years and hundreds of millions of dollars, the first operating scramjet test engine has yet to fly. This is only one example out of a great many that could be cited.

Explosive weapons

Much optimism has been expressed about the prospects for reductions in the sizes and improvement in the performance of non-nuclear weapons. Most of this centers in three areas:

- Great reductions in sizes of explosive weapons, made possible through more powerful chemical explosives.
- Substantial improvements in missile performance on the basis of more energetic rocket fuels.
- Attainment of much higher velocities for projectiles by using rail guns or other electromagnetic launch systems.

All of these ideas hold some significant realism and merit. But none is likely to have the sort of sweeping and revolutionary implications often claimed. The problems parallel those of vehicle technology. There is a long, tortuous, and vastly expensive engineering path to be followed in bringing ideas—even very good ideas—of these sorts to fruitful reality. There are always many surprises, and few of them are pleasant.

Again, each of these ideas merits much more extended treatment than can be given here. But a realistic and thorough analysis of their potentials and problems does not support the notion that any of them will have "revolutionary" effects on military affairs in the large.

Unmanned vehicles

Unmanned vehicles have been a subject of serious military interest since World War I and of more-or-less concerted and continuous effort since the early days of World War II. They have improved greatly over the past 50 years and have come to be more and more important. Yet they remain confined to fairly narrow niches.

Many believe strongly that this should change dramatically and soon, and that wholesale replacement of manned with unmanned systems will bring great benefits. Unfortunately, the reality is that attempts to implement this in the near future would almost certainly bring only greatly increased costs and diminished capabilities.

Unmanned vehicles combine the problems of vehicles and information processing. To many, it seems as if the problems of vehicle technology should be greatly lessened for unmanned vehicles, both because they need not be so safe as manned vehicles and because they are relieved of the burden of carrying a crew and their life support. There is some truth in this, but less than sometimes supposed.

While the crash of an unmanned aircraft, for example, does not hazard a crew, it does endanger others who may be nearby. Moreover, loss of a vehicle is expensive, even if no life or limb is lost. In earlier periods, it was thought that unmanned vehicles could be so inexpensive that the losses would be of little significance. But in most cases the savings of omitting crew are offset, partly or fully, by the costs of data links and remote controls.

Even if an unmanned vehicle could be made, let us say, only 10% as costly as an otherwise equivalent manned vehicle (a test which very few unmanned vehicles can come close to meeting at this time), it is far from expendable. If the manned vehicle is expected to deliver 2,000 operational sorties (excluding those strictly for aircrew training) then the unmanned equivalent must deliver 200, or suffer a cost penalty. No unmanned air vehicle type has so far even closely approached an ability to operate for an average of 200 sorties without loss.

A large part of the reason for the very high loss rates of unmanned vehicles is that components of low reliability have been used in their construction in an effort to save costs. Many of the smaller UAVs, for example, use for propulsion engines originally designed for light consumer or auxiliary power applications. Development of better components will alleviate the reliability problem. Increases in cost must be set against economies from longer useful life and better mission assurance.

In many ways, information and sensor technologies represent a greater limitation on unmanned vehicles. The designers of manned aircraft have long been under great pressure to reduce the number of aircrew. Today a "fighter" with a single crewmember can deliver more ordnance with greater precision than could a "bomber" of half a century ago that needed ten or more men to operate it. In the process, the engineers have automated all the functions that most readily lent themselves to automation. Those which remain are the ones that are especially difficult for machines.

Eventually there may be machines that will be capable of flying an aircraft through a complex mission or driving and fighting a tank in natural terrain. But to declare, as many do, that we will never need to develop another generation of manned aircraft or fighting vehicles is silly. Time enough to discuss such prospects after the first successful prototype demonstration. As yet, no one has any clear plans for how to make such a prototype, even in simulation.

Overall, the prospect is that unmanned vehicles will continue to make steady but unspectacular progress, much as they have over the past half century. Matters could be speeded by a carefully planned and well supported program to develop the necessary contributing technologies and components in an orderly way. Much money could be squandered in crash programs.

Advanced biological weapons

The dangers of biological agents are not in doubt. People die every day from the results of ingesting microgram quantities of biological organisms or toxins. It is not a difficult matter to build a plant and produce enough of such agents to kill every human alive, if the agent could be delivered. We are frequently treated to speculations concerning how many kilograms of anthrax spores may lie in the hands of Iraq, say.

However many kilograms it may be, it is certain that that vast majority of the world's supply of anthrax does not lie in Iraqi hands. The same holds true for almost any other agent one can imagine. The world already is filled with such hazards as a result of natural action. And we also gain a great deal of protection from them through natural factors, which we aid in their operation through public health measures.

If it were easy to devise and distribute a biological agent that would be capable of wiping out humans there wouldn't be any humans. There never would have been any humans. Nature would have gotten to it millions of years before anyone ever heard of Saddam Hussein, and some other, fitter species would have taken our place.

But doesn't genetic engineering change everything? No, it doesn't. With genetic engineering, humans can only do what nature allows to be done. Moreover, it only allows humans to do what they understand how to do, and that's precious little.

All this is not to say that the dangers of biological warfare can or should be overlooked. Epidemic disease and mass poisoning remain serious risks, as public health experts try to warn us, and malevolence can surely do something to exacerbate and focus these dangers, as well as introducing them in new forms. But there seems little reason to foresee a "revolution" in biological threats.

Destruction from on high

The laser was first developed more than 40 years ago. Even before it had actually been demonstrated in practice, engineers speculated about what might be involved in using it as a weapon. By the time the laser sees its fiftieth birthday, the first laser weapons may well be in service.

There is not much question that a powerful laser weapon in low Earth orbit (LEO) could deliver damaging amounts of radiation to terrestrial or airborne targets near nadir, assuming that there is not too much intervening cloud cover. The coverage footprint against ballistic missile targets would be larger. From the standpoint of tracking, pointing, and beam direction, a space vehicle is in many ways the ideal platform for a laser weapon.

At the same time, there are a number of serious problems and limitations to be overcome, including

- **Cost.** The laser and all its reactants would have to be boosted into orbit, at enormous cost. (Successful development of scramjet boost vehicles might reduce these costs significantly, but that is not likely in the near future.)
- **Numbers.** In order to have a laser available to engage targets at a few minutes notice, it would be necessary to have dozens on orbit, since in LEO they would

be in constant motion relative to the Earth, while each one would have an engagement window of no more than a few hundred square miles at a time.

