Fixing Defense Acquisition Programs

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The Department of Defense (DoD) spends more than $100 billion per year on procurement of durable items and more than $75 billion on research and development (R&D). It all comes under the general heading of *acquisition*. There is a substantial amount of quite mundane stuff in this, but the great majority of the money is spent on technology goods, very often as part of major systems. The public and Congress generally believe that there is a good deal of waste in all this, and this is a case where the popular view is not altogether wide of the mark. It is also generally believed that much of what is developed doesn't work as well as it should, and there is some truth in that as well.

I have a great deal of experience with defense acquisition, and no axes to grind, and I've decided to try to explain briefly and simply what the fundamental problems are, in terms that do not demand prior familiarity with the military's acquisition system, and to lay out the most important steps that can be taken to make it better.[1]

The acquisition problem

DoD, at congressional insistence, keeps report cards on itself in its role as a manager of acquisition programs, called the Selected Acquisition Reports (SARs). The most recent set of these reports, as this is written, were issued in September, 2008.[2] They cover programs with a total estimated cost of more than $1.6 trillion, a number that commands attention even in these times. Actually,

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[1] For an authoritative overview of DoD acquisition from the management perspective and pointers to the major directives see *Introduction to Defense Acquisition Management*, published by the Defense Acquisition University. It is updated from time to time; at present the latest edition is the 8th, dated December 2008. Available at http://www.dau.mil/pubs/gdbks/idam.asp.

[2] DoD, "Selected Acquisition Report (SAR) Summary Tables," September 30, 2008.

they were originally supposed to have cost considerably less – DoD estimates that on average their cost has increased by nearly a quarter. And others grade DoD more harshly than it does itself, in part because the SARs move the starting line from time to time in ways that can obscure how much growth there really has been.

The watchdog Government Accountability Office (GAO) also tracks DoD's management of acquisition. Its most recent summary shows that on average program R&D cost has increased by a swingeing 42%. And in 42% of cases the acquisition costs per unit have increased by more than a quarter.[3]

As I'll point out later there are inherent risks in many of the kinds of things DoD needs to accomplish and we have to expect that some things won't work out. But the problem has been getting worse. Just eight years ago, for instance, the GAO found that the average R&D cost growth was 27% rather than today's 42%, and that the proportion of programs whose unit costs had grown by more than a quarter was 37%, as contrasted to the current 42%.[4]

Outgoing Bush Administration defense acquisition chief John Young, a former Capitol Hill staffer, fired off an angry rebuttal to the latest GAO report arguing that the problems are all statistical illusions and anyway they hadn't come on his watch.[5] But to most people it appears that DoD's management has been getting worse, or certainly no better.

Meaningful comparison of disparate programs over time involves many pitfalls, making categorical statements hazardous. A recent careful statistical analysis concludes that, "[D]evelopment cost growth in the past three decades has remained high, with no significant improvement."[6] Moreover, the same work suggests that

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[3] GAO, "Defense Acquisitions: Assessments of Selected Weapon Programs," GAO-09-326SP, March 2009, p. 7. These percentages would be considerably higher if the GAO counted from when DoD first committed to the programs, rather than from the date at which plans are first fully defined.

[4] *Ibid.*, and *idem*, GAO-08-467SP, March 2008, p.7.

[5] GAO-09-326SP. USD(AT&L) memo for SecDef of 31 Mar 2009, subject, "Cost Performance on Defense Acquisition Programs."

[6] Obaid Younossi, et al, *Is Weapon System Cost Growth Increasing? A Quantitative Assessment of Completed and Ongoing Programs*, MG-588-AF (Santa

the variability of cost growth has grown, meaning that there is a higher chance that the real cost will be a great deal higher or lower than originally estimated.

More detailed examination at the program-by-program level bears this out. As an acquisition official in DoD in the 1970s and 1980s, I thought I had seen every conceivable form of acquisition disaster, but now I know how wrong I was. Some of the things I find in tracing out the histories of today's programs boggle even my imagination. They are no different in principle from the earlier disasters, but have been allowed to get much further out of hand. If we are ever to have an affordable and effective defense for our nation we absolutely must get control over defense acquisitions.

This is about more than controlling costs. In the great majority of cases the cost growth reflects unforeseen (but not necessarily unforeseeable) technical problems, at least in large part. And in many of them DoD has ultimately given up the struggle and accepted serious limitations that affect military capabilities because it simply cannot solve the problems.

Is all this result of incompetence on a vast scale? In a word, yes, in a sense it is. But there is much more to it than that. And in any event, ranting against incompetence will accomplish little.

