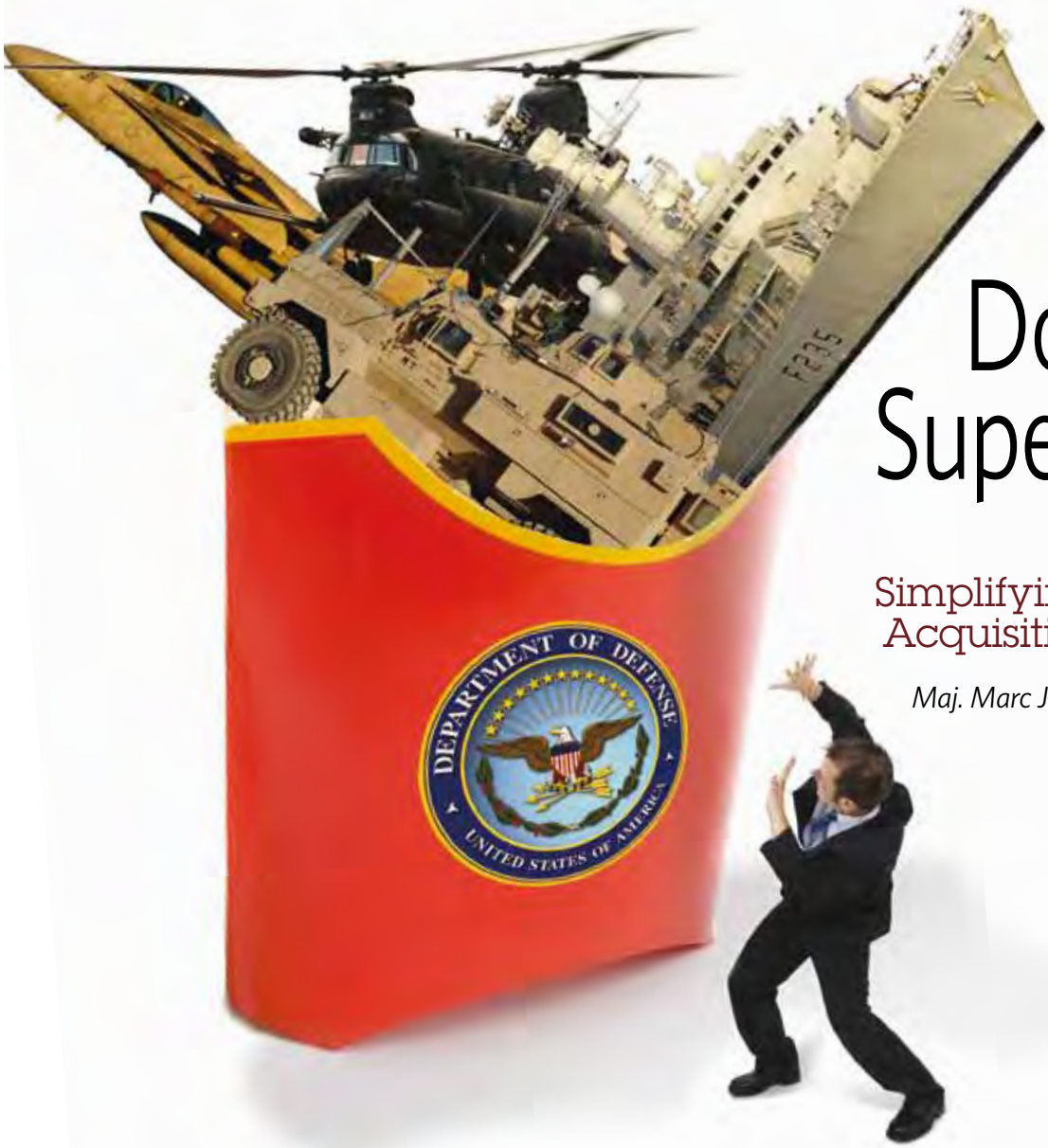

Don't Supersize!

Simplifying Defense Acquisition Reform

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In early 2009, shortly after the latest defense acquisition reform legislation, the Weapons Systems Acquisition Reform Act (WSARA) was signed by President Obama, I decided that as a defense acquisition professional, it would be good to study the history of such reform. I had been taunted by the old adage from high school history class—that those who do not study the past are doomed to repeat it.