- **Targeting.** With the laser 100 miles or more from its targets, and moving very swiftly relative to them, a very complex targeting system would be demanded. Enough has already been said about complex systems.
- **Safety.** The weapons would crisscross over the entire Earth, friendly and neutral as well as hostile. Great care would have to be exercised to ensure that these robot weapons did not take airliners or school buses under fire. These measures would undoubtedly interfere with the proper functioning of the weapon at times. Occasionally they probably would fail, with tragic and politically damaging results.
- **Countermeasures.** If the orbital weapons represented a serious danger, there are a variety of countermeasures that enemies could take to reduce their effectiveness. These could be countered to one extent or another by adding to the power and precision of the weapon. But that also would add still more to its cost.

There are other space weapons concepts, but all are subject to similar reservations.

It seems likely, notwithstanding all this, that space weapons will be built and will be found to be useful. But it will not happen soon and will not be revolutionary in a larger sense.

The quick and the slow

Before leaving the topic of technology, it is perhaps important to address a matter that puzzles many, and has led many to erroneous conclusions. While we have observed that there are limits to information technology and what it can accomplish, there can be no question that its progress in our lifetimes well qualifies for the adjective "revolutionary". Yet I've doused expectations of similar revolutions in other areas with generous quantities of cold saltwater. What reason can be given for the difference?

Innovation as an economic good

In the past two decades, many economists have modeled technological innovation as a good in the economic sense, something that can be bought, with a supply curve that slopes upward with price in the normal fashion. Historians of technology and science put forward models of innovation that seem much more complex, in which factors such as culture and aesthetics play a substantial role.

While we would probably all agree that economic factors play little role on when the next Newton or Einstein will be born, it does seem that they play a part in whether genius is likely to turn its attention to law, say, as against technology.

Thus in some sense it does seem as if it should be possible to command innovation with money.

The prize

But suppose that in the year 1900, for instance, someone had conceived the need for microprocessors and had offered some prize of astronomical value—let us say gold worth $100 billion in today's values—to whomsoever should first build a factory capable of producing processors equivalent to the Intel 4004 (i.e., the first commercial microprocessor). What would have been the result? One answer obviously is *pandemonium,* but how much would this have accelerated the advent of the 4004 (which in fact came on the market in 1971)?

In light of what we know about the sequence of discoveries and developments that led to microprocessors, it is not at all clear that even so enormous a price could have bought much acceleration of the process. In 1900 even the most brilliant and far-sighted physicists had no notion of how something like the 4004 might have been constructed. A great deal of basic scientific discovery had to be accomplished before it was possible to begin intelligent effort toward solving the technological problems. Yet by formulating the price strictly in terms of delivery of the final product, those who sought the result would have given little incentive to pursue the necessary scientific means.

Had the organizers decided to divert some of their great wealth to the encouragement of physics research, it is indeed likely that they could have accelerated the advent of the microprocessor, at least if they had done so in an intelligent manner and foresighted. But unless their foresight were perfect, most of the research they funded would ultimately have been found to have little direct application to microprocessors. They would have enriched the world with wonderful knowledge, much of which would eventually have found uses in other fields, but not microprocessors.

And what of the point at which the threshold of enabling physical knowledge was at last crossed—say with the construction of the first transistor? Once the necessary physics had been understood, it would have been possible to make a crude transistor quite a long time before the feat actually was achieved. Imagine, for instance, that the first transistor had been made in 1927 instead of 1947. What then? Many of the production technologies that were harnessed in the 1950s to produce the first integrated circuits (ICs) did not exist in the 1930s. It would have taken quite a lot of development across a wide range of industries before a clear path to the IC and then to the microprocessor were opened. Again, we may question how effective a price put solely on the final product would have been in stimulating the necessary subsidiary developments.

Finally, I should remark that this story is less whimsical than it may at first appear. Even with a sizable discount rate, the value of Intel's future earnings in 1971 far exceeded $100 billion. That is, there truly was a prize of astronomical value for those who first made a microprocessor. Why did rational, economically motivated innovators fail to respond earlier and more vigorously to this incredible incentive? There are two parts to this answer. First, they didn't realize that the incentive existed until perhaps the mid 1950s at earliest. And some of those whose efforts were needed could see no way to claim a share of it on the basis of their work.

Since the 1970s, Intel Corporation has presented a classic example of an intelligently-managed near-monopoly possessed of extremely strong incentives for innovation. It would be difficult to conceive of any ways in which microprocessor innovation could have been made to proceed any more rapidly than it has over the past 30 years. Yet it is worth noting that Intel has not been a primary source of microprocessor technology in this period. That role has been filled largely by universities and government-fostered consortia (both, of course, with strong encouragement and support from Intel).

Kinds of innovation and risks

Innovation in microprocessors has included elements of both *process* and *product* technology, but process technology has been the greater and more important of the two. That is, much of the effort has focused on *how* to make microprocessors, not *what* microprocessors to make.

Of course, failure to develop microprocessors that attract a great market would have very grave effects on the industry and its processes of innovation. But given that there is a strong market for its products, the industry finds strong incentives to pursue process innovations that offer the capacity to produce improved products at lower costs. Indeed, the incentive has continued strongly as the capital costs for a single microprocessor *fab* (fabrication plant) have climbed from a few million dollars to tens of billions.[12]

More uncertainty can attend the development of a product, especially when it is of a new kind for which there is limited market precedent. The recent market failure of the Iridium satellite cell phone system and resulting bankruptcy of its corporate producer stands as sufficient example. Still more chancy is development of process or enabling technology for an unproven product, with its piling of uncertainty upon uncertainty.

The chilling effects of these uncertainties are magnified when the costs of innovation are large relative to the resources of the company or organization which must bear or guarantee them. The effects of this can be seen very vividly in the case of things like large vehicles, where major innovations have, as earlier noted, become extremely rare. Even in the most vigorous of large vehicle markets, that for commercial airliners, each new model is notoriously a "you bet your company" proposition for the developer. Now, as a direct result, only two producers of major

[12] While the complexity of microprocessor chips typically roughly doubles at each 18- to 24-month "generation," the cost of the fab necessary to produce the new generation typically quadruples.

airliners remain in the world, neither one of which shows much disposition to wager its entire future on new concepts such as a supersonic airliner or laminar flow control. These and other innovations may eventually come to market, but the path will inevitably be slow and circuitous.

Unfortunately, many of the things DoD needs most for improved capabilities and reduced costs—to say nothing of the truly "revolutionary"—fall squarely in the class of innovations whose risks are too great and payoffs too uncertain to engage major commercial commitment.

Commercial vs. military innovation

Finally, before summarizing this section, I must comment on the virtues of "commercial technology" as opposed to "military technology". Prior to the 20th century, very few people would have identified the military as a major source of technological innovation. Indeed, for the most part the military was regarded as a bastion of technological conservatism.

