SYSTEMS OF DENIAL

Strategic Resistance to Military Innovation

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Successful organizations can be extraordinarily persistent and creative in denying the obvious, ignoring signals that suggest a need to challenge key strategic assumptions. The U.S. military has been the world's unrivaled force for twenty-five years, even lacking a peer competitor in some domains—naval operations, for example—since 1943. A danger of such sustained success is that the military might come to view these strategic assumptions not as ideas requiring continual reassessment but as enduring laws. The current and future strategic environments demand that the military innovate and question its strategic assumptions, not because we know that they are wrong, but because every theory of competition eventually succumbs to new facts.[1] The military should be extremely sensitive to the risks of believing things that are no longer (or may never have been) true; yet it is particularly vulnerable to persistent denial, and the wartime consequences of such errors are dire.

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These assertions are not ours. The 2014 Quadrennial Defense Review (QDR) mandates that innovation within the Department of Defense (DoD) be a central line of effort. In his assessment of the QDR, Chairman of the Joint Chiefs of Staff General Martin Dempsey states, "With our 'ends' fixed and our 'means' declining, it is therefore imperative that we innovate within the 'ways' we defend the Nation. Successful innovation, particularly for an organization as large and complex as

the U.S. military, is difficult."[2] DoD leaders are also concerned about the alignment of current military concepts and capabilities with a dynamic environment.

This article explores how successful organizations focus organizational energy and attention on refining their dominant theories of competition, often resulting in dysfunctional organizational responses, or *systems of denial,* to strategic anomalies—inconvenient information—that contradict assumptions. Our goal is twofold. First, we apply a novel theoretical approach in seeking to make leaders more aware of a persistent strategic vulnerability—that is, how organizations ignore or dismiss strategic anomalies (events, ideas, or technologies that contradict core strategic assumptions) through three systems of denial: "killing the messenger" by questioning the source of the anomaly; questioning the validity or relevance of the anomaly; and revising the competitive theory to make it more vague and less testable. Second, we describe six ways leaders can create conditions that increase recognition of anomalies, by creating opportunities to see things that are contrary to strategic expectations, as well as to protect anomalies from the organization's systems of denial. Organizations have a mixed record on both tasks, and dominant organizations, such as the U.S. military, are almost universally bad at the second. Developing appropriate and effective responses is a strategic leader responsibility and a fundamental requirement for leading change and innovation.

INNOVATION VERSUS SYSTEMS OF DENIAL

The U.S. military seeks a sustainable competitive advantage—it wants to win now and in the future. An organization sustains success when its strategy and resources align with the opportunities of the competitive environment. In a stable environment, dominant organizations (e.g., the U.S. military) succeed by becoming better at executing their existing strategies, focusing on increasing efficiencies and improving core capabilities. When the environment changes, however, organizations succeed through innovation—developing and experimenting with novel strategies, and shifting resources to new approaches. These two organizational competencies have been called *exploitation* and *exploration*.[3] Yet this presents a paradox: organizations that are good at one tend not to be good at the other.

Dominant organizations have systems that focus organizational energy and attention on exploitation—that is, sustaining the status quo and continuing to improve what they already do. This is a reasonable result of success. However, efficient exploitation often comes at the expense of continued learning and innovation. The ironic effect of systems of exploitation is that they generate dysfunctional organizational responses to inconvenient information, what we call systems of denial.

Unlike a business, most of the time military organizations are not actually engaged in competition; they must manufacture competitive conditions. The wars that the U.S. military imagines and the competition that it manufactures through its scenarios, war games, and simulations reflect its prevalent theories of the security environment and the uses of military force. These theories tend to reflect the military's successes in conflicts that may be of dubious relevance to the current and future security environments. For example, we believe the experiences of Iraq and Afghanistan have been insufficient (perhaps because they were painful) to challenge the military's core assumptions about its required capabilities. After these distractions, the U.S. military has reengaged the exploitation behaviors. It knows what it does well and is determined to continue to do those things. This is good, as long as the things it does well now correspond to the things it actually needs to do. But competitive systems change. The danger for a successful organization is that, absent a significant competitive threat, it tends to fall back on comfortable assumptions and ignore, misrepresent, or manipulate information and innovations that contradict its most-cherished strategic beliefs.

