On Failure

Maj. Dan Ward, USAF
Maj. Chris Quaid, USAF
Maj. Gabe Mounce, USAF

Try as hard as we may for perfection,
the net result of our labors is an
amazing variety of imperfectness.
We are surprised at our own versatility
in being able to fail in so many different ways.

Rev. Samuel McChord Crothers, American essayist

Failure is inevitable.

That may be a strange sentiment to come from a group of wild-eyed optimists like ourselves, but it is one we stand behind with confidence. Let's say it one more time, with feeling: *Failure is inevitable*. Go ahead—take a moment to let that sentence sink in.

Unfortunately, people in organizations like the Department of Defense and NASA tend to say things like "Failure is not an option," as if such bravado could somehow ensure unmitigated, unvarnished, unequivocal success. While such a dramatic statement makes for an inspiring movie quote, it can have a bad effect in real life. We think it reveals a counterproductive fear of failure and a fundamental misunderstanding of what failure really is. The problem is that people who think failure is not an option may feel the need to call it something else when failures occur—and trust us, they occur—which can lead otherwise honorable people to dissemble, deny, and disguise failures. The truth is, failure is *always* an option. Indeed, failure is inevitable.

The inevitability of failure doesn't mean success is impossible. It simply means that given sufficient time and multiple attempts to accomplish any given objective, we can all expect a certain amount of failure. No matter how smart, talented, focused, prepared, hard-working, or lucky we are, sometimes things just don't turn out the way we planned. Failure is an

Ward *is currently assigned to the Acquisition Chief Process Office at SAF/AQ. He holds degrees in electrical engineering, engineering management, and systems engineering; and he is Level III certified in SPRDE and Level I in PM, T&E, and IT.* **Quaid** *currently works for the secretary of the Air Force, Office of Special Projects. He holds a Master of Business Administration degree and a Level II COTR certification.* **Mounce** *holds an advanced degree in electrical engineering from the Air Force Institute of Technology. He is Level I certified in T&E and PM.*

inescapable part of the human condition, and the sooner we recognize that, the better. Of course, when lives are on the line, the only *acceptable* failure rate is zero. Unfortunately, in the long run, a zero-percent rate of failure is impossible.

A vast army of experts and success gurus happily tell us failure is good and an important part of learning and growth. They trot out dusty old examples like Michael Jordan getting cut from his high school basketball team and drone on about how we miss 100 percent of the shots we don't take. Fine. They may be right; failure might be good for us, but that's not what this article is about. We are simply here to point out that failure is *inevitable*, and to tactfully observe that we all miss a certain percentage of the shots we *do* take. Whether that's good news or bad isn't important right now. We just want to help everyone recognize the reality of failure's inevitability.

The Quality of Failure

While nobody can avoid failure entirely, it *is* possible to influence the direction in which we fail. Failures may never be "good," but some failures are better than others. In his book *The Black Swan*, Nicholas Taleb suggests aiming to create "situations where favorable consequences are much larger than unfavorable ones." That is, we ought to pursue situations in which the benefits of a positive outcome significantly outweigh the cost of a negative outcome—recognizing, of course, that even our attempts to do so will, upon occasion, fail.

We invite you, dear readers, to consider two ways to improve our inevitable failures. The first is to minimize exposure to loss. The other is to ensure that any negative outcomes become learning experiences and building blocks for future endeavors. (Yes, just like the failure-is-good-for-you idea that success gurus recommend. Sigh.)

The ideal failure, we believe, is one in which exposure to loss is low and opportunities for learning are high. Such a failure, in which little is lost and much is learned, could be termed an *optimal failure*. In contrast, a *negative failure* is one in which much is lost and little is learned. The table below illustrates the differences between optimal failure and negative failure.

Failure Types	Exposure to Loss	Opportunity to Learn
Optimal	Low	High
Negative	High	Low

Acquisition project leaders would obviously prefer to succeed, but they should remember that a certain amount of failure is inevitable. And unfortunately, negative failures are arguably the only kind of failure a major defense acquisition program can experience, given the typical MDAP's enormous budget and decades-long schedule. Every time an MDAP fails, it fails spectacularly, costing billions of dollars and teaching too little, too late. That doesn't mean we shouldn't have MDAPs, but it certainly means we should be aware of the risk.

Of course, even projects below the MDAP threshold can experience negative failures, given enough years and dollars. Whenever large quantities of time and money are expended, we are exposed to significant loss. If a large percentage of participants have moved on to other projects and/or retired before the failure is observed, our opportunity to learn is low. And frankly, even if the original decision makers are still around and directly witness the consequences of their actions, it is often too late to apply any lessons learned because learning requires both *observation* of the phenomena and *timely reflection followed by action*—neither of which is likely in big, lengthy, expensive projects.