The events of World War II changed all that, and the momentum of the technology advances associated with that conflict (skillfully promoted by advocates) combined with large Cold War defense budgets to produce an environment in which it was taken for granted that defense was not only technologically progressive but was indeed the primary force for technological progress of all sorts.

In the 1990s, however, we have swung back to the historically normal view that private industry is the "natural" source of technology and that the military's role is to function as a largely passive recipient and adaptor of commercial innovations.

I am going to suggest that the virtues of commercial technology derive from economic resources and not from any inherent moral or aesthetic superiority of the *commercial* mode over the *governmental*. Many people will disagree, and to the extent that this is a transcendental question it is of course inherently insusceptible to resolution on evidence. But there has been some empirical investigation of the related claim that industry is more efficient in the sense of producing more valuable technology per unit of investment, with the result that little evidence to support this has been found.

In the field of microprocessor technology, the superiority of commercial resources is plain enough. Estimates of U.S. commercial investment in this area suggest that it considerably exceeds the sum of all investment made by DoD in all areas of technology development. Thus it is scarcely to be wondered that industry is the primary source of technology of this sort.

Summary

- Money definitely can buy innovative *activity*. Whether it can buy a particular desired innovation is a much more complicated question without an unequivocal answer.

- Under favorable circumstances, commercial innovation can create great wealth, and prospects of further wealth from further innovation that stimulates intense innovative activity in directly related fields.
- Where the paths to wealth are not so clear, the commercial incentives to innovate operate much more weakly, and innovation is likely to proceed much more slowly and fitfully, especially in areas where the costs of innovation are high.
- Outside the area of information technology, many of the innovations which would appear to be of greatest potential value for DoD are only weakly incentivized commercially, if at all.

Cultural and organizational change—the "Dilbert" problem

While Owens makes the greatest point of it, all of the principal proposals for RMAs count a great deal on cultural and organizational changes to achieve their ends. This is certainly realistic in the sense that no great alteration in the effectiveness or efficiency of any group's functioning can be expected without some shifts in how the group conducts its affairs. But to achieve their "revolutionary" ends, the RMA advocates mostly seem to rely on directing this change authoritatively from the top. This is understandable—it's the military tradition, of course, and it gives the appearance of being fast and simple.

It also tends to be largely ineffective in bringing genuine cultural change. Responses typically run from incomprehension to malicious compliance. To plan quick and simple major changes in cultural and organizational attitudes and behavior is unrealistic, and to count on them for major results is folly.[13]

Business economies—a good idea, too often oversold and inadequately executed

Almost everyone talks of wanting to improve the "tooth-to-tail ratio" of DoD, and most count on this to pay at least some of the costs of investing in new technology and more equipment. The phrase itself makes no sense at all and the concept behind it is extremely slippery.[14] But if we substitute a less tortured notion, such as the desirability of reducing the costs of the business and support operations of DoD, then it should be possible for everyone to support it in principle.

In practice, it's not so easy. Most of what DoD spends goes to salaries, and so cutting costs means cutting jobs or pay. Everyone now knows Joseph Schumpeter's chillingly evocative phrase for this: *creative destruction.* We've learned to live with this in business, more or less, but not yet in politics.

[13] See the paper, "The RMA and the idea and practice of revolution" in this series, and its appendix B, "Prometheus and Epimetheus".

[14] See the paper, "The Owens program," in this series.

Even if Congress can be persuaded to approve of some act of creative destruction, it is difficult to carry it out too quickly without turning into destructive destruction. Where a large, complex organization produces outputs that continue to be needed, it is rarely possible to improve the efficiency with which it produces them by an average of much more than about 5% per year, over any extended period of time.[15] In practice, it is usually found that it takes several years of intensive effort and investment in new capital and processes before significant improvement starts to accrue.

There is every reason to think that significant improvements in efficiency are in fact possible. To seek, say, a 20% reduction in DoD annual support costs over the period of a decade seems ambitious but not entirely incredible, given political will. But to try to do it overnight by fiat, without careful analysis to identify the most productive approaches, would probably lead to nothing but chaos and waste.

It is especially unrealistic to expect sudden, dramatic improvements in productivity as a result of particular investments and initiatives, even very ambitious ones. In the economy as a whole, for instance, it took well over two decades of heavy investment in computerization before the productivity effects began to be truly apparent.[16]

Rubber yardsticks—unvulcanized

One great oddity of the RMA is the extent to which many advocates express much faith in technology and little in science. That is, they envision technological marvels that will bring a new order in military affairs, but scout notions of serious scientific study and analysis of these affairs.

The best basis for revolution is science. Indeed, the whole phenomenon of widespread "respectable" enthusiasm for revolutionary change is largely a result of the rise of science.[17] But today's science of military affairs is a great deal more like medieval alchemy and astrology than any modern concept of science. This is not to say that it is all wrong—but it's difficult to know what is right, and there certainly isn't enough known to provide a good basis for revolution. That is, we may make revolutionary changes, but we are poorly prepared to predict what the effects will be, or whether the result will even be an improvement.

There is no way to tell what a genuine science of military affairs might produce, any more than James Clerk Maxwell could have foreseen that microprocessors and

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[15] In the late 1990s, total factor productivity (TFP) in American non-farm business as a whole has been improving at a rate of about 1.5% per year (after adjustment for cyclical factors)—quite a high rate by historical and international standards. Labor productivity growth is always higher than TFP growth because of progressive substitution of capital for labor, among other reasons. Labor productivity has recently been improving at nearly 3% per year. As this implies, reducing costs by 5% per year is likely to mean cutting payrolls by something like 10% per year.

[16] Again, the *Economist* article on "Untangling e-nomics" summarizes the evidence digestibly.

[17] See "The RMA and the idea and practice of revolution" and "Prometheus and Epimetheus".

satellite communications would be products of his model of electromagnetism. But we have ample reasons to believe that science is likely to produce valuable results in any area where it is seriously applied.[18]

Just as people in the Middle Ages explained away the defects of alchemy and astrology, so do experts today tell us that we can't really have a science of military affairs, due to "friction," "fog," "chaos," etc., etc. And just as alchemists were always just about to find the philosopher's stone and astrologers always about to find the perfect horoscope, given sufficient access to the royal purse, so today we have leaders who are certain that they can lead us to a revolution that can not fail, and chafe at the notion that anyone might want to spend more time on "studies" of a scientific sort. After all, to a military leader, nothing is more to be avoided than the trumpet that blows an uncertain note.[19]

Are the means being used to justify the ends?