For the U.S. military, sustaining dominance will require significant exploration of the emerging competitive environment. The crucial point is not whether the military has the correct competitive theories right now but what it does when confronted with innovations that suggest its theories may be incomplete or wrong. In peace, the military has the luxury to be wrong for a limited but unknown time, and it is difficult to conclude whether any theory is right or wrong. What does the military do with that time? How does it seek new information? How does its strategy evolve in response to change and innovation? Continued dominance depends on the ability to subject theories of competition to continued, relentless scrutiny. Unfortunately, competitive dominance tends to frustrate this honest exploration.

When something arises that is contrary to our theoretical expectations, it is an anomaly. Though anomalies appear to be failures, they are the essence of progress, because they provoke further inquiry, lead to new discoveries, and may yield new and better theories. Yet organizations often respond to anomalies not by subjecting them to honest examination but by ignoring them, suppressing them, or manipulating the learning process to protect familiar assumptions.

CHALLENGING THEORIES AND CHANGING MINDS

There is no more venerable military tradition than a healthy skepticism regarding new ideas. But such skepticism must be accompanied by openness to new information that would lead us to change our minds and, in the case of strategic leaders, to create conditions in which those discoveries can happen. Furthermore, when information suggests that assumptions supporting the status quo should

be examined, we must be equal opportunity skeptics—we cannot spare the old simply because it is comforting.

The assumptions that guide strategies have achieved that status because they have worked in the past, doing an excellent job of producing expected results, connecting hypotheses to corroborating findings. Theories are discarded when two conditions are met: first, the community of inquiry accumulates a sufficient number of observations contrary to core theoretical expectations (or anomalies); and second, an alternative theory replaces the old system of explanation.[4] Anomalies arise at various points in the process popularly known as the scientific method: observing; theorizing; deducing testable hypotheses from theory; creating conditions to test those statements (experimentation); observing the results; and refining, revising, or rejecting the theory depending on the outcome.[5] Once the facts no longer accord with expectations, a theory is discredited, and a theory that better accords with observation replaces it.

What does theory mean in the military context? Theories of military competition are reflected most clearly in operational concepts and doctrine, usually built on what has worked in the past. These theories give us the means to organize and filter the limitless data of the environment. Without them, the world is just disconnected facts. Theories also provide a logical structure from which we can derive predictions, and those predictions guide strategic choice. The early twentieth-century theory of strategic bombing and the Mahanian concept of naval warfare are examples of powerful theories of military competition that allowed military leaders to organize a myriad of information (about technology, force structure, and so on) and to derive predictions that drove strategic and operational decisions.

Yet theories are not simply organizing frameworks. The predictions of a good theory should be testable or verifiable. Finally and crucially, all theories are provisional; a theory is never proven true, but it can at any time be refuted, and just one anomalous result may be sufficient to do it (though more anomalies are usually required). Progress in any field depends on the acceptance of the idea that theories may be wrong—that the conditions that gave rise to a theory can and will change.

Therefore, theories should be expected to die and be replaced on a regular basis, and (in the long term) this appears to be the case. Knowledge increases over time. But the short term (which in scientific history can mean decades or even centuries) seldom conforms to the rational ideal of the scientific method.[6]

Although theories should be subject to the facts, they can survive long after the evidence has contradicted their fundamental assumptions. Why? Theories shape our understanding of the world. The evolutionary biologist Stephen Jay Gould wrote, "Facts do not 'speak for themselves'; they are read in the light of

theory."[7] In other words, "facts" only become facts when we decide, and the timing and content of our decision depend on our theories. We want to learn the right lessons from the past and present, and we want to be prepared for future competition and conflict. Yet "right" is not a fixed concept. The process of learning and anticipation is highly subjective. We seek and interpret facts about the past and the present on the basis of theories of the way the world works. The past and present are not preselected sets of unambiguous facts with transparent causal connections to the present and future. They are more like a gigantic buffet of information, and the gathered facts on our little plates depend very much on tastes and appetites.

For a scientist who reaches the pinnacle of the geology community by describing a world in which the continents do not move, the notion that they do move (plate tectonics) is not simply a different theory; it is a repudiation of his life's work and the work of his closest colleagues. Impassive acceptance of this kind of change is contrary to human nature. We are social and emotional creatures, preferring to make inconvenient facts (anomalies) subject to our theories. We do this as individuals, and we do this even more in organizations.