My research found a great deal of history, with a great deal of it repeated.

The pattern was this: Studies identified problems. Panels proposed solutions. The government directed reforms. Two of the first reform studies I read referred to over 200 other studies, panels, and reports. What surprised me was how many of the ideas generated by this excess of think-tanking were implemented. Defense acquisition reform is that rare subject that garners broad bipartisan as well as cross-government support. WSARA, for example, passed both the House and Senate unanimously, despite strong partisanship on virtually every other issue.

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I wondered: "Why is defense acquisition reform so uniquely persuasive—and why do these much-agreed-upon reforms appear not to be working?"

The metrics for our largest weapons systems show a near-unbroken trend of unexpected cost and schedule growth trailing back to 1950, despite more than a dozen attempts to reform the system and eliminate the trend. Perhaps, in our desperation, we failed to understand some fundamental causes and have over-engineered solutions. While many reforms do relieve stress in the system, collectively they add stress and steer the acquisition system down the path that our tax code has taken—that of extreme complexity. To rebalance, we should ask ourselves: Which remedies will have the broadest positive effects but require relatively simple and concise actions? How do we nudge rather than pummel ourselves to positive results?

Supersize that Order, Please…

I contend that the department's tendency to supersize the scope of many programs is a root cause of unrestrained, unnecessary cost growth. This is sadly ironic since many reform efforts began by scouring Major Defense Acquisition Program (MDAP) data to discover the common issue to target, while somehow missing the realization that the size of those programs is itself that common issue!

Setting desperation aside, let's approach this idea through the lens of logic:

- Every program has inherent risk.
- Programs with more functions and more complexity have more acquisition risk, all other factors being equal.
- Larger programs (e.g., ACAT-I programs) have more functions and more complexity.

Although there can be exceptions, this logic trail generally leads to the conclusion that our largest weapon system programs are inherently risk-intensive. This increased risk, moreover, is not linear, since the larger the integration scope, the greater chance a realized risk in any one area has of propagating instability across system or process interfaces. Smaller programs have this trouble too, but they also have the implicit firewall that comes with economy of scope. Their larger peers, however, generate multiple independent pockets of risk (technical, process, schedule, integration, etc.) that end up superposing with other risks, creating program-wide instability that is more than just a sum of the individual issues.

The result can—and repeatedly has—become the program equivalent of the Tacoma Narrows Bridge disaster. Tacoma Narrows is often used as proof of how spectacularly a project can fail when instability overwhelms a system not designed to tolerate it. Just as strong winds caused that bridge to oscillate with ever growing amplitude until a cascading failure ripped it apart, complex programs lacking robust and proactive risk management are destabilized in the presence of compounding risk. Often the result is a similar cascade of failures with parts of program scope being stripped away in the hope of stabilizing cost and schedule. Sadly, though the purged system has real utility, it is often less than what was intended, is behind schedule, and has squandered resources. Thinking smaller and smarter at program initiation would be a better alternative to this.

Big Deal

Program size also factors into other defense acquisition challenges. For one, it fosters consolidation in the industrial base, leaving few defense firms able to compete for our most ambitious or complex contracts, and drawing talent from other critical, but smaller efforts. Another consequence of supersizing is the extension of acquisition cycles. Giving the same scope to a set of smaller, more focused, and independent efforts would minimize delays in delivering capability to the warfighter. Expanded integration activities require their own resources and admit new risks, a key reason why even the most effective complex MDAPs miss the department's 3-to-5-year procurement-cycle goal.

One final effect of supersizing is the burden of MDAP regulatory reporting that is automatically placed on the likely already-stressed program acquisition workforce, causing them to be more outward and backward-focused on the very programs that require the most intensive inward and forward management cynosure. In some cases, the fear of this last consequence may lead programs to accept weak assumptions of low risk, even in the face of unprecedented complexity; a course which allows for artificially low cost estimates, but just delays and worsens the inevitable (which is why we should never waive an independent cost estimate, even on an ACAT-2).