One striking aspect of virtually all of the RMA proposals is the perfunctory and superficial nature of the putative justification for a "revolution" to begin with. The need for revolutionary change is almost always explained in terms of the most common and banal of conventional wisdom about the continuing need for a U.S. role in the world and the risks that a threat might arise.

Nor do advocates spend much time or energy in explaining with any specificity how the supposed products of their RMAs will relate to American needs in the future, or why we should invest money and effort in the RMA in preference to other national needs.

All in all, it is difficult to avoid the impression that much of the enthusiasm for revolution has preceded any careful consideration of its benefits, and indeed virtually excluded serious thought about them.

A trumpet with a very uncertain note

The RMA proposals I've reviewed here all have some elements of attraction, but all are gravely flawed. Need, value, and feasibility all are left in serious question. However hard their advocates may puff, these instruments cannot be cleared, and can produce none but the flattest and most wavering of notes.

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[18] See "The RMA and the idea and practice of revolution" and "Prometheus and Epimetheus", which summarize the reasons for the efficacy of science and provides some criteria for distinguishing science from non-scientific modes of inquiry.

[19] This in part accounts for the very curious fact that the DoD, which is a major funder and performer of basic science, does scarcely anything to promote serious, broad-scale development of a science of military affairs.

Challenges that might demand an RMA

Should this be taken to mean that all is well in this best of all possible defense establishments, and that we should simply hold on course? That is more or less what many critics of the RMA seem to say. It would be nice to think this is so, but I am not convinced it is tenable. Let me point out two things that I see as potentially quite serious challenges of a sort that may demand major changes in our defense posture. These are challenges that everyone knows something about, everyone talks about, and scarcely anyone relates clearly to our security needs and problems: the *age of aging* and the *real energy crunch*. Together they are likely to change the world, and the changes could easily have a serious effect on U.S. security, if we simply continue on course.[20]

The *age of aging*

Everyone knows that there are problems with the U.S. social security system and Medicare. Politicians all talk about their proposals for putting the systems back on a sound financial footing; it's one of the issues of the day. Sophisticates warn that money will be short for defense as a result of social security and Medicare fixes.

Welcome to the age of aging, and what could very easily prove to be the great crisis of Western Civilization.

After World War II, babies became very popular throughout North America, much of Europe, the richer parts of Asia, and the places with close ties to these societies. But then, starting with the 1970s, people lost interest in having babies. This wasn't true all over the world. Babies continued to be born in record numbers in South Asia, Africa, and other poor places. But in the richer, more industrialized parts of the world, birth rates went down and have stayed down.

The effects of this are not difficult to work out, and are subject to very little uncertainty. We may not know what the technology of 20 years from now will look like, but we know a lot about the age distribution of adults then: all of the people who will be adults in 2020 are already alive, and unless there is some mass dieoff of humanity, there's essentially no question about how many of them there will be. And unless there is a sudden spurt in longevity, there's essentially no question about what the age distribution will be.

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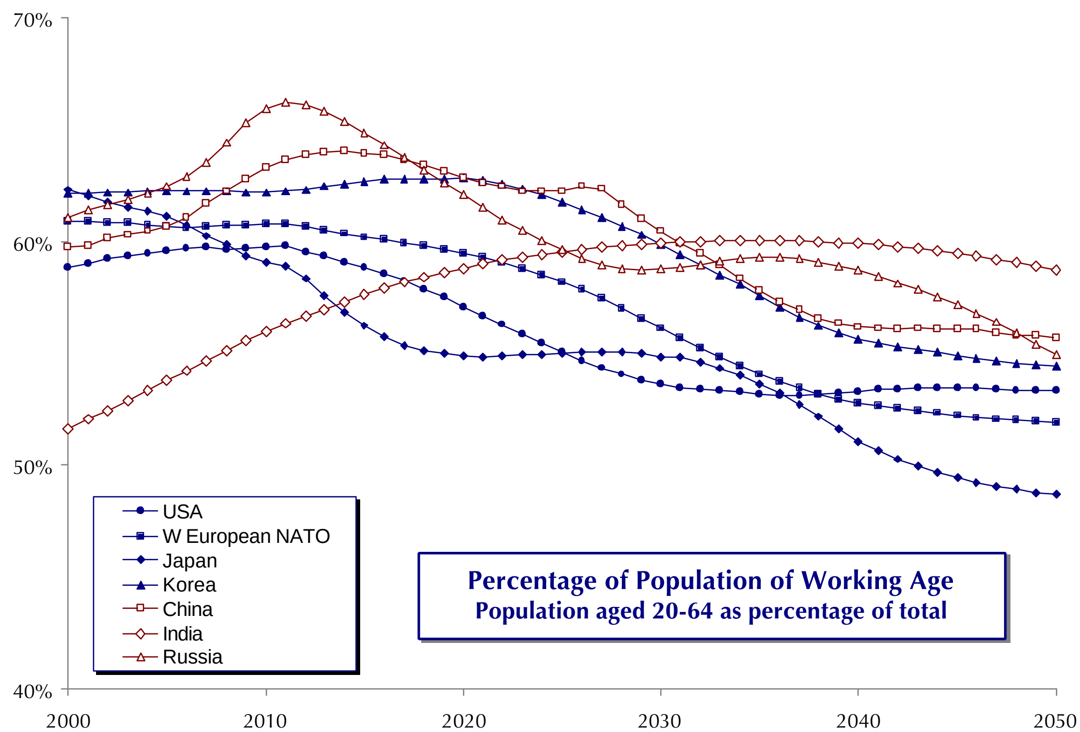
[20] For both, see William D. O'Neil, "Innovating for 21st Century Naval Operations," presented at a conference on *The Role of Naval Forces in 21st Century Operations,* sponsored jointly by the International Security Studies Program of the Fletcher School of Law and Diplomacy, Tufts University; the Institute for Foreign Policy Analysis; the Commandant of the Marine Corps; and the Chief of Naval Operations, in Cambridge, Massachusetts, on 20 November 1997. A much more extensive treatment in this series, entitled "Oil as a strategic issue," is now in draft and is available in draft form from the author.

Even if we look all the way out to 2050, the people who will comprise the older part of the population are all alive today. And unless birth or death patterns change very sharply, we can make reasonably certain calculations of the numbers and age distribution of adults in 2050.

When we do this, we find that the people who were born in the post-World War II baby boom progress like an indigestible lump through the snake of time, resulting in a larger and larger proportion of old people. This will happen not only in the United States but virtually everywhere.[21] In fact, the problem is less severe in the U.S. than in most industrialized countries, due to continuing strong immigration as well as relatively high birth rates.