Let's get specific. From 1983 to 2004, the U.S. Army spent $7 billion developing the Comanche helicopter, then cancelled the program and had zero aircraft to show for their troubles. In February 2004, Lt. Gen. Richard Cody, deputy chief of staff, G-3, said, "If you told me six months ago that I would be standing here saying the Army no longer needs the Comanche helicopter, I wouldn't have believed you." That admirably honest statement highlights the inherent difficulty in learning from experience on a long project, and shows that we really don't know what the lessons will be until the story is finished. For nearly 21 years, the Army apparently thought things with the Comanche were just peachy, maybe even worthy of imitation. They didn't have the opportunity to learn the true lessons of the Comanche until it was cancelled. Until that moment in 2004, there's a good chance the Army was learning—and teaching—the wrong lessons from their $7 billion tuition payment to Experience University.

We're not trying to pick on the Army, but their Crusader artillery piece has a similar story, albeit on a slightly smaller scale. The Crusader took only seven years and $2 billion before it was cancelled in 2002, having delivered zero artillery. Interestingly, two months before Crusader was cancelled, C. Emerson published an article in *Field Artillery* magazine, "Crusader: Hammer for Today, Forge for the Future," in which he stated that the project was on schedule,

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on budget, and a mere six years away from being fielded. We could fill this magazine with similar stories from *all* the military services, but two is probably enough to make the case that until we see the end of the story, it is difficult to glean meaningful lessons; and the longer the development timeline, the harder that is.

In both cases, we probably gained *something*—some new technology that survived the cancellation and could be used on a future project, perhaps. A negative failure is not necessarily a total fail or a complete loss, but it's not exactly optimal either. Since failure is inevitable, we really shouldn't put ourselves into a position to encounter negative failures if we can help it. Fortunately, there are alternatives, in which our exposure to loss is smaller and the opportunity to learn is larger. Regular readers of our articles may have already guessed where this is heading.

Optimizing Failure: Think Small

We introduced the FIST (Fast, Inexpensive, Simple, Tiny) model for acquisitions in an earlier series of articles, culminating in "FIST Part 5" (*Defense AT&L*, May-June 2006). By design, FIST projects are low-cost and rapid. Unlike what happens in the traditional approach, the inevitable FIST failures are discovered before much time and money are expended, reducing our exposure to loss. FIST failures also have a high probability of conveying meaningful lessons learned because on a small team with a fast schedule, project leaders actually witness the impacts of their decisions and can directly learn from—and share—their experiences.

This approach to failure is one of the guiding principles behind FISTy approaches like extreme programming, spiral development, agile acquisition, and NASA's Faster, Better, Cheaper (FBC) initiative. We cannot dismiss those approaches because they sometimes fail. *Everything* fails sometimes; even rigorously controlled MDAPs. But when FISTy approaches fail, they tend to do so optimally rather than negatively ... and that's a good thing.

This distinction between negative and optimal failures has an important implication when it comes to accounting for failure. In the traditional technology development model, each project is expensive and takes a long time to complete. Project leaders therefore aim to prevent and avoid failure because traditional failures are negative failures, and negative failures hurt a lot. Accordingly, it makes sense to measure failure rates on a per-attempt basis (i.e., failures-per-cohort or -per-portfolio) and to try to minimize the organization's failure-per-attempt rate.

The FISTy approaches we mentioned two paragraphs ago require a different perspective on failure accounting because they produce a different kind of failure. Optimal failures, while still undesirable, are more tolerable and do not cause as much damage. When attempts are quick and inexpensive, a relatively high failure-per-attempt rate might, therefore, be more acceptable or perhaps even irrelevant. Indeed, a relatively high failure rate should perhaps even be *demanded*.

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In the early 1990s, when NASA's FBC initiative was launched, then-NASA Administrator Daniel S. Goldin showed an appreciation for the different types of failures when he warned against excessively high success rates. He told the Jet Propulsion Laboratory's staff, "[A] project that's 20 for 20 isn't successful. It's proof that we're playing it too safe." That perspective was possible only because when an FBC project failed, little was lost and much was learned (relatively speaking, of course). So rather than measuring failure on a per-attempt basis, it might make sense to measure FIST failures on a per-dollar basis, with some accounting made for the benefits of learning that optimized failures convey. A dozen failed FIST projects could conceivably cost less (and teach more) than a single failed MDAP. Indeed, NASA's 16 FBC missions, of which 10 were successfully accomplished, cost less than a single traditional planetary mission.

Play Our Failures Right

Let's say it one last time: *Failure is inevitable*. No amount of process, preparation, oversight, or regulation will ensure a 100-percent success rate, even for a large, expensive project that is "too big to fail." Just ask the Comanche team. The best we can do is try to optimize our failures and create situations in which our losses will be low and our opportunity to learn will be high.

Unfortunately, DoD tends to prefer Big Projects, and Big Projects only fail one way—negatively. A framework that relies heavily on MDAPs (and MDAP wannabes) will therefore result in a certain number of painful negative failures. Losses will be high, and opportunities to learn will be few and far between. That's a bummer. It hurts our credibility, wastes resources, diminishes the acquisition community's capacity to accomplish the mission, and ultimately impedes the warfighter's effectiveness.

It doesn't have to be that way. Yes, we're going to fail sometimes, but if we play it right, our failures don't have to hurt quite so much. We might even be able to learn something in the process.

The authors welcome comments and questions and can be contacted at the.dan.ward@gmail.com, chris.quaid@gmail.com, and gabemounce@earthlink.net.