Monica: RAND, 2007) http://www.rand.org/pubs/monographs/MG588/, p. *xx*. For further comparisons see, e.g., Mark V. Arena, et al, *Historical Cost Growth of Completed Weapon System Programs*, RAND Technical Report 343, 2006; David S. Christensen, David A. Searle, & Caisse Vickery, "The Impact of the Packard Commission's Recommendations on Reducing Cost Overruns on Defense Acquisition Contracts," *Acquisition Review Quarterly*, Summer 1999, pp. 251-62; Vince Sipple, Edward White, & Michael Greiner, "Surveying Cost Growth," *Defense Acquisition Review Journal*, January–April 2004, pp. 79-91; and James P. Smirnoff & Michael J. Hicks, "The Impact of Economic Factors and Acquisition Reforms on the Cost of Defense Weapon Systems," *Review of Financial Economics* 17 (2008): 3–13. I don't think any is without methodological flaw, but the more serious problem lies in the quality of the data they use. To assemble a data set that would truly be meaningful for analysis would be a massive task, and maybe completely impossible given the limitations of the records. But as far as they go, none of them show any evidence of real improvement. The most optimistic study is Smirnoff & Hicks, but their data clearly cannot bear the weight even of their analytical apparatus, with all its limitations, and even they show only slight and limited improvements.

To see clearly why problems are so pervasive in defense acquisition programs (and in other acquisition programs in other agencies) it helps to look at a great number of them, so the patterns become apparent. For better or worse, I have had occasion to examine scores of them at first hand, and hundreds more in some detail. I began my study in 1958 as a 19-year old engineering assistant on the night shift of a major defense contractor, while working my way through UCLA, and have continued it with few breaks ever since. Along the way I spent some years as a defense official with responsibility for more than 20% of all of DoD's acquisition programs at the time. And I also worked in the defense industry, directly involved in some major programs and at higher levels looking across all of the programs of one of the very largest DoD contractors.

Some things have changed in the past half century, but fewer than might be imagined. In 1988, the Armed Services Committee of the House of Representatives published a 993-page review of all the major government reports on defense acquisition reform published over the preceding 40 years.[7] A summary bearing the signature of the committee's chairman, Rep. Les Aspin, stressed the degree to which all the various commissions had found very much the same pattern of problems, and how little had been accomplished in making major changes.

A few years later, Mr. Aspin became the Clinton Administration's first Secretary of Defense. In his brief one-year tenure he launched some efforts at reform of his own. These were directed by his deputy, William Perry, who continued to pursue them in his own tenure as Secretary after Mr. Aspin's departure. There have been periodic efforts at reform since then.

Nevertheless, as the GAO reports show, the situation has not improved significantly. Indeed, if we were to update the 1988 report to reflect what has happened in the interim, we would find it had grown to 1,500 pages or more, but we could use very nearly the same summary as the one Mr. Aspin signed.

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[7] *Defense Acquisition: Major U.S. Commission Reports (1949–1988),* Volume I, November 1, 1988.

So here we have a problem that a lot of smart people have been working hard on for more than 60 years – many more, actually – without seeming to make progress on solving it. The reasonable response might seem to be despair and resignation. It is true that there is no way to fix acquisition on a permanent basis – there are possibilities for fundamental systematic improvements in some areas but most of what is needed is continuing determined and intelligent effort. The resigned "stuff happens" approach to acquisition problems in fact has been responsible for much of the worsening trend over recent years. We need to reinvigorate efforts at control, while building better structures to support it.

I frankly do not have a formula for making acquisition programs succeed. But half a century of experience with hundreds of them has taught me a great many ways that they can fail – and I have augmented my own experience with study of the history of many other programs. My experience in engineering of complex systems shows that systematically identifying and avoiding known paths to failure can go a long way toward easing the path to success. So here I am going to outline the major paths to failure in defense (and non-defense) acquisition programs and what we can do to try to block them.

Original sins

If you start off for the wrong destination, all roads lead to failure of one kind or another.

Broadly speaking, a defense acquisition program begins in one of two ways, a military requirement or a technology opportunity. A military requirement is simply a statement by someone in authority that he thinks there is a need for a certain system. Authority is important in big organizations in general and in the military in particular, and a lot of deference generally gets paid to military requirements. But the people in authority are human and it is fairly common to find that a "requirement" doesn't amount to a great deal more than a casual wish expressed by an important person – call it a "desirement."

Military requirements come first in the great majority of programs, but some are driven by a technological discovery that seems to

offer potential. The classic example is the atomic bomb. It was a letter written by a bunch of physicists, saying, “we think we might be able to do this,” that touched off the Manhattan Project. That was before my time but I was involved with stealth and with a number of other things that started in comparable ways. Most of the technology-opportunity projects I’ve been associated with are things most people have never heard of, and hopefully will remain in ignorance of for a long time to come. Like the Manhattan Project (so named because it had nothing to do with Manhattan, and thus the name revealed nothing about the project) these tend to be carried out through secret efforts, in the hope of achieving surprise.

I’ll return to the subject of technology-driven programs later, but for now I want to focus on the much more numerous (and usually more expensive) requirements-driven programs. A little thought shows the problems latent in a purist approach. Suppose, for instance, that General X decides that there is a requirement for a time-travel machine. It is easy to see how valuable this might be – imagine if we could go back and divert the young Osama bin Laden into some more useful endeavors. There is one problem, however: no one knows how to make such a device, and so far as modern science can tell it’s not even possible.