These tendencies are particularly important when the theories of a professional community involve competitive systems in which human beings make strategic choices. An organization is a social system with purpose, culture, structure, and resources built around a theory (or theories) of competition. In such complex and adaptive systems, there is no equivalent to the natural laws of the sciences, and therefore theories are much more likely to be wrong. Actors in a competitive system adapt according to the actions of others, changing the behavior of the system as a whole.[8] Because of these adaptations, all theories of competition must be provisional.

Successful organizations are marked by an almost single-minded adherence to a few core assumptions and are built on an organizational structure and culture that both shape and are shaped by a powerful theory of competition. In the context of sustained success, organizations' theories of competition can be extremely resistant to new and inconvenient facts. This is even more pronounced for a dominant military, which must imagine or synthetically produce competitive forces in peacetime.

Theories of competition are filters that exclude facts that do not fit the competitive model. Such filtration of relevant and irrelevant phenomena is a core (and necessary) function of theory—no one can pay attention to everything at once. Biologists are unconcerned with gravitational singularities, and economists do not worry about the breeding habits of the mayfly. Problems arise when the theories exclude phenomena that should be core to strategic choice. Military and

business history demonstrates this troublesome legacy of success—that is, when theories that were once the basis for dominance become barriers to innovation and frustrate adaptation, decision makers become structurally blind to significant changes in the environment. The fall of the Polaroid Corporation is a stark example of this.

Through sixty years, Polaroid's business was built on two powerful assumptions: consumers wanted a physical photographic print to preserve a moment, and they valued instant gratification.[9] Polaroid was served well by these theories. In the world of instant photography, there was Polaroid and nothing else. And then Polaroid was gone, destroyed by the advent of digital photography. The digital photography revolution corroborated one of Polaroid's strategic assumptions —the value of instant gratification—but it completely refuted the notion that consumers needed a physical print. Curiously, Polaroid pioneered some key digital imaging technology and was an early developer of a highly capable digital camera. Yet these innovations were ignored and misunderstood, as they did not conform to Polaroid's theory of competition. Polaroid's final investment in digital photography was a compact ink-jet printer to produce the physical print from the digital camera, demonstrating the firm's continued adherence to the dubious assumption that consumers needed a physical print.

Similarly questionable assumptions have provoked crises for militaries. Consider the following, apparently unobjectionable statements: victory at sea depends on the destruction of an enemy's fleet; freed from a tyrant, a liberated people will welcome and cooperate with its liberators.

The first concept was the guiding assumption for Royal Navy strategy at the start of both world wars. In both conflicts, the German navy circumvented the Royal Navy, avoiding direct engagement and focusing its efforts on building U-boat force structure and antimerchant operations.[10] The Royal Navy's slowness in recognizing the German submarine threat to merchant shipping, and its delay in adopting convoy tactics, nearly brought the British war effort to ruin twice within a twenty-five-year span.[11] These crises were rooted in the Royal Navy's view of the protection of commerce as a mission unworthy of its attention and resources.

The second statement describes the core assumptions of American military operations during preparation for the Iraq war and throughout the first three years of the conflict. During the crescendo of violence against coalition forces in Iraq from 2003 to 2006, U.S. military and civilian leaders insisted that attacks were carried out by foreign fighters or that there was no Iraqi insurgency.[12] American policy makers and military leaders could not effectively respond to rising violence in Iraq until they recognized its sources. Why did they take so long to do so? In the absence of discovering weapons of mass destruction, the most important justification for the war was the liberation narrative, which included the

assumption that a liberated people does not kill the liberators. Acknowledgment of the insurgency contradicted the dominant theory of the political situation in the country and, as a result, delayed changes in American strategy for three years.

In both military cases, the organizations persisted in their erroneous assumptions as they followed a path of systematic denial, despite evidence that their assumptions were wrong. This persistence is destructive, yet avoidable. It is one thing to suffer catastrophe because of the unforeseeable.[13] It is another to suffer it because we—repeatedly, over an extended period—refuse to see what is right in front of us.

In highlighting how an excessive commitment to the dominant current theory perverts organizational learning, we do not argue for reflexive abandonment of key strategic assumptions. Chaos results from the instantaneous abandonment of core assumptions in response to contradictory information. The key is to do it right. We now turn to what "wrong" looks like.