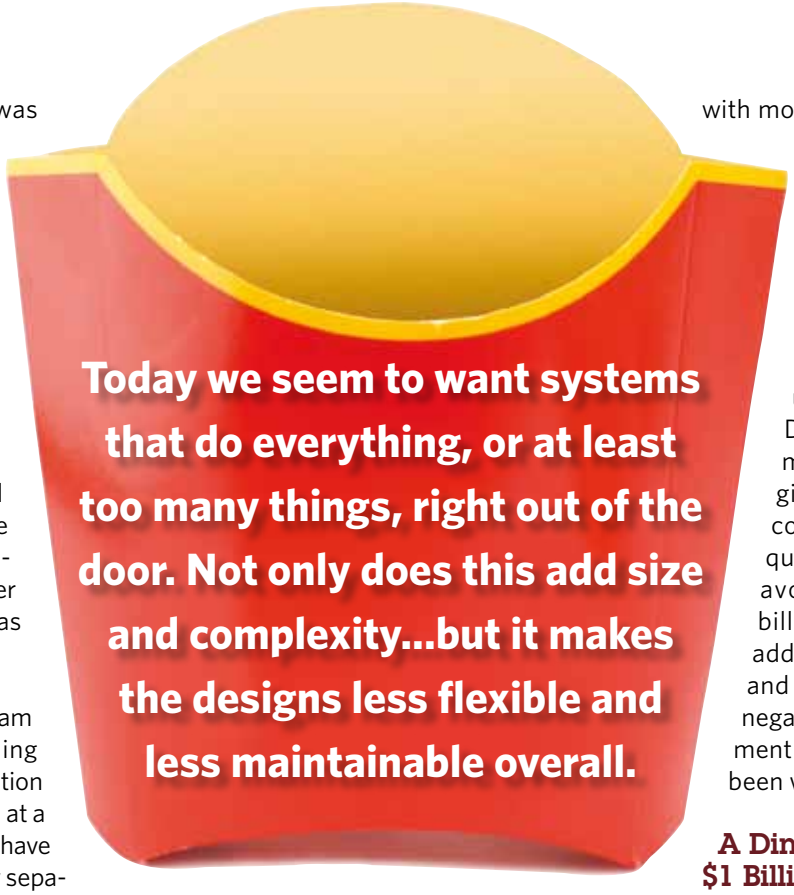
One Example from a Crowd

Concern over program obesity is validated by the history of any number of large programs, but only the Space-Based Infrared System (SBIRS) will be cited here, as it often is elsewhere. SBIRS, in brief, started out as heir to the Defense Support Program (DSP), but with three additional missions added. At least one initial cost estimate (approximately $9 billion) was shelved in lieu of one less than half as large.

The higher-cost estimate was shelved because of the reasonable belief that the program would not be authorized with such a high, though accurate, cost. The smaller estimate was chosen, and assumptions were provided to back it up, including: Software integration would be low risk; the system solution would just be an update to DSP; and only 42 days of schedule would be needed for slack across three years of development.

It didn't take long for the true risks and complexity to appear, invalidating these assumptions. The program, having been funded and structured based on assumed low risk, proved incapable of performing the necessary integration and risk management. All internal cost and schedule margins evaporated in attempts to address unassumed risks that had be-

come real issues. It was not enough. Just like the Tacoma Narrows Bridge, the instabilities continued to grow: In the first 5 years, the program re-baselined annually (four re-plans); these were followed by two Nunn-McCurdy breaches, with SBIRS Low being offloaded completely from the larger program of record, moving to another effort which itself was later restructured.



Today the SBIRS program of record is proceeding with a partial constellation on orbit, years late and at a cost growth that could have funded several entirely separate MDAP efforts. The current estimated cost is approximately $11 billion, yet the series of unfortunate results has not ended. The soon-to-be-launched SBIRS High GEO (geosynchronous) vehicles will be orbiting for years before being able to fully exercise their native capability, because the needed ground software budget was redirected to cover other unanticipated costs.

As a reformist community, we have looked at SBIRS and other MDAPs and made recommendations on how to move forward. Some key ideas include using risk-based source selection and certifying independent cost estimates up front. These are excellent must-do fixes that are in place today, but they skirt the key issue of over-scoping. Remember the $9 billion SBIRS estimate that was shelved? If we'd used that estimate and run a risk-based source selection on the same scope, that program likely would not have been approved. The SBIRS program was always executable, but it was never affordable. It was too big.