I’ve never actually heard a military authority call for something as flatly impossible as a time machine, but I have seen cases that came closer than was very sensible. In principle, requirements are supposed to be verified as technologically feasible before they are accepted as authoritative. But if a powerful official wants something badly enough and it is not too obviously outlandish then someone with seemingly good technical credentials can usually be found to vouch for it. This is all the more so because there can be rewards for pleasing powerful officials and sanctions for displeasing them. In fact, being in on the start of a major program can have many rewards, tangible and intangible alike. And if you

get out in time, the rewards may be substantial and enduring even if the program ultimately does not succeed.[8]

In the case of technological initiatives, it is necessary to secure a military requirement statement in most cases before the project can proceed very far. This is often far more difficult than to find someone to vouch for the technical feasibility of a requirement. That makes a certain amount of sense in that military requirements often call for relatively incremental improvements on existing systems while proposed technological innovations frequently presuppose some fairly radical changes in the existing military order. But it certainly is a lot more common to see promising technological ideas balked or delayed through unwillingness to formulate a military requirement than the other way around.

There is also supposed to be an analysis to show that the requirement reflects a real need. This is usually pretty cursory, often amounting to little more than an assertion of military judgement. It makes no difference in the kind of accounting for success and failure agencies such as the GAO usually do, which generally take the importance of the requirement for granted. But failure to take this seriously can contribute to decisions to proceed with systems which meet no important need and are likely to become obsolete and unwanted before their time.

Faulty foundations

Before proceeding with development there is supposed to be careful analysis and assessment of prospective effectiveness, costs, and technical risks. Advocates invariably grumble about “paralysis by analysis,” complaining that it adds cost and delay but no value. Sometimes these analysis and assessment tasks are skipped or evaded on such grounds. It’s a lot like trying to erect a building without foundations.

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[8] A timeless joke among engineers has it that the phases of any major project are: 1. Wild enthusiasm, 2. Disillusionment and despair, 3. Blind panic, 4. Search for scapegoats, 5. Punish the innocent, and 6. Reward the uninvolved. I first heard this decades ago and it has gone the rounds of every project I’ve been involved in since, with some variations.

(The advocates of a program generally are diverse, including members of the sponsoring service or organization, those who are convinced of its importance to the nation, those who expect to benefit from it, and (depending on its nature) those gripped by romantic enthusiasm. Opponents usually have motivations converse to these. As always in human affairs, views invariably reflect a mixture of pure cognition and emotion, and often are dominated by the emotional component. Emotional attitudes toward the authority represented by the sponsor often play a major role.)

The effectiveness analysis will usually compare several alternative concepts under several different scenarios. Usually the requirements sponsor already has a favorite concept which it is determined must win the analysis. Once a scenario is settled upon an experienced analyst usually can guess pretty accurately which concepts will look best (although not necessarily by how much). Often the sponsor will already have had an in-house or analysis-for-hire outfit do a private effectiveness analysis so it can tell which scenarios it needs to back to get what it wants. Thus there can be a tug of war over which scenarios are to be used, with the outcome frequently determined by how much influence the sponsor organization can exert. One way and another, it is rare for the effectiveness analysis to sink the alternative that the sponsor prefers.

Conflicts over cost estimates are all but universal, with the sponsor again usually emerging as the victor – even if only a Pyrrhic one. To form an estimate of costs one breaks the work involved down into its component pieces and projects the cost of each based on the most comparable work in the past. For instance, if it is estimated that a new airplane's airframe will weigh 20,000 pounds and experience shows that airframes of this type cost $1,000 per pound to build then the estimated cost of the plane's airframe is $20 million. Similar principles govern estimates of engines, systems, avionics, etc. Naturally, real estimates are much more complex, but the principle is broadly the same.

The cost factors, such as the $1,000 per pound figure, usually are determined through statistical analysis of the actual costs of

previous work of similar type.[9] The characteristics of the system, such as the estimate of 20,000 pounds weight, will be the result of preliminary engineering design studies. Needless to say, there is a great deal of room for disagreement about both.

Usually there is no serious disagreement about the statistical analysis of cost factors, but much about which experience is truly relevant. There is a *leitmotiv* which runs through advocates' discussions of such issues and many others: *this time we will do it better*. We will use better technology, we will motivate contractors more effectively, we will avoid the pitfalls we've fallen into in the past. Of course there really should be something in promises like these; the question is, how much? Sometimes it is a great deal, and sometimes very little.

DoD has its very own corps of cost wonks, the Cost Analysis Improvement Group or CAIG.[10] The CAIG's estimates often have been considerably lower than the final figure, but they rarely have significantly *over*-estimated costs. Yet in reading the histories of programs that have run into cost growth we find in case after case that the top decision-makers in DoD agreed to proceed on the basis of estimates that the advocates preferred – estimates invariably lower than the CAIG's. DoD has difficulty in rejecting the lower cost estimates because generally it is the service that is advocating the program that will be responsible for managing and executing it.