THE SYSTEMS OF DENIAL AT WORK

The U.S. military does not have a monopoly on the stubborn adherence to an erroneous theory. The history of science is rich with examples, and the methods by which scientific communities resist theoretical innovation are instructive. Philosophers of science, such as Karl Popper, Thomas Kuhn, and Imre Lakatos, have examined the social dynamics of scientific communities and theoretical progress.[14] Kuhn's work on scientific revolutions and paradigm shifts has been imported to the social sciences. However, Popper's and Lakatos's influential frameworks for examining how theories are refuted have been largely ignored.[15] Adapting to the military context what Popper called *conventionalist strategies* for defending existing theories against contrary evidence helps us identify three dysfunctional responses, or systems of denial, that occur when organizations are confronted with information that challenges their core competitive assumptions.[16] That is, organizations tend to do the following:

- Question the intentions, authority, or legitimacy of the source (colloquially, "killing the messenger").
- Question the validity, generalizability, or applicability of the information ("that doesn't apply to us").
- Revise the theory to make it less testable (and more resistant to refutation).

These responses are not in themselves bad. Indeed, each is a fundamental part of legitimate inquiry. But they can be misapplied, as often happens in the defense of established theories and concepts against an onslaught of information that undermines them. Such dysfunctional responses to anomalies are predictable, destructive, and preventable.

Killing the Messenger: Questioning the Source of the Anomaly

All inconvenient facts come from some source. If the source of an anomaly is not believable, then we are freed from the burden of dealing with the anomaly itself. Successfully undermining the source of an anomaly requires that we not only convince ourselves of the unreliability of the source but also persuade other key stakeholders of that fact.

Lieutenant Colonel Douglas Macgregor's 1997 book, *Breaking the Phalanx,* criticized U.S. Army structure, culture, and strategy, and recommended significant change.[17] Macgregor was subsequently dismissed as a complainer with a myopic worldview (at best), or treated as a heretic (at worst).[18] "Killing the messenger" can be accomplished in three ways: attack his *legitimacy* by questioning his credentials; attack his *credibility* by asserting that the source has bad intentions; or make *ad hominem attacks*.

When attacking a source's legitimacy, we question his qualifications in making statements or presenting data, based on professional credentials, experience, seniority, etc. This approach is preferable to assaults on credibility or ad hominem attacks, because it seeks to discredit someone on the basis of impersonal considerations. Convincing people that the messenger does not have the required expertise or knowledge fully to understand the phenomenon ends the conversation. Failing that, we can usually argue that the messenger is not privy to the critical information that enables us to understand the situation more fully. In the military, a hallmark of this tactic is invoking classification restrictions—for example, "If he knew what we knew, he would agree with us."

A second approach to undermining a source is to question his credibility, casting doubt on trustworthiness or intentions. We may assert malice of intent, self-interest, mixed loyalties, or conflicts of interest. Contending that a source wants to undermine the organization or that a source benefits from actions taken as a result of a change makes it easier to dismiss the source's criticisms. Questioning credibility suggests corruption in the source's specific motivations—for example, "John Doe is criticizing the Army because John Doe hates the Army."

A third means of discounting a source is a simple ad hominem attack. As defined here, ad hominem attacks suggest corruption in the source as a whole. Whereas in a credibility attack, we question motive, in an ad hominem attack, we seek to undermine the source completely—such as "John Doe is criticizing the Army because John Doe is an idiot." Ad hominem attacks generally take one of two forms: abusive or circumstantial.[19] Abusive attacks, such as "John Doe is an idiot," involve unambiguous statements that question a source's mental competence, character, honesty, etc.; in circumstantial attacks we indirectly suggest corruption in a source. Because preserving and propagating ambiguity are one of the core principles of resisting anomalies (clear statements can be refuted),

circumstantial attacks are more devious (and effective) than directly abusive attacks. Guilt by association, in which we note that a source is part of a community or organization widely regarded in a negative light, is a classic circumstantial attack. For example, saying, "He gets most of his ideas on the role of the military from his State Department friends," may convince many in the military that the messenger is not to be trusted.

While questioning the source is the easiest way to dismiss an anomaly, it also incurs risks. When a large, powerful organization is perceived to attack a less powerful outsider or a maverick within the organization, the sympathies of key stakeholders may be swayed by the natural tendency to root for the underdog. For this reason, when effective proxies are available to question a source's legitimacy or credibility, or to attack the source's character, they tend to be used. Those who allow others to do their dirty work for them are more likely to appear to be impartial observers.