The world is not going to run short of people, of course. But the proportion of people in their prime working years, living in societies where they get the social,



educational, and capital advantages to allow them to be highly productive—that proportion will fall sharply from now through the middle of the century and beyond. Not just in the U.S., but almost everywhere that is today rich—and in many places that are not.[22]

This does not necessarily mean that the world will get poorer, but it certainly does imply severe economic pressures. The growth in per capita income is almost

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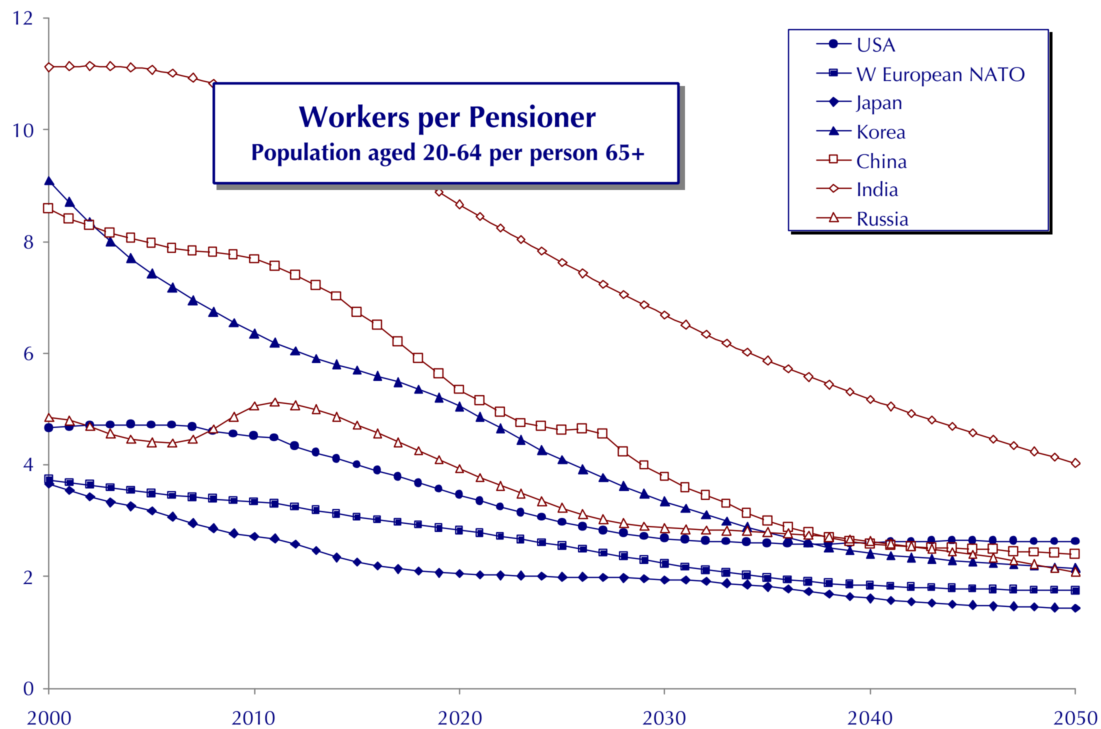
[21] Africa may be an exception because of very high adult mortality due to AIDS and other diseases.

[22] Data presented in the graphs are derived from the U.S. Census Bureau's International Data Base (IDB), edition of 10 May 2000.

sure to slow, and could reverse in many places. This is clearly going to bring a lot of strains.



Moreover, the aging process is going to occur at different rates and at different times in various places. China's aging bulge will lag ours by 15 years or so, for instance, while the proportion of elderly in Europe and Japan will reach heights we and China will not approach.

All of this will clearly mean a shortage of resources for defense in the world of the first half of the 21st century. And it may mean increased challenges, as we face a world roiled by hardships and divisions related to aging populations.

The *real energy crunch*

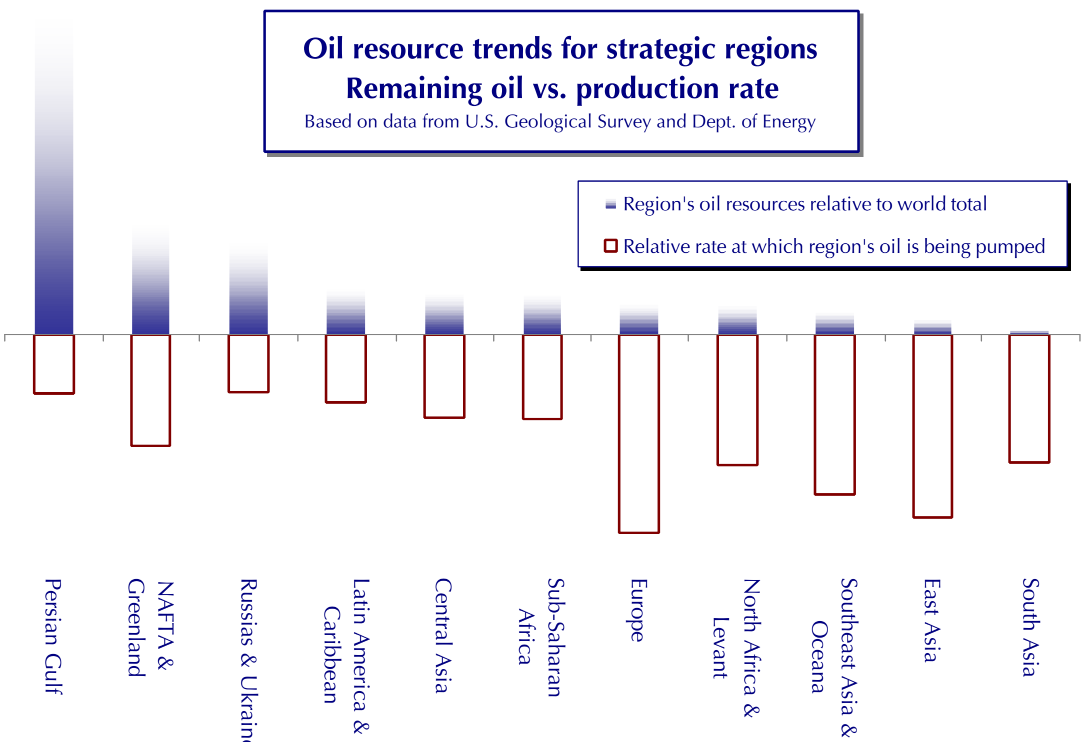
Everyone knows that there are problems about oil shortages. Politicians all talk about their proposals for assuring adequate supplies of heating oil and motor fuel; it's another of the issues of the day. Military analysts warn that we can't let our guard down in the oil-producing regions of the world.