So why would a service that is going to have to execute a program want to start with an unrealistically low cost estimate? I've had the opportunity to ask that question of a number of senior military officers and officials, people I knew fairly well, being careful to put it in a tone of friendly neutrality. Their answers, in rough order of decreasing prevalence, came down to these:

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[9] For an illustration of the process of determining the proper relationships for estimating the costs of a high-technology system see Obaid Younossi, et al, *Military Jet Engine Acquisition: Technology Basics and Cost–Estimating Methodology,* MR-1596-AF (Santa Monica, California, RAND, 2002) http://www.rand.org/pubs/monograph_reports/MR1596/.

[10] Donald Srull, ed., *The Cost Analysis Improvement Group, A History* (McLean, Virginia: Logistics Management Institute, 1998).

1. By adopting a "tight" estimate we will keep the pressure on for tight execution of the program.
2. The CAIG just goes by history [which is not in fact the case]; we have made some major technological or management improvements that they are not accounting for.
3. We've structured this to put much more competitive pressure on industry than in the past, and bring in new suppliers with fresh ideas.
4. We have a much stronger partnership with industry than we've had in the past.
5. [Very rarely] If the costs look larger it will be harder to sell to Congress.

Of course the last one is an admission of deceit, however well-intended, and people might very well be reticent about such motivations even when they are strong; there is widespread suspicion that cost estimates to Congress are often deliberately "lowballed." There seems to be no way to prove this, one way or another.

The other four reasons all sound plausible and not discreditable, but the history of such things suggests strongly that they have almost always been wrong. The CAIG cost estimates are a quite imperfect guide, but the best there is.

Does it really make a difference what estimate is used? Isn't it enough just to go ahead and do the best you can? Unfortunately, it truly does make an important difference. If a program runs into unbudgeted cost growth then it is necessary either to cut back its efforts or to reduce other programs. It is far too facile to say that there is always a lot of waste elsewhere in defense to be cut into for savings; in fact it is practically impossible to identify candidates for savings in the immediate term without *causing* a great deal of waste.[11]

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[11] For a good example of what is involved in finding real opportunities for savings see Jino Choi, et al, "Improving Navy's Buying Power Through Cost Savings" (CNA, Oct 2006) www.cna.org/documents/D0014799.A2.PDF. As the authors stress, virtually everything they talk about represents a long-term effort that involves some uncertainties and risks, and in many cases would require up-front investment.

Cutting back is probably the most common response to program cost growth, slowing the program and stretching it out. Delaying the delivery of needed equipment can force costly gap-filler measures. And prolonging a program means paying its overhead costs for longer, raising its total cost. But the real damage from lowball initial estimates can be far worse, as I'll outline shortly in connection with risks and testing.

All of the preliminary assessments depend on a preliminary or conceptual design of the system. Even if this involves no advances at all in technology or design over previous systems there are always risks that things will not go as planned in building anything. But if advances are planned the risks grow, and the larger the advances the greater the risks.

A crucial part of the foundation for a new program is supposed to be a thorough assessment of the risks, and careful plans for how they are to be managed and reduced. If the risks are high then there is supposed to be enough R&D beforehand to get them well under control before system development begins. The remaining risks – and there are always some – are to be addressed, assessed, and dealt with as early in the program as is practicable.

Fundamentally the risky aspects of the program have to be put to the test, to see how bad they are and try out solutions. If the risk is serious and involves something critical to program success then it may be desirable to work on several potential solutions at once. Prototypes of critical components have be designed and built and then subjected to a variety tests to determine how well they can meet all of the needs. If they fail then the problem has to be analyzed so that a new round of designing, building, and testing can begin. It's expensive and time-consuming. And you cannot be sure in advance how long it will take or how much it may cost.

It is very easy to underestimate the risks at the outset. One well-known story is nuclear fusion for power generation. Half a century ago it was widely believed by experts to be within grasp. After fifty years and tens of billions of dollars it remains an attractive concept, but the list of identified yet unresolved risks is longer than it was at the beginning.

Indeed, if you know something substantial about a technology you are much more likely to be able to see its risks. Moreover, those who have been through previous development efforts generally are more alert to risk potentials even in unfamiliar technology areas. Very frequently, however, those who make the critical decisions about DoD acquisition programs have relatively little direct experience in comparable development efforts. (Indeed, it is very difficult to find anyone who has had relevant experience in the whole range of kinds of systems DoD develops.) Unless they are awfully careful to listen and pay attention to those who have developed sharper judgement of risks through education and experience, it is very easy for them to be drawn in by siren songs of enthusiastic promoters.

DoD has had persistent trouble with risk assessment, and careful investigations of troubled programs almost always reveal one or more cases where there was warning of a risk but the warnings were dismissed or papered over. Often these foreseeable but unacknowledged risks play major roles in unsatisfactory program outcomes.

