Faster, Better, Cheaper Revisited

Program Management Lessons from NASA

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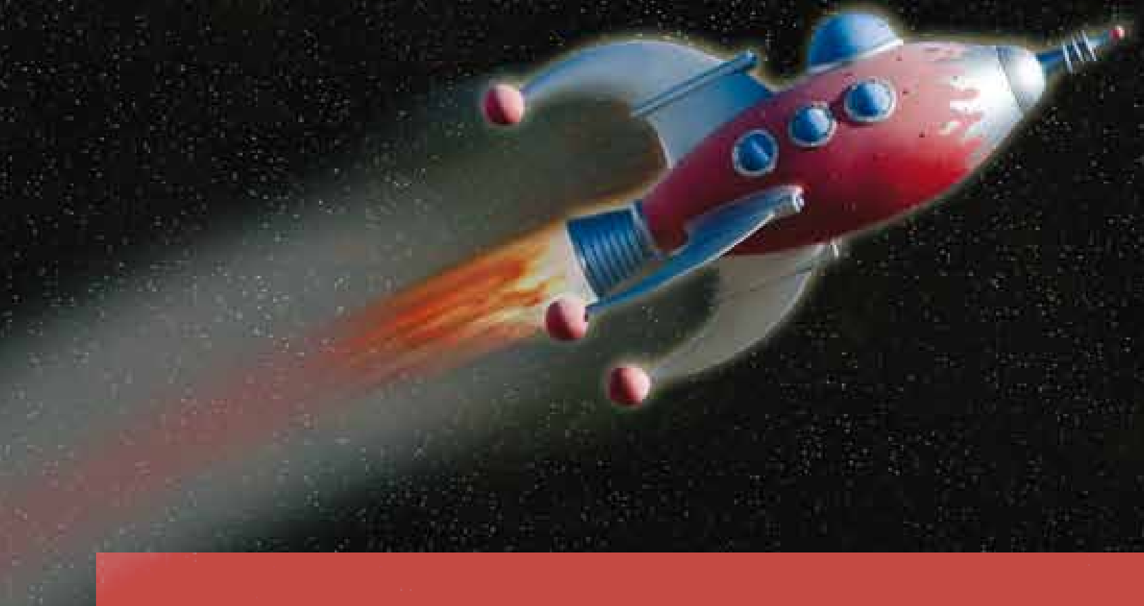

In 1992, NASA administrator Daniel Goldin began the agency's "Faster, Better, Cheaper" initiative. Over the next eight years, 16 missions were launched under the FBC banner, including the remarkable Mars Pathfinder mission. Today, however, many people look back at FBC with disparaging chuckles and wry remarks, as if it were an embarrass-

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ing failed experiment. Casual observers and serious students alike have apparently concluded that it's impossible for a high-tech project to be simultaneously faster, better, and cheaper … and that it's foolish to even try. The popular consensus on FBC is often expressed in the supposedly self-evident saying: "Faster, better, cheaper—pick two."

It turns out popular consensus is wrong.

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Looking Beyond Received Wisdom

A closer examination of NASA's FBC missions reveals an admirable record of success, along with helpful and illuminating lessons for anyone involved in developing and fielding high-tech systems. Far from an embarrassing failure or proof that program managers must "pick two," the FBC initiative actually improved cost, schedule, and performance all at once. NASA's experience provides an insightful organizational roadmap for sustaining mission success while respecting constraints of time and funding.

I should mention that NASA wasn't the only one to try FBC, but the agency flew the FBC banner prominently, long, and well. The fact that NASA's experience encompasses a complete portfolio of 16 missions, unencumbered by classification restrictions, makes it a particularly attractive and useful data set. Let's take a look, shall we?

According to Dr. Howard McCurdy's 2001 book F*aster, Better, Cheaper: Low-Cost Innovation in the US Space Program*, the 16 FBC projects (between 1992 and 1999) were "five missions to Mars, one mission to the moon, three space telescopes, two comet and asteroid rendezvous, four Earth-orbiting satellites, and one ion propulsion test vehicle." These were not simplistic backyard science projects. They were bold attempts at some of the hardest and most important unmanned missions NASA performs. The initial results were encouraging—nine out of the first 10 missions succeeded.

It is tempting (and would be fun) to spend all our time looking at a few of the FBC missions, such as the Near Earth Asteroid Rendezvous (NEAR) project. NEAR launched in February of 1996 (a mere 27 months after it was funded) and cost $122 million instead of the $200 million originally estimated. Its two-billion-mile journey produced 10 times more data than expected. As its mission drew to a close, despite the fact that the spacecraft was not designed to be a lander, NEAR coasted to a successful landing on the asteroid Eros—the first time NASA ever attempted such a feat.

We could also consider the 1997 Pathfinder mission to Mars, which cost one fifteenth (6.7 percent) of what NASA spent on the Viking Mars mission 20 years earlier. Pathfinder was the first successful attempt to send a rover to another planet, and it produced over 17,000 images. Or we could look at Goddard's Small Explorer project, which delivered six low-cost, high-performance spacecraft in 10 years. ... You get the picture. The bottom line is that nine of the first 10 missions succeeded.