Divide and Conquer

If SBIRS was never affordable, what could we have done differently by recognizing that up front? For one, scope could have been carefully separated, perhaps aided by a shared interface control document (ICD) defining points of interoperability and minimizing dependency. Doing this could have won the option to defer the riskier elements in two dimensions: within separate programs' delineated scope and between the programs themselves.

This approach would have let the separate programs proceed on relatively independent acquisition schedules and with more bounded risks to schedule, cost and technology without altogether ignoring integration. This tactic would also have mitigated the biggest risks up front, rather than assuming them away; further, MDAP designation might have been unneeded, freeing our lean DoD acquisition staffs for a more proactive and strategic vice reactive and tactical course forward. Finally, and quite importantly, we'd have avoided a 15-year-long $11 billion-plus program that, in addition to delaying capability and consuming treasure, drew negative publicity on the department for years. That would have been worth something.

A Dime a Dozen—$1 Billion for One

Many of us have grown up as acquisition professionals weaned on supersized efforts like SBIRS and its 90-or-so sister MDAPs. However, we can't let that exposure blind us to believe there are no small-scale alternatives for many programs.

The B-52 is an example of a program that had a strong, flexible baseline design that was able to take on incremental upgrades for more than two generations, yet still has a projected operational life to 2040, an operational readiness rate three times higher than the more costly and modern B-2, and even cost less than $1 billion ($FY 2000) to initially develop.

Today we seem to want systems that do everything, or at least too many things, right out of the door. Not only does this add size and complexity, with all their accompanying effects mentioned above, but it makes the designs less flexible and less maintainable overall.

In contrast, look at the Surrey Institute and other similar centers of excellence for creating economy of capability. Surrey's niche is in designing and developing small satellites and having them on orbit in generally less than a year from design kick-off, and at a cost often in the neighborhood of $10M per satellite. The capability may not be state of the art, but it is competitive and what is launched can always be replaced with better technology in 2-3 years, as necessary. Instead of having a satellite on orbit for 15 years whose technology is outdated by the mid-point of that span, why not have a rolling wave of innovation? Furthermore, the small physical size of those satellites leads to lower launch costs and provides the option for multiple satellites to be orbiting at once.

Applied to DoD space needs, this simpler approach would allow the U.S. to accelerate its fledgling Operationally Responsive Space (ORS) initiative, and take a similar approach to unmanned ground, maritime and air vehicles (UGV, UMV, and UAVs).

Write On!

We see the problems. Hardly an issue of *Defense News, Defense Acquisition Research Journal*, or this publication goes by without discussion of some acquisition trouble or proposed solution. We can add these commentaries to the formal reports and studies I cited earlier. It seems that in the DoD acquisition community, everyone is a reformist, including those who call for a halt to reforming. After all, with reform being the rule rather than the exception for 50 years running, stopping reform is a rather radical change!

This school of thought has a point. Because MDAPs have long acquisition cycles and a lot of momentum in whatever direction they are headed, much of the effect of current reforms may not manifest results for years yet. WSARA's Nunn-McCurdy strengthening, for example, would not trigger on a SBIRS-sized program unless cost grows another $2B or so.

A Note of Irony

With this enlarged perspective on the effects of size and complexity on our programs (for better and worse), is it not ironic that our historic solution to cost and schedule growth is to make the acquisition system itself even larger and more complex? Wouldn't that incur a level of systemic risk on the government side of development for the same reasons? Instead, we must take the simplest, most strategic actions and defer any further accumulation of tactical reforms.

By employing stronger economy of scope on as many programs as possible, and otherwise applying processes already in place, we will make good progress. First, we initiate more programs, enabling a broader industrial base that includes smaller and more innovative firms. Second, we support our acquisition action officers as they seek to establish process stability. Third, instead of stretching out program leadership tours to provide continuity, we shrink the length of the programs to achieve the same result. Finally, we get incremental capabilities to the field faster and free up resources for the next generation of investment.

Limiting program size is a strategic fix, not a tactical one. It does not affect the momentum of our current largest weapon systems, but instead is insurance against another generation of resource-hungry MDAPs following in their wake. DoD acquisition professionals of the future should not be faced with 200 or more studies of why they cannot do their jobs, but rather by articles and accolades on how well they succeeded. We literally cannot afford for history to repeat itself.

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