It might seem that advanced electronics programs would be especially prone to major risk problems, but this has not been the experience. The most costly problems tend to occur with the things that move – spacecraft, missiles, aircraft, ground vehicles, and ships. In engineering terms these are all lumped under the heading of *vehicles*.[12] Vehicles are generally the most expensive things that DoD develops and buys, and they tend inherently to have high risks.[13]

Many of the costly risks in a vehicle tend to be linked in one way or another to weight. Most of the technology advances pursued in vehicle programs are aimed at cutting weight, at least in large part, and if they don't work then weight pays the price. Of course this is more true of some kinds of vehicles than of others – weight

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[12] *Vehicle* is a word deriving from an ancient root word, universal in Indo-European languages, denoting carrying or something that carries.

[13] Having been involved in a number of both vehicle and electronics programs I understand why vehicles tend to run into higher risks, but it is a complication we really don't need to go into here.

problems are much more serious for spacecraft, for instance, than for low-speed ships. But even for ships, accurately determining the weight at the outset plays a crucial role in program success.

Weight is particularly critical because it is multiplicative: more weight begets still more weight. In one advanced aircraft program I was involved with, for instance, we found that on average adding one pound somewhere on the plane resulted in a 4½ pound increase in total weight. Worse still, a substantial increase in weight may force fundamental changes. For instance, it may be necessary to change to more powerful engines, or to make costly modifications to the engines to increase their power output. And even if it can be dealt with somehow in engineering terms, a weight increase can reduce the military effectiveness of the system, sometimes dramatically. For instance, in the case of a ground vehicle a weight increase may mean that it can no longer be carried in common airlift aircraft but must be transported instead in more costly and inflexible ways.

There is no real equivalent of the CAIG for providing independent assessments of risk and critical engineering characteristics such as weight. The officials who hold responsibility in these areas do not have adequate professional staffs of their own to collect the information and do analyses, and very often must rely on work by those who have incentives not to find problems, or have too few resources to do a good job.

The CAIG's estimates of costs can only be as good as the information it gets about weight and risk. Sometimes the CAIG is able to identify weight and risk issues on the basis of its experience with other programs, but it currently is not equipped for comprehensive assessments in these areas, and not chartered to do them.

Industry and execution

Once the program is approved to go ahead it is put out for bid. Fools will rush in where those with experience fear to tread, and no program of any complexity whatever can safely be awarded to the low bidder simply on the basis of pure price competition. In most cases the award goes to the firm which can credibly offer to

complete the program satisfactorily at the lowest cost to the government. When high risk is involved, it may be necessary simply to award contracts to the firms that seem to have sound ideas about how to resolve it and the technical resources to implement them, with little consideration of price.

Very few people in our society entertain serious doubts about the benefits of free, competitive markets as the best way to achieve economic efficiency. To the most ardent advocates it seems as if major development and acquisition contracts should be awarded on such a basis. The model they usually have in mind would involve writing very exact specifications for the final performance required (but not how the system should be built) and then holding a free and open competition, selecting the winner strictly on price.

In fact, this was very much how nearly all defense-related acquisition programs were conducted in the 19th century and well into the 20th. Use of cost-reimbursable contracts (CRCs) during World War I raised public and Congressional fury when it was revealed that some firms had made what were represented to be excessive profits.[14] As a result, the military returned to advertised-bid contracts immediately after the war.

But the advent of the airplane as a major item of military procurement raised problems for which solutions never could be found under the old system. The airplane and its engine were simply too complicated and involved too many risks and unknowns. After a string of colorful procurement disasters, new legislation was enacted in 1926 which permitted negotiated contracts with restricted competition for aeronautical matériel. Today's contracting environment for aircraft and other technology systems has evolved from that origin.[15]

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[14] CRC is the blanket category covering all forms of contract in which the contractor is paid for his costs, or some stipulated portion of them, and possibly a fee (profit) as well.

[15] The history up through World War II is covered thoroughly in I[rving] B[rinton] Holley, Jr., *Buying Aircraft: Matériel Procurement for the Army Air Forces*, United States Army in World War II: Special Studies (Washington: Department of the Army, 1964).

In World War II contracting for high technology shifted largely to CRCs, with close government supervision and financial incentives for performance. This was the period of the lowest aircraft prices in history, relative to weight, reflecting the huge volume of production. Much of this approach carried over into the 1950s, a period of extraordinary technological progress but also some extraordinary cost growth and spectacular program failures. When business executive Robert McNamara took over as Secretary of Defense in 1961 he initiated major contracting reforms, with very mixed results. Many approaches have been tried (or re-tried) since, with little evidence of overall improvement. Despite many charges of tremendous "fat" in defense contracting, repeated studies have failed to find evidence that major defense contractors consistently make above-normal returns on investment.[16]

Not so long ago there were many more major aerospace, heavy electronics, and shipbuilding firms than there now are. In the period of contraction that followed the Cold War, senior DoD officials encouraged mergers, with the result that in many areas there now are only two or three firms that compete for business – or in some cases only one. The competitive situation is not helped by the tendency (also with official encouragement) for firms to form teams of two or more companies that join to pursue a contract. Under these circumstances it is extraordinarily difficult to see how an environment of wide-open price-based competition could ever be arranged for major defense systems.