There is no real shortage of oil—not yet. What we've been seeing is simply normal fluctuation in market conditions as the market swings between temporary tightening and loosening.

But there is a shortage on the way. The amount of oil that lies beneath the surface is finite, and eventually exhaustion of the most cheaply extractable reserves will force prices upward on a continuing, irreversible basis.

Predictions about the oil running out are a running joke in the industry. People have been making them for 150 years, and they have proven to be wrong time and time again. There is today a reasonably good accounting of how much oil is left to be extracted, but there are many uncertainties which affect the final result.

According to the best current estimates, in 140 years of production humankind has used less than 25% of the world's original endowment of oil. It is estimated that the amount of oil still left to be discovered is greater than the quantity so far consumed.[23] Nevertheless, it seems fairly likely all in all that the underlying supply-price curve for oil will start to move upward at some point in the second quarter of the 21st century, as demand starts to press on the cheaply-producible supply. In principle this price rise could come smoothly and gradually. In practice, something very much more dramatic seems likely.



The world's greatest oil fields lie around the northern parts of the Persian Gulf. Even though these fields supply nearly 30% of world production, they are so vast that in a relative sense they are being depleted less rapidly than other reservoirs. Thus it seems very likely that most of the last remaining major reserves of oil will lie in the northern Persian Gulf region. It seems very possible that nations will

[23] See "Oil as a strategic issue".

become desperately concerned to secure access to their share of the supply from this region as prices rise and deliveries dwindle. If the United States is to remain master of its own fate, we will have no choice but to continue to assert our ability to prevent anyone else from exerting unfair control in this region. And that means continuing to have a need, probably even more strongly than in the past, to be able to project military presence and power halfway around the world, in the face of the strongest opposition others can mount.

A case for change?

I submit that the imminent approach of the age of aging indicates significant changes in defense planning, and that the likelihood of a real oil crunch suggests the need for further changes.

The impact of the age of aging

The age of aging will bring a slowing in the rise of income per capita. (In some places it probably will bring a drop, but probably not in the U.S.) It will also bring an increase in demand for expenditure on medical care (as well as turmoil concerning the distribution of income across age groups). All of this will exert downward pressure on military budgets. This pressure will increase, not fall, and will go on increasing through 2040 at least. It's not a problem we're going to get past soon.

The impact of oil

As oil production comes more and more to be centered in a region halfway around the world from us and as forebodings about shortages become more acute, it will come to seem even more important for the U.S. to have the capacity to prevent domination of this region by others. Ideally, we will not have to resort to force. But in matters so dire as this, to deny ourselves the capacity for the *ultima ratio regum* would be folly.

This logic is reinforced by the possibility of sudden climate shifts (and other, less likely catastrophic changes). For any such shift would inevitably put sudden additional pressure on oil supply, and could well lend a note of desperation to the contest for control.[24]

Thus the likelihood of increasing pressures in the oil-producing regions around the northern Persian Gulf, together with the possibility of shifts that could suddenly intensify these pressures, argues that we should move even more toward a military capable of exerting decisive force halfway around the world.

[24] See "Innovating for 21st Century Naval Operations".

More and less

That is to say that we must become better at accomplishing the most difficult and costly of military missions, and we must do so in a period when resources will be especially tight. We may quibble over whether the changes this calls for are "revolutionary", but surely it calls for change.

What might it take?

I want to emphasize that what I am about to say is largely speculative. It isn't any more speculative than most of what else has been written on RMAs, but it isn't a great deal less. To get answers that a rational policymaker would like will take time, effort, and money to pay for the effort. There is (a little) time, there are people who are capable of doing the work, but there has been little money for such things. Unless and until some agency is prepared to pay for serious scientific study, policy in these areas will have to continue to be based in speculation. It is in this spirit that I offer mine.

Implied needs

Let us imagine that in the year 2030 Cynicstan, a major Asian regional power, decides that it must gain much greater and freer access to oil so it can pursue its historic and natural destiny without hindrance. It makes common cause with Peraq, a major oil producing nation situated near the head of the Persian Gulf. With Cynicstan's help and support, Peraq demands that its neighbors give up important concessions and acknowledge its right to dominate their policies. The U.S. is to be excluded from the region. And if the U.S. tries to intervene, Cynicstan will use its asymmetric military capabilities to strike at us.

What differentiates this from many other RMA scenarios is the clear focus on a specific area—a focus that makes it clear just how important distance is.

There are many ways that we might respond to this. I would suggest that the way we would probably prefer, if feasible, would include the following elements:

- Interposition of our military forces (together with whatever allied forces we could prompt to come to our side) to block and if necessary defeat Peraqi-Cynicstani aggression in the region.
- Interposition to sever the lines of communication between Peraq and Cynicstan, and to cut off the supply of oil to the latter while denying oil revenues to the former.
- Neutralization or destruction of Cynicstani forces that could threaten the U.S.
- Defense against Cynicstani attacks on the U.S., our forces, or our critical allies.
- Infliction of extensive damage on Peraqi-Cynicstani military forces and their critical infrastructure, to emphasize the costs of aggression, and reduce their capacity for more.

The logical inference is that we want to have this sort of spectrum of capabilities at a level sufficient to deal with any likely "Peraq-Cynicstan" alliance. Of course it is difficult to say what threats are truly likely three decades from now, but we have no choice but to make a stab at it, since the decisions we make now will have strong effects on the forces we have in 2030, or even in 2040.

"Centers of gravity"

Some RMA visionaries, borrowing a term (if not the style of fact-based, logical thought that goes with it) from physics, would shorten the list to one item. If we can attack the enemy's "center(s) of gravity" in overwhelming strength, they urge, we need not worry about the rest.

Find the right horseshoe nail, knock that out, and victory is assured.

It's an appealing idea, but the justification for it so far is very slim, either in theory or in history. If there were a genuine body of science about military affairs, it might be possible to say with some certainty whether there truly are such "centers of gravity" whose destruction can knock out a modern state, and what they might comprise. In the meantime, we run much less risk if we depend on more conventional strategic logic.

Far, far away

The United States is unique in the distance at which it exerts its military power. If we are to accept the logic of the PC ("Peraq-Cynicstan") scenario, the demands for long-distance force projection will, if anything, be still greater in the future.

Some people claim that we can exert decisive influence on events halfway around the planet while never leaving home, employing "virtual" means. We should think about this in the same terms as we do about the "centers of gravity" theories. Whatever value there may really be in such ideas needs to be evaluated in a far more objective and solidly-based way before we can afford to place reliance in them.