"But They're Not Us": Questioning the Data

Having failed to reject the anomaly on the basis of the messenger, we may question the validity of the data. Three classic strategies for challenging data, in order of difficulty, are, first, question the results on procedural grounds ("they did it wrong"); second, question the generalizability of the data ("it doesn't apply to us"); and third, dismiss the results on the basis of contradictory replication studies ("we tried it ourselves, and it didn't work"). All three of these arguments may be legitimate challenges to inconvenient information, but they may also be abused.

Procedural challenges to data involve questioning the way in which the data were gathered—focusing on the conduct of the experiment, simulation, war game, etc. They free organizations from the necessity of analyzing the results of an experiment or the character of the phenomenon. If the outcome is the result of nonstandard inputs, then we have an easy escape: "They did it wrong, so the results don't matter." The complexity of the inputs in competitive environments provides myriad opportunities to dismiss data on procedural grounds. Look hard enough, and a leader can always find something objectionable in the way that a result was produced. The list of excuses is long.

Even in wartime, when current operations should provide unambiguous evidence of failure, militaries have a great capacity for denial. In the first years of World War II, the U.S. Navy Bureau of Ordnance refused to acknowledge that its Mark 14 torpedo was fundamentally flawed, despite submarine crews' repeated reports of failures. The torpedoes ran too deep and were equipped with a flawed magnetic exploder, and "the contact exploder was so fragile that a direct hit was sufficient to destroy it without causing the torpedo to explode."[20]

In the meantime, the bureau blamed the crews, concluding they must be using it improperly—a procedural objection to the data. When the bureau finally sent an officer to observe whether the error indeed lay with the crews, he was unable to find a single fault with the crews' actions; yet his report still placed all the blame for the torpedo problems on the personnel.[21] Not until the summer of 1943 would a new design resolve the torpedo problem. If this is what military organizations can do in a time of war, when lives are being lost, what are organizations capable of explaining away in less dire competitive contexts?

Failing to dismiss the data on procedural grounds, we can question the generalizability of the data themselves; that is, we must demonstrate that the data do not apply to us, usually by arguing against an analogy. The potential arguments against an analogy are too numerous to catalog here; we highlight two broad objections.

First, we may argue that the competitive context—the time, the place, the product market, the economic conditions, etc.—is too different for a valid comparison. For example, American car manufacturers could have glimpsed their own future in the near bankruptcy of Harley-Davidson motorcycles in the early 1980s. From 1972 to 1982, Harley-Davidson's U.S. market share in motorcycles with an engine displacement exceeding a thousand cubic centimeters dropped from 100 percent to 14 percent, as Japanese imports offered better performance, fuel efficiency, and reliability—all at lower cost. But motorcycles are not cars, and Harley-Davidson's brush with corporate death was little noted by auto manufacturers, except in its demonstration of the appeal of protective tariffs.[22]

Second, we may argue that a difference in one of the key actors makes a comparison invalid. In this case, we acknowledge that the competitive context is comparable, but one of the competitors is not. For example, when a Southeast Asian guerrilla force defeats a European ally armed with American weapons and fielding an army with veterans of the 1944–45 European campaign in key positions, we dismiss it because the French are a colonial power in decline, lacking the righteous purpose and military strength of the United States.[23] The French experience in Vietnam is therefore judged as irrelevant to American strategy. Arguing against the generalizability of an anomaly is easy; the complexity of competitive environments provides numerous candidates.

The final means of questioning data is through a replication study that is engineered to fail—that is, to try to reproduce an anomaly in an environment that we control, to demonstrate that the anomaly is not real. Although the spirit of a replication study is (not surprisingly) to seek to replicate the result of someone else's study or experiment to corroborate a finding, an organization often uses one to get the result that it wants. In the early twentieth century, a young American naval officer named William Sims sent the Bureau of Ordnance reports documenting

astonishing feats of naval gunnery that were based on modifications to equipment and training that he adopted from the Royal Navy. The U.S. Navy initially ignored his reports, but Sims persisted. Finally, he was told that his reported results were simply impossible. The Navy, it turned out, had conducted a replication study using Sims's suggested modifications and had concluded that Sims's results were impossible, because the gun crew could not elevate and depress the gun fast enough.