Typically, requests for proposals (RFPs) on major programs are very lengthy and complex. In principle they are supposed to tell the contractor what the need is rather than how to fill it, but it is not

[16] I have been following studies of defense industry profitability for 40 years and never have seen one that concluded defense firms made super-normal profits over an extended period. For a clear-minded recent survey see Scot Arnold, "Does DoD Profit Policy Sufficiently Compensate Defense Contractors?" *IDA Research Notes*, Fall 2008, pp. 13-15. For one aspect of the performance of defense firms compared to other private industry see Maryellen R. Kelley & Todd A. Watkins, "Are Defense and Non-Defense Manufacturing Practices All That Different?" Second Klein Symposium on the Management of Technology, Smeal College of Business Administration, Pennsylvania State University, September 15-17, 1997.

always possible to make a clear distinction. The proposals will each generally run to many thousands of pages. They are evaluated by teams of government personnel, hopefully possessed of considerable expertise, who rate them on a variety of factors relating to technical approach, management, experience, costs, and others as may be relevant. A top official usually acts as the source selection authority, generally based closely on the proposal evaluations.

Each proposal for a major program generally requires 100,000 labor hours or more of effort in research and writing, virtually all by engineers and other professionals. The cost is borne by the government in one way or another – the industry has no source of "free money." It is tempting to take shortcuts in the interest of speed and economy, and in selected cases this can yield very good results. But it can also be very costly if the result is a contract that turns out to require major revision.[17]

Once contract execution begins we enter the domain of program management. Experience has shown over and over that the management of major programs has to be very tight and rigorously organized. All aspects of progress must be tracked very closely and continuously and vigorous corrective action must be initiated whenever unanticipated problems emerge. At the same time, the environment must be conducive to creative and imaginative effort, particularly on the part of the engineers who will be working to extend the frontiers of knowledge and practice in their fields. Program management is a great skill, a product of native talent and inclination, plus intensive training and experience.

As the program manager must understand and take responsibility for all the major decisions in the program, he or she must have a firm grasp of the technical issues, user needs, economics, and legal and regulatory demands. The program manager should draw upon

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[17] For a succinct comparison of two quite different programs in which acquisition reform measures had very different outcomes see Dominique Myers, "Acquisition Reform – Inside the Silver Bullet: A Comparative Analysis – JDAM Versus F-22," *Acquisition Review Quarterly*, Fall 2002, pp. 312-22. The article makes a strong case that policies must be tailored to the circumstances of the program in order to achieve their objectives.

and depend on the inputs of technical experts who are able to study these more deeply, but must be able to understand what he or she is being told thoroughly and be able to set it in the context of all the other factors.

In DoD the usual practice is that the top program management jobs are filled by military officers. Civilians are also employed, but their prospects for gaining top jobs generally are limited. The military officer system involves rotational assignments, and it is obvious that broad experience such as this can provide is crucial for developing the wide range of knowledge and skills that program management demands. But military officers must retire relatively young – generally in their early fifties for colonels or navy captains, or no later than age 62 for the admirals and generals who run some of the larger and more critical programs.

In the great majority of cases, this means that a military program manager will have a tenure that is significantly shorter than the decade or more that the development of a major system typically requires. Often, heading a major program will be the officer's last military job, and since retirement comes young he or she will necessarily be thinking about a post-retirement career at the same time. Studies of acquisition have consistently urged more continuity in program management, and Congress has mandated it, but it is very difficult to accomplish this within the structures of the military officer personnel system.

The contractors also will have their own program managers, who report to the government program manager and manage at a more detailed level. Generally a program manager in industry will attempt to review progress and problems in all aspects of the program – which may involve the efforts of thousands of people – at least on a weekly basis, and often daily. Industry ordinarily has much more continuity of program management, the fruits of its more flexible personnel and incentive systems. Sometimes industry

program mangers are former officers or civil servants who have retired from program management careers in government.[18]

It is during development that the greatest cost growth usually takes place, or at least comes to the surface. There are a number of possible reasons, and usually several of them will operate in a program, including

- Industry "buys in" by deliberately bidding less than the program could reasonably have been expected to cost.
- The contractor fails to fully understand what the implications of the RFP truly are, or understands them in a significantly different light than the customer does.
- DoD changes the requirements after the program is started in ways that not only make the development intrinsically more challenging but require back-tracking and re-sequencing of work.
- The government cuts funding below the levels anticipated in the plan, requiring a stretchout which results in inefficient operation.
- Optimism about risks turns out to be ill-founded, demanding corrective action or other costly adjustments.
- Technical problems emerge that no one could reasonably have anticipated.

Unforeseeable technical problems truly do occur – I've rarely known a program that did not encounter some. They are a major test of how wisely the government has chosen its contractor – and its program management staff. Ideally the contractor will have the capability to recognize and diagnose the problem at an early stage, and will either know how best to resolve it or know where to look for help. In a highly risky program a super-capable contractor can save the government a great even though its rates are higher than others. But in a low-risk program that doesn't need such skills it is usually a good deal more economical to choose a less capable contractor. (An added complication is that capability does not grow

[18] Conflict-of-interest rules generally prohibit former government program managers from working in industry in programs they have had previous contact with, but the skills involved in program management are fairly transferable.

on its own, and the government needs to think about how it will ensure that the capabilities it needs will be there when needed.)