We could almost stop the assessment there. The events of these years show that when NASA tried to apply FBC to 10 cutting-edge missions, including things that had never been done before, their success rate was 90 percent. That alone is enough to prove FBC is possible, but unfortunately, it's not the whole story.

Depends on How You Do the Math and What You Mean by Failure

In 1999, four out of five FBC missions crashed and burned (sometimes literally). NASA ended up with a total of six failures out of 16 FBC missions—a success rate that was deemed unacceptably low. The party was over. Indeed, a report by retired Pathfinder project manager Tony Spear states that "the current Mission failure rate is too high," a sentiment echoed in several other studies and reports.

However, if the low success rate was a central reason for cancelling FBC, it seems someone made an unfortunate miscalculation. While it is true that 10 out of 16 is 63 percent, that number is not an accurate measure of what FBC accomplished. There is much more to the story than NASA's batting average.

We've already seen that Pathfinder cost one-fifteenth of the traditionally managed Viking. Dig a little deeper and we find the pattern of remarkably low-cost programs continues. In fact, all 16 FBC projects cost less than the Cassini mission to Saturn. This means FBC delivered 10 successful missions (plus six unsuccessful ones) for less than the price of one traditional mission.

I would like to respectfully suggest that success-per-dollar is a more meaningful measurement of achievement than success-per-attempt because there is no limit to the number of attempts we can make. The only real constraint on our activity is the amount of time and money we can spend. In other words, the important thing is not how much success we get out of 100 tries, but rather, how much success we get out of 100 dollars.

Imagine with me for a moment: If a magic space genie offered to give you 10 successful programs for the price of one, would you really care that he threw in 6 failed programs too? It's still a pretty good deal. For that matter, if he only offered to give you two successful programs for the price of one, it's probably an offer you should seriously consider.

Now imagine if this magic space genie added 10,000 failed programs to those 10 successes without increasing the overall bill. Sure, that's a lot of failure and would be difficult to accept, psychologically and politically. But financially, it would still be worthwhile, wouldn't it? If we can deliver a significant number of meaningful successes within our cost constraints, who cares how many failures we also deliver?

Speaking of failure, let's take a closer look at the missions that didn't work out. Spear's report states: "Most failures… can be attributed to poor communication and mistakes in engineering and management." Such failures are arguably avoidable, but they are neither unique nor ubiquitous to the FBC method. We can easily find examples of cripplingly poor communication and epic engineering mistakes in traditional projects as well as examples of FBC projects where communications were good and mistakes were rare. Those failure modes are valid criticisms of individual programs, but not of the FBC method as a whole.

The fact that FBC's failures clustered in 1999 should also give us pause. If the method itself was intrinsically flawed, wouldn't we expect the failures to be evenly distributed? The events of 1999 suggest other explanations: Perhaps people got burned out, sloppy or overconfident; perhaps the initial successes attracted people who did not sufficiently understand FBC; or maybe NASA pushed the envelope too far, over-correcting an initial success rate that was perhaps too high. Maybe there's another explanation entirely, but the least likely explanation is that FBC project leaders should have "picked two."

One more observation: assessments of the failed FBC missions often identify complexity as a root cause. McCurdy points out that FBC went badly when project leaders "reduced cost and schedule faster than they lessened complexity." In contrast, successful programs not only operated within tight cost and schedule constraints, they also insisted on simplicity—technically and organizationally. This preference for simplicity was not an explicit component of FBC's banner, but was clearly a top priority for the people who led the successful projects.

The Burden of Proof

Moving on, alert readers no doubt noticed the FBC missions were all unmanned missions. It would be reasonable to ask whether the FBC approach could be applied to manned missions, where the tolerance for failure is lower and where the necessary technical complexity is higher. In the realm of manned missions, our magical space genie's offer of 10,000 failures is quite unattractive.

If we want to improve our outcomes, the history of military acquisition reform shows we cannot limit our changes to methods and processes, or rely solely on systems analysis and statutory reform.

And yet, the traditional, non-FBC approach does not exactly guarantee success, does it? Given the outcome of missions like Pathfinder and NEAR, is it not possible to imagine an approach to manned space flight that is faster, better, and cheaper than previous attempts? Perhaps we can't do it for one-fifteenth of the price (or maybe we could!), but even cutting the price in half would be a step in the right direction. To say that such a thing is impossible is to assume a serious burden of proof.

Speaking of proof, the main point I want to make with this article is that a high-tech program can be simultaneously faster, better, and cheaper; there is no intrinsic need to "pick two." Having demonstrated this to my own satisfaction, I must confess I chose the easiest kind of challenge. Those who say a thing is possible need provide only one example, and NASA generously provided us with 10. Those who say a thing cannot be done have a much harder task—they must prove a universal negative. To disprove FBC requires not merely establishing a universal negative, but a universal negative in the presence of 10 positives. Even in the case of manned missions, I find little support for the idea that faster, better, and cheaper is impossible.