The failed replication study reveals the power of organizations to deceive themselves. The Bureau of Ordnance had conducted the trials on land, meaning that gunnery crews were attempting to elevate and depress the gun without the assistance of the compensating roll of the ship; that is, the experiments did not replicate conditions at sea, where the gun is elevated on the down roll and depressed on the up roll. (As one side of a ship moves downward or upward, its motion reduces the force required to elevate or depress the gun at a given rate.) Intentionally or not, the bureau ensured that it would arrive at the answer it wanted.[24]

The Shape-Shifting Theory: Resisting Refutation through Constant Theoretical Change

It is possible that none of the aforementioned systems of denial will work. The source may be reliable (or assaults against him may fail). The data may be impossible to ignore. Does this necessitate abandonment of core assumptions? No. In the event that we start to lose the game, we can always change the rules. The last refuge of a weak theory is the revision of the theory, usually to make it less testable.

Theoretical revision is not necessarily a bad thing—revising a theory in the face of disconfirming information may be exactly the right thing to do. A single meaningful result that is contrary to expectation may be sufficient to refute a theory, but mature theories are seldom abandoned on the basis of just one anomaly. Instead, when an anomaly is accepted as a legitimate challenge to the theory, we prefer to revise the theory first—replacing it with another theory as a last resort. A successful theory makes predictive statements (if *x,* then *y;* or $[x \rightarrow y]$). Theory revisions modify these statements (while preserving the core theory) in one of three ways: redefine the outcome, redefine the phenomenon, or add auxiliary hypotheses.

The distinction between the right and wrong ways to revise a theory is simple: proper revisions to theory yield hypotheses that are more testable than they were prior to the revision; improper revisions yield hypotheses that are less testable.[25] Indeed, "last refuge" revisions often result in changes that make testing the theory extremely difficult, if not impossible.

Redefining outcomes and redefining the phenomenon are ways to reframe the success or failure of a theory and produce moderate reductions in testability. These tactics capitalize on the complexity and ambiguity of the strategic environment—the more complex a phenomenon, the greater the potential for disagreement regarding basic concepts such as success and failure, or the essential characteristics of the phenomenon itself.

When we redefine the outcome, we change our definition or interpretation of success and failure (changing our definition of y in $[x \rightarrow y]$). If a theory does not produce its predicted result, or if some other theory challenges it, we simply change the desired outcome to make the dominant theory appear once again successful, or to make the challenging theory appear unsuccessful. For example, during World War I, the British defeated the German submarine threat by adopting convoy tactics. After the war, the Royal Navy was eager to forget this experience and focus on the kind of naval warfare it liked, epitomized by the battle of Jutland.[26] When naval planners reflected on the effectiveness of convoy operations, they emphasized that the convoy escorts had sunk just twenty out of 178 total German submarines destroyed during the war. This amounts to changing the rules of the game. As Holger Herwig observes, "They refused to recognize that what counted was not the number of submarine 'kills,' but rather the number of merchantmen safely in port."[27] Under the new performance measure, convoys appeared ineffective and were therefore easier to forget. Redefinition of outcomes may delay refutation by forcing a theory's opponents to justify prior definitions of success or failure, or to seek to refute the theory under the new definitions (and therefore execute another round of observation and experimentation).

The testability of a theory is also reduced if the new outcomes resist clear measurement or observation. Sometimes an outcome is only partially observable or cannot be observed at all, or it cannot be observed under the desired conditions. This is almost always true of strategic outcomes. Even with a consensus regarding the definition of success or failure, there may be disagreement about what actually happened. What was the outcome of the Iraq war? Who lost? Who won? These are simple questions without simple answers. Many theories of that conflict can find some justification in historical evidence. The potential for redefinition of what "winning" means and for reinterpretation of the actual outcomes is almost limitless.

Redefining the phenomenon (changing our definition of x in $[x \rightarrow y]$) offers another potent means of resistance to refutation. We do not just change the definitions or interpretations of success and failure; we change the entire framework for measurement. If a review of the utility of the aircraft carrier reveals that its core role as the instrument of attack for the Navy has been rendered obsolete by the combination of low-end adversaries that offer few worthwhile targets for

aerial strikes and high-end adversaries who make carriers too difficult to protect within their effective strike range, we may defend the carrier by broadening our definition of what it *is:* not only an attack platform but also a symbol of American power. Thus, the carrier's symbolic value becomes a key measure of performance. Testing this proposition requires that we measure how effective the carrier is as a symbol, but the concept is so fuzzy that it defies measurement. This makes it particularly appealing as a theoretical defense for the carrier concept.