In the meantime, we must fight distant wars with forces already on scene and/or those brought in for the purpose. Lacking any plausible means to base large forces in the region, we can maintain forces there only through rotating (or prescient) deployments. Of course both rotational deployments and crisis deployments depend on strategic mobility. And strategic mobility can only be provided by vehicles.[25]

Vehicles actually enter into the strategic mobility issue in another hugely important way. At the operational and tactical level, war depends on tactical mobility that is provided by tactical vehicles—fighters, tanks, trucks, guided missiles, and the like. Most of these vehicles have to be transported to the theater by strategic

[25] Just a reminder that I use the word "vehicle" to mean any mechanically-propelled device.

lift vehicles, together with spare parts and maintenance equipment. All of their ammunition must also be carried. And if we cannot depend on local fuel supplies then these must be lifted as well.

Once the tactical vehicles are in the field, it gets even more complicated. All of their fuel and ammunition must be carried to wherever they are, and their demands are voracious. The region around the head of the Persian Gulf, with its difficult environment, diverse and difficult terrains, and great distances makes especially great demands on tactical vehicles and their logistics.

All in all, vehicles and their associated support account for a huge percentage of our defense expenditures. More than anything else, our need for vehicles is what forces us to spend vastly more than other military powers in order to maintain moderate levels of superiority.

Can't we just think about it?

One of the promises of at least some schools of RMA thought is that we should be able to dominate in maneuver simply by possession of superior knowledge. If you know exactly where you need to be far enough in advance, this logic goes, then you need be in no rush to get there. This is based in faulty logic and failure to examine relevant historical precedents. In fact, disparities in information can most often only be exploited by superior mobility. Certainly, superiority in mobility is the key to being able to rely on getting full value from superior information.[26]

What superiority in information can buy is freedom from the need to cover so many bases, and thus a potential to cut force structure. But the forces one does have must possess the capabilities to exploit the information effectively—and that means mobility.[27]

A meaningful and feasible RMA?

A very large fraction of the costs of U.S. defense get spent in one way or another on vehicles. A large portion of our troops serve as vehicle crewmembers, or in the logistics system that supports vehicles. Our needs for strategic and tactical mobility alike are likely to be greater, not less over the coming decades. Our resources of money and manpower for defense are going to be under pressure for decades to come.

What does this all add up to? I submit that it does not clearly point to a need to put our principal emphasis on development of electronics and sensors, to put "experimentation" unguided by clear analysis at the head of our priorities, or to plan

[26] See William D. O'Neil, *Technology and Naval War,* 1981;2000.

[27] Within some limits, tradeoffs can be made between numbers and mobility, and the right choice will generally depend on the relative prices of each.

on "getting well" by gaining an increased share of the nation's resources for defense.

Here are the things that seem most desirable and important:

- Cut support costs by improving productivity of businesslike activities.
- Cut force costs by improving productivity of warlike activities.
- Improve the technology of vehicles so as to increase performance and lower costs.
- Improve the technology of the other things (beside vehicles) that we must transport to distant theaters so as to reduce the demands on lift. This principally means logistic support systems and weapons.
- Reorder our organizations and concepts to make most effective use of the improved technology.
- Develop a sound science of military affairs so that we will have a good basis for evaluating the value of alternative forces and concepts.

Cut support costs with improved productivity

I've already addressed this above, but of course it bears re-emphasis. There is every reason to expect that consistent, well-conceived efforts will yield large and continuing savings over the long term.

Cut force costs with improved productivity, too

If the opposition to productivity improvements in support activities (with consequent manpower cuts) is external politics, for productivity in military force components (and its force cuts) the opposition is internal politics. Few military leaders want to preside over smaller forces, even if nothing is lost in effectiveness. But how can we say that military forces, unique among all the major institutions in our society, are immune to improving productivity through intelligent application of technology?[28]

Get more with less from vehicles

This is a task to make the strong tremble. Few people think that achieving major improvements in the price/performance ratio of vehicles is easy—and none of them have ever tried it. But if we want to do something technological that will

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[28] Improving the productivity of military forces doesn't automatically imply that we should reduce their size, any more than company workforces always shrink in response to productivity gains. It is possible to take the gains in the form of more output for the same input rather than less input to produce the same output. More output may be demanded, after all, if productivity gains bring a reduction in price. But in practice, for military forces productivity gains are likely to translate at least in part to smaller forces.

bring real gains, this is it. There are three main parts to this, presented in terms of the categories of propulsive, kinematic, and mass efficiency introduced earlier:[29]

Propulsive efficiency

Improving propulsion and power systems is the most highly leveraged thing we can do to improve vehicles. Improvements to gas turbine and Diesel engine technology should be pressed with renewed vigor, for they will bring dividends across the board for all kinds of vehicles (and fixed power plants as well). The fuel cell is the dark horse, at least for power and non-aircraft propulsion. The potential of the fuel cell has been very clear for a long time, but the obstacles have been great and there has not been sufficient economic impetus to resolve them rapidly.

Kinematic efficiency

Cutting drag without reducing lift (and thus improving what engineers call the L/D) is partly a matter of understanding more about the mechanics of fluids and partly one of finding better ways to mechanize what we already know.[30] The achievable improvements for existing types of vehicles tend to be worthwhile but small. There may be a potential for big improvements from entirely new types of vehicles, but the costs of innovating in big vehicles tends to discourage progress.

Another route to significant improvement may lie in exerting control over the details of the flow of air or water about the vehicle's skin. Such *boundary layer control* or *laminar flow control* schemes have long been pursued with little result, but new technologies arising from chaotic control and micro-electro-mechanical systems (MEMS) technologies may offer promise for practical methods.

Mass efficiency

We know many ways to make vehicle structures light and also ways to make them inexpensive. But light plus inexpensive is very difficult. It is not simply a matter of better materials. Many a material with very promising properties has never made it through the engineering process to actual use in vehicle structures.

After structure, the major contributor to vehicle mass in most cases is propulsion and power. Again, efficiency of propulsion (which of course is crucial for reducing the weight of fuel to be carried) tends to war against lightness.

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[29] These are indeed the things that count in terms of the performance of vehicles for strategic lift missions, as demonstrated by what is known as the Bréguet range equation. Naturally, there are other things that are important in practice as well, particularly for tactical vehicles. I don't mean this list to be exclusive.

[30] "Fluids" here means air or water. From a physics standpoint the two are the same except for certain constants in the equations.