If the risks turn out to be greater than the contractor is prepared to handle there is bound to be real trouble. Trying to insist that the contractor has a legal obligation to do something it lacks the technical (or financial) capacity to do is futile and self-defeating. The government has few good options once it has gotten into such a situation. Clearly, this underscores the importance of analyzing the risks well at the beginning, so an appropriate contractor can be chosen and so that the contractor can be primed to know what to expect.

Funding cuts are sometimes a self-inflicted wound, a near-term savings that is costly in the longer run. But they frequently are a more-or-less inevitable response to problems either with the program being cut or with other programs of higher priority or greater urgency. If one part of a program hits an obstacle it may or may not be possible to surmount, it is natural to be reluctant to continue funding other parts that may have to be re-done or terminated when and if the problem is resolved.

I can recall a few programs that made it through to the end without any significant changes in requirements, but only a few. A change does not have to affect one of the headline performance parameters to be quite significant from the standpoint of those working to develop or produce the system – changes of a more technical nature can have far-reaching effects. Everyone knows very well that changes are costly and disruptive, and program managers do not make them lightly, but it is rarely possible to avoid them altogether. Changes in military needs (or perceptions), other systems, or environmental laws or regulations can force specification changes, as can discovery of unforeseen dangers or new technology. One insidious source of change is the reliance on commercially-available hardware or software that generally can do a good deal to reduce costs – if something goes out of production unexpectedly for commercial reasons it may force significant revisions. In a complex, tightly integrated system with high levels of interdependency there can be no such thing as an insignificant change.

In a special class of their own are the changes that result from inadequate thought or preparation in the first place. They are closely related to the problem of serious misunderstandings about what is to be done, and often it is difficult to distinguish one clearly from the other.

And both are related to underbidding. The folklore is that companies buy in with the full intention of getting well on change orders which will raise the price. This may happen, but I personally have never seen an example of it on a major program. That isn't to say that I've never seen a serious example of underbidding. It happens a lot, but the causes are usually a bit more subtle and less devious.

It usually begins with a serious underestimate of cost on the part of the government, for one of the reasons we have already encountered. The contractor invariably knows how much the government thinks the program will cost, even if officials have not been advertising their ideas in public. Just as there are usually people in the government who are skeptical about overoptimistic cost estimates, so are there in industry. But there are always many people who are ready to believe in the "authoritative" figures, just as in government, and they tend to be the ones who hold the power. Skeptics in the company may warn that this is a program where the only thing worse than losing will be winning, but its leaders are likely to believe that the firm needs the business and that surely the government has good reasons for its optimism. So the skeptics are brushed aside, just as they are in the government, and the firm bids in the belief that the magic can be made to work. It has been known to happen – optimism in such matters is rarely entirely unfounded – but not often.

There are other reasons for underbidding, usually related to lack of sufficient knowledge or competence on the part of the contractor. The government process for evaluating bids and selecting the winner is supposed to catch such cases, and often does when top officials are not pressing for an unrealistically low cost.

One source both of real savings and serious trouble is use of commercial parts, systems, and standards. In many cases there are

more or less close commercial counterparts to the systems which are built for military purposes, or for critical parts and subsystems, and because they have a larger market they are almost invariably cheaper. There are usually reasons why the military system is different, often having to do with ruggedness or damage resistance. But with some ingenuity and adaptation a commercial system can often be made to serve well enough, at worthwhile savings.[19]

But sometimes the military system is different in ways that are less than obvious but very important for its unique missions. Particular skepticism is needed when a claim is made that building some complex system to commercial standards rather than military specifications will in itself save a good deal of money. There are outmoded military specifications that ought to be dropped or skipped in particular cases, but every military specification I've ever had occasion to look into in detail was originally adopted for a reason, usually as a result of some very costly lesson. Simply to say that they are all outmoded or unnecessary is dangerous, and almost always brings costly and unsatisfactory results. Unwise attempts to "go commercial" have not been a major cause of trouble in most programs, but I know of at least one case where it cost several hundred million dollars. (Again, however, it needs to be borne in mind that smart application of commercial systems and standards can save a good deal. Current acquisition policy stipulates that commercial standards will be used except in cases where military standards are necessary for specific reasons.)

The end game

The main product of any development program will be test articles and prototypes that are more or less representative of the final

[19] The Joint Direct Attack Munition (JDAM) program was an outstanding and highly successful example. For much detail about how commercial parts and technologies were made to meet military needs in this case see "Joint Direct Attack Munition (JDAM)" in Office of the Deputy Under Secretary of Defense, Acquisition Reform, Pilot Program Consulting Group, *PPCG 1997 Compendium of Pilot Program Reports*. But the Joint Air-to-Surface Standoff Missile (JASSM) program, conducted under very similar groundrules and under the direction of the same Air Force team, has been a great deal less stellar in performance. To understand why, it is necessary to look carefully at the differing needs of the two programs.

intended system, or portions of it. These must be subjected to rigorous testing to see whether they meet the specifications to which they were originally designed – and if not then what needs to be done better. This is *developmental test and evaluation* – DT&E.