A third approach to theory revision is the modification of the theory itself. We say that *x* is going to result in *y,* but when we do *x,* something unexpected (different from *y*) happens. In such circumstances, the concept may yet be preserved by adding auxiliary—literally, "helping"—hypotheses to explain *y*. An auxiliary hypothesis adds conditions to our predictions when our predictions turn out to be incorrect—that is, changing the prediction from $(x \rightarrow y)$ to $(w \,\&\, x \rightarrow y)$. It is not inherently wrong to do this. Indeed, auxiliary hypotheses are a central aspect of the refinement of existing theories.

One of the best examples of proper theory revision comes from nineteenth-century astronomy, when scientists noticed that the planet Uranus's orbit failed to follow the path predicted by Newtonian physics. Instead of abandoning Newton's laws and searching for a better alternative, astronomers postulated that an undiscovered, more distant planet was affecting the orbit of Uranus. Using the actual path of Uranus's orbit, two mathematicians told astronomers where to look, calculating both the probable mass and position of the unknown planet. Neptune was soon discovered very close to the predicted position. Newton's theory was corroborated, but our model for the structure of the solar system had to be revised.

The story of Neptune's discovery illustrates the key characteristic of a good auxiliary hypothesis: it imposes a higher experimental or observational test. Postulating not only the existence of an undiscovered planet but also its mass and position was a precise and highly improbable prediction (i.e., the random chance of finding a planet in that part of space was extremely—almost infinitesimally—low). That the planet was found was a great achievement of the theory of Newtonian physics. Note that we may lack the technical capability to perform the test; for example, it took centuries for astronomers to verify the distances of the stars that were postulated by the adoption of the Copernican system. The key is that an effective auxiliary hypothesis postulates something that drives further inquiry. In resisting innovation, however, organizations often employ auxiliary hypotheses in a way that hinders learning.

The main indicator of a bad auxiliary hypothesis is that it cannot be unambiguously measured or verified, and therefore cannot be refuted. In the years leading up to World War I, the theory of warfare favored by the British and the

French was what historian Tim Travers calls "the cult of the offensive."[28] However, when the British army suffered terrible losses in assaults in the early battles of the Boer War, European observers were disturbed. Why would a continental army trained and hardened by years of colonial warfare and equipped with the latest weaponry struggle so much against an irregular force? The advent of modern firepower (mobile, rapid-firing artillery; accurate, rapid-loading rifles; and machine guns) meant that an advancing force could no longer protect itself with its own fires. At least it appeared that way. To preserve the theory of the offensive, strategists needed to come up with an alternative explanation for why the British failed. A widely articulated auxiliary hypothesis was that the British army lacked the proper spirit to carry out effective offensive operations on the modern battlefield. This explanation arises from procedural objections to the anomaly; in essence, the British did not do it right ("it" being the offensive).[29] The beauty of this auxiliary hypothesis is that it has the quality most essential to defensive revisions of a weak theory: it cannot be refuted.

Auxiliary hypotheses can be very effective in providing a theory with an impenetrable barrier to refutation. If we propose, "An attacking force with a ratio of at least five to one within the area of assault will be successful in the assault, regardless of the defensive fortifications and firepower," we have the potential to be refuted. The ratio is verifiable. A single example of a sufficiently superior force failing in the assault will contradict the hypothesis. However, if we revise the prediction as follows, "An attacking force with *high morale* and a ratio of at least five to one within the area of assault will be successful in the assault, regardless of the defensive fortifications and firepower," we have compromised the theory. The addition of three small words has made our statement impervious to refutation. Any time an assault fails, we can dismiss it as a failure not of the concept (or training, equipment, etc.), but of morale, a conveniently unmeasurable quality. Irrefutable statements are the hallmark of a bad theory. As the scientist Richard Fortey observes, "The theories that can cause much more trouble are those that can twist and turn in a breeze of new facts without ever fracturing completely."[30]

We have described three organizational tendencies that form a powerful system of denial in response to inconvenient information. How can the military avoid making common mistakes in responding to data that contradict its theories of what does and does not work? What can a leader do to discourage these behaviors and encourage the right responses? While there is no simple solution, in the following section we describe six practices that will improve a leader's chances to "beat the system" of denial, to identify anomalies and follow up with a balanced and thoughtful exploration of what they mean.