Improve technology to lighten the load

A great deal has already been accomplished in this regard. The proportion of weapons which hit their targets has improved dramatically in many areas, implying that we should be able to carry fewer weapons in many cases. Improved reliability (and improved techniques for planning spares policies) imply that we can get along carrying fewer spare parts. The list goes on, and there's more to be done. Many of the issues closely parallel those for vehicles.

Change the ways we operate to make use of new technology

A lot of good technology gets "stranded" because there is no one with the will and the means to ensure that it gets meshed with processes that will actually work for the people who must use it. The defense acquisition system has gotten better at this, but still has a long way to go. If we are to develop truly "revolutionary" systems, the challenges of effective integration will grow.

Do science

Science is the study of what works. Military people have always tried to study what works, but science offers a better, surer, deeper method. It is truly absurd that a DoD that spends billions of dollars every year on science can not support a serious program to build a science base for its own operations. It is impossible to predict the value of any particular scientific effort, and usually impossible to accurately assess the value even in retrospect for many years, but we have every reason to suppose that scientific effort in general has high rewards.

A "revolution" driven by need

This is a program that is not going to be easy. Unlike some would-be "revolutionaries", I have no quick and easy solutions to offer. The problems are difficult and the solutions arduous. But if we want the United States to remain free, prosperous, and independent through the perils of the age of aging and the real oil crunch, I don't see a good alternative.

End

Appendix
RMA Models

The *JV 2020* RMA model

This is an outline of the model of the RMA implied by *JV 2020.*

Demand stimuli

- Continuing U.S. global interests and need for global engagement.
  - Adversaries have access to same dominant commercial technology base as U.S.
- Adversaries will adopt asymmetric and niche capabilities. Most serious threat to U.S.

Supply stimuli

- *"Information revolution"* is creating qualitative change in information environment.
  - Creates opportunity to achieve ***"decision superiority"*** through combination of *information superiority* with superior processes.
  - *Global information grid:* network-centric environment with global interconnection.
- Innovation in technology, organization, and concepts

Actions to be taken

The actions to be taken to implement an RMA on these bases are described in only the very most general of terms in *JV 2020*. Stress is laid on *experimentation*.

Results envisioned by 2020 period

- Superior *command and control* strengthened by decision superiority.
- Growing dominance of *information operations*—much more important both in themselves and as a part of other operations, as result of the information revolution.
- *Dominant maneuver,* both tactically and strategically, enabled by decision superiority.
- *Precision engagement,* also a product of decision superiority.
- *Focused logistics,* implying much greater logistic efficiency, responsiveness and foresight, arising out of the information revolution.
- *Full dimensional protection* for U.S. forces resulting from decision superiority.

General hypothetical RMA model

This is an outline of a model embodying elements found in many RMA proposals

Demand stimuli

Not significantly different from those of *JV 2020*.

Supply stimuli

- Computer and electronics advances enable major steps in weapons and warfare.
- Sensors becoming radically more capable, making the battlefield "transparent."
- Land vehicles, ships, rockets, and aircraft becoming drastically lighter, more fuel efficient, faster, and more stealthy.
- New types of weaponry—such as space weapons, directed energy beams, and advanced biological agents—will be developed and widely deployed.

Actions to be taken

Would require major reorientation of R&D and procurement. Stress on *experimentation*.

Results envisioned by 2020 period

- *System of systems:* Weapon systems capabilities greatly enhanced by seamless computer-facilitated global integration.
- *Dominant battlespace knowledge.*
- *Global reach, global power:* Great advances in mobility and agility, tactically and strategically, through improved vehicular technologies.
- But *vulnerability* to advanced asymmetric capabilities if we do not act.

Owens' "American RMA" model

This is an outline of a model of the RMA implicit in Owens' *Lifting the Fog of War.*

Demand stimuli

- Forces are being hollowed out by over-commitment and under-investment.
- Eventually U.S. will face a crisis or threat for which we will be unready.
  - China is a rising rival that has committed itself to an RMA of its own.
- Fragmented, parochial, tradition-bound structure makes U.S. forces very inefficient.

Supply stimuli

- Organizational and cultural concepts from civilian business
- Computer and electronics advances enable major steps in weapons and warfare.
- Sensors becoming radically more capable, making the battlefield "transparent."
  - Space-borne sensors are especially important and powerful.
  - Mostly commercial technology.
- Technology for huge new "mobile offshore bases" (MOB)—floating islands.

Actions to be taken

- Completely reassess and radically redefine every aspect of DoD force structure, command structure, service divisions, decision processes, and culture, to include:
  - Remove all requirements responsibilities from services and centralize at DoD level.
  - Organize all operating forces jointly; no single-service operating commands.
  - Consolidate management and operation of logistics, medical, communications, and intelligence support completely. (Frees $20B/year to meet investment needs.)
  - Train and educate jointly to eradicate divisive splits between services.
- Accelerate R&D on *integrated sight* initiatives and *MOB.*
- Major base closure and realignment; move to joint basing
- Realign DoD, State Department, and Intelligence Community to form coherent whole.

Results envisioned by 2010 period (*rather than 2020*)

- Greatly enhanced force efficiency and effectiveness through integration and synergy.
- *Integrated sight*—see, identify, and track *everything* of military significance at all times:
  - *System of systems*—integration is the key enabler.
  - *Advanced sensors*—especially space sensors, which will be nearly all-seeing.
  - *Dominant battlespace knowledge*—provide *conclusive verification* of targets
  - *Near-perfect mission assignment*—always the right weapon for the right target.
  - Immediate and complete battle assessment.

The *age of aging* and *real oil crunch* RMA model

This is an outline of my model of the RMA.

Demand stimuli

- The *age of aging* will put increasing pressure on resources.
- The looming *real oil crunch* will bring the oil-producing areas around the northern Persian Gulf under enormous pressure, and intensify U.S. security concerns there.

Supply stimuli

This vision of "revolution" is driven by perceptions of need. No supply stimuli as such.

Actions to be taken

- Build consistent long-term program for continuous productivity improvement.
  - Both in businesslike *and warlike* activities.
  - Internal and external *political consensus* is a key factor.
- Improve the technology of vehicles and the major things we need them to carry.
  - Affordable capabilities to quickly move strong forces halfway around the world.
  - Mobility to take advantage of information for decisive combat results.
  - … but don't slack off on exploitation of information technologies.
  - Mold organizations and processes to take full advantage of technology.
- Build a real science of military affairs, so we can be more certain of what will work.

Results envisioned by 2030 period (*rather than 2020 or 2010*)

- Costs (relative to capabilities) cut 40%+ to make defense affordable in age of aging.
- Affordable capabilities to decisively prevent unfriendly domination of key oil-producing regions as the real oil crunch intensifies threats to them.