The next logical question is how to do such a thing. This is a good question, and we once again look to NASA's experience. How did NASA manage to deliver 10 successful programs (and six failures) within such tight cost and schedule constraints? It appears the secret was to apply FBC principles to just about every aspect of the program, from engineering architectures to organizational behavior.

For example, NEAR engineers gave three-minute reports and used a simple 12-line schedule. Many so-called "good ideas" were rejected during the design phase because they would have increased the cost, schedule, or complexity of the project.

Alexander Laufer's book *Project Success Stories* quotes NEAR program manager Thomas Coughlin: "Had I incorporated even half of these good ideas, the spacecraft would never have been built. Only those changes that could be made with negligible or minimal disruption were even considered." Other FBC projects took a similarly restrained approach, limiting organizational, operational, and technical complexity as a means of minimizing expense and delay.

The bottom line? After studying the entire cohort of NASA's 16 FBC missions, McCurdy makes the following observation: "Engineers and other experts can reduce the cost of spaceflight and the time necessary to prepare missions for flight. Moreover, they can do so without significant loss of reliability. They can also do so with only modest reductions in spacecraft capability."

This willingness to make modest reductions in capability is a key aspect of FBC—and a key point of controversy. The tricky thing is that "better" is a notoriously subjective assessment. FBC leaders asserted, "A reduced capability does not mean the mission is automatically worse. A mission with one-half the capability will be 'better' if it performs that mission at one-tenth the price." This is a philosophical position, and one that no doubt led to many spirited debates between those who believe More Is Better and those who worship at the church of Less Is More. FBC was decidedly on the latter side.

For any who are tempted to argue that reduced capability does not equate to "better," I once again point to NEAR's remarkable landing on Eros. Had NASA designed it to be a lander, they would have spent more time and money to produce a more complex system with an increased design capability, but because complexity increases the number of possible failure modes, its operational reliability would likely have decreased. It turns out, the spacecraft's operational ability to land on an asteroid was demonstrated in the absence of such design additions, perhaps pointing to the superiority of systems with reduced capabilities.

After those 16 missions were completed and analyzed, what conclusions did NASA itself draw? Spear's report was emphatic: "Dan Goldin is right on with his FBC thrust." In a similar vein, a 2001 report by NASA's Inspector General Roberta Gross recommended that NASA "fully incorporate FBC into the strategic management process." This recommendation comes after acknowledging that "NASA has been using the FBC approach to manage projects since 1992," to which I would add the word "successfully." This does not constitute a rejection of FBC. It is clearly an endorsement. No evidence here of the necessity to "pick two."

The Lesson for DoD

Why did I tell you all this? Why write about NASA in a DoD magazine? It's because NASA's experience provides data that is highly relevant to the DoD's current efforts to improve defense acquisitions. If we want to improve our outcomes, the history of military acquisition reform shows we cannot limit our changes to methods and processes, or rely solely on systems analysis and statutory reform. We need to go deeper and change how we think and what we value. That's exactly what NASA did. They created a cultural framework of principles, priorities, and values, which shaped their decision making and guided their organizational behavior.

As for DoD, as long as we equate complexity with sophistication, complexity is going to eat our lunch, reducing our systems' reliability and operational effectiveness. As long as we believe adding time and money makes the project better, we're going to have overruns and delays. And as long as we believe in "faster, better, cheaper—pick two," we are going to be stuck in a self-limiting mindset, and our outcomes will suffer.

As an alternative, we might consider the skunkworks-esque FIST value set, which says it is important and good to be fast, inexpensive, simple, and tiny [note: read more about FIST in the May/June 2006 issue of *Defense AT&L*]. FIST is not the same as FBC—note the absence of the highly subjective "Better" and the explicit emphasis on "Simple." But when the FIST values shape our decision making, we end up pursuing projects that look an awful lot like the early FBC missions: small teams of talented people, with short timelines and small budgets, using simple technology to develop and field world-class operational capabilities.

Implementing things like FIST or FBC requires an understanding that these approaches are not methods or processes, but rather something akin to a worldview. They are sociological and cultural—not procedural—approaches. FBC was never a checklist. It was a way of life. And that's why it worked as well as it did, for as long as it did.

When NASA's leaders said, "It's good and important to be faster, better and cheaper," they meant it, pursued it, and rewarded it ... and for a time, people believed it. FBC wasn't about superficial modifications to the way NASA worked; it was a radical reimagining of what was possible, a cultural shift away from the idea that budget overruns and schedule slips are inevitable. Most important, it was a redefinition of what was desirable.

The DoD could do worse than adopt an FBC-like approach to acquisition improvement. Whether it's FBC or FIST or another social framework, the most effective way to genuinely change acquisitions lies, not in additional oversight or improved procedural efficiencies, but in a cultural shift. This is perhaps the hardest type of challenge, but as NASA showed, it can be done.

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