The next step is building pre-production or limited-rate production articles that are intended be fully suitable for military service. As soon as possible these should be put though *operational test and evaluation*, or OT&E, intended to determine just how well they actually perform in conditions which simulate real combat (or other) demands very closely.

T&E is awkward and costly. It goes beyond the test activities themselves. What do you do with the engineers and production workers while you are testing to see if their work has been good enough? Slowing down or stopping work will cost additional money, and delay deliveries.

There are other reasons why it is resisted, however. Simply put, no one likes to be checked up upon, of course. But more than that, there is always the risk that T&E will derail a program by finding that it does not meet its specifications, or real military needs.

The result of all this is that T&E frequently is shortchanged in one way or another. Whatever it may save in the short run, this often turns out to be costly, ultimately.

Another part of the end game of a major program is initial support – all the test equipment, spare parts, training material, etc., that will be needed to get the system into successful operational service. Initial support is not very visible, but inadequate initial support provisions can have catastrophic impacts in service.

These and other end-game activities are vulnerable to cuts because the program is running short of money and needs to try to make it look as if it is not over-running (or over-running more that it already has).

Improvement

I'm not going to label this "solutions," because there are no *solutions* short of turning development over to angels rather than humans. But real improvement is a realistic possibility.

The beginning of wisdom about major programs is that their worst problems stem from lack of wisdom at the beginning. Yes, there are problems that are truly unforeseeable, but most of the problems encountered in actuality not only are foreseeable but actually are foreseen by people whose alarms are ignored or silenced.

This is not going to be fixed by tinkering with the organization or rules for acquisition programs. We are going to continue to have people at the top who are not universal geniuses and do not know everything they should. High officers and officials whose background is not primarily technical will continue to fall prey to excessive and unwarranted enthusiasm about dubious ideas. And the men and women who depend on them for promotion and preferment will continue to defer to them even if they have qualms. We may be able to make improvements in these areas through better training and procedures, but expecting fundamental change is unrealistic.

And so we will continue to have very strong pressures to start programs with seriously overoptimistic assumptions, leading to inadequate resources, unrealistic schedules, neglect of risks, and poor prospects for ultimate military worth. The only hope of real improvement is to counterbalance the pressures.

The Cost Analysis Improvement Group (CAIG) is a good start. It is by no means perfect, but if more heed were paid to the CAIG's evaluations a lot fewer programs would start out seriously underfunded. As we've seen, the underfunding begins a snowball that ultimately can bring costs that are far higher than they would be if the program had been realistically funded in the beginning.

The same is true of unrealistic schedules, and doubly true of inadequate recognition of risks. So there ought to be an expanded group, with great strengths in all of these areas, charged with making broad and entirely independent assessments of programs. Moreover, it should be required to assess the basis for the military

requirements for programs. It ought not to be beholden to those with an interest in the programs, nor to those who are charged with making the top-level decisions on them; it ought to be truly independent (as the CAIG is, to a large extent, if not deliberately suborned by top political officials). Those charged with making the ultimate decisions on major programs ought to be required to listen to its assessments, and ought to be required to state in detail and in writing exactly why they make the choices they do, whether in accordance with the recommendations of the independent review group or not.

Moreover, the decision made, together with all the information involved, should be fully open to the General Accountability Office (a Congressional agency independent of the Executive Branch), and the GAO should be staffed to critically evaluate and report on them.

There should also be a continuing in-depth review of how programs have performed and why performance has departed from plan. There is generally a reluctance to "point fingers," but objectively examining what has gone wrong (and right), and why, is crucial to learning how to do better. It should not be left to people like me, drawing on an inevitably limited (even if quite extensive) set of knowledge.

All of this will raise fears of paralysis by analysis, of the stultifying effects of regulation, of wasteful spending on staffs who are not contributing directly to projects. There may be some validity in all of these, but sober examination of acquisition history suggests strongly that such provisions could save huge sums of money – billions and perhaps tens of billions of dollars per year. And moreover, they could reasonably be expected to get systems into service more rapidly and in a more finished state.

All of this is an investment that will cost money and headaches in the near term and only pay off in the future. The payoff is surely big enough to make it worthwhile, but we also have immediate problems. The GAO's assessments of 96 current major programs totaling $1.6 trillion show many of them in trouble[20] – and I believe

[20] GAO, "Defense Acquisitions: Assessments of Selected Weapon Programs," GAO-09-326SP, March 2009.

that in some important cases they are actually being optimistic. So far they have collective cost growth of about $300 billion, and there is worse to come if strong action is not taken.

There are a number of other ways in which acquisition could be improved, worthy initiatives that would surely repay their costs and ought to be pursued. But none of them offer the same level of potential.