SIX WAYS TO BEAT THE SYSTEM (OF DENIAL)

Building organizational skill in recognizing and analyzing novel phenomena is the means by which we seize control of strategic innovation. If we fail to alter our theories in the absence of dangerous competition—when we have the time and luxury to do so—we will be compelled to do it through painful experience, under less appealing conditions. Effective military leaders must create an environment in which organizations identify anomalies in the external environment; seek anomalies through their own exploratory activities; and revise their strategic assumptions (theories of competition) to make them more testable or replace them with alternatives. For these three tasks we recommend six practices and explain them in approximate order of the required commitment in time and resources (low at the beginning, high at the end).

Identify Anomalies in the External Environment

Military organizations must develop sensitivity to anomalies as they arise in the external environment. Yet organizations function by channeling and filtering information, and by directing leaders' attention. These processes are necessary to the normal functioning of organizations; without channels, filters, and direction, we would be overwhelmed by unstructured data. But leaders must develop mechanisms for recognizing important changes that arise outside these channels.

Conduct Formal Thought Experiments with the Help of a Team. On a regular basis, leaders must specify what new information or changes in environmental conditions would cause them to question or abandon their core assumptions. In essence, leaders need to imagine when they would change their minds. This is perhaps the most fundamental of all skills required in leading innovation.

We like to believe that when the facts change, we change our minds. But the recognition of such changes depends on whether we are looking for them, and we find them more readily when we seek them in response to a question that we have already asked. We recommend that at regular intervals, leaders work with a team built specifically for this purpose.[31] The product of the team's work is a formal description of core strategic assumptions (at least three, no more than five) and events or facts that would invalidate those assumptions. What signals would suggest that assumptions are at risk? At these meetings, leaders and other team members make "reputational bets" about the environment and their strategic assumptions.[32] For example, the force structure of the Air Force reflects the technological limitations of munitions. Fighter jets exist not for their own sake but because small, numerous platforms are required to get close enough to strike enemy air and ground forces. Efficient directed-energy weapons would potentially upend this framework, favoring the development of large, "mother ship" platforms that

would destroy targets from great distances using energy traveling at the speed of light. At some point, Air Force modernization and force structure would need to change radically to realize the potential of these weapons. What events in the environment would signal a need to begin such change? A thought experiment is a commitment to reexamine core assumptions when the facts change.

A structured thought experiment serves four important purposes. First, it helps us to identify core assumptions; the principles that lie at the heart of our theories are often so familiar to us that they are invisible. We treat them as laws of nature, never to be questioned, not as assumptions that are inherently provisional. Second, it forces us to make formal commitments to reviewing our theories. Refutation is a matter of degree, not absolutes, and we can always reconcile facts to our chosen theories. Nevertheless, if we say that we shall change our minds if *x* happens, and *x* actually happens, then we must either change our minds, or explain how we have adjusted our assumptions to accommodate *x*. Third, in identifying conditions that would prompt us to change our minds, we become more likely to recognize those conditions if they arise. This is a crucial point: the simple act of naming something makes us more likely to see it. Finally, the thought experiment highlights the parts of our system of assumptions that are *not* subject to facts. We do not wish to denigrate the role of faith (or its close cousin, intuition) in strategy development. However, wherever faith becomes the main support of an assumption, a leader should create mechanisms in the organization through which those who do not share that faith have the freedom, the power, and the resources to challenge the status quo.

Don't Succumb to the Tyranny of Expertise; Institutionalize Brokerage. Leaders must have the wisdom to separate the messenger, however abrasive, from the message. Challenges to orthodoxy are more likely to come from sources that stand outside the dominant strategic system for the very reason that they are outside the system. They are not captured by our theories and are more likely to see emergent inconsistencies between our core assumptions and strategic reality.

First, we must overcome the tyranny of expertise. We may believe that only the experts are qualified to comment on a theory's soundness. However, those who have the strongest credentials under an existing theory also tend to be credentialed by a system that has arisen around that theory. That is, their status as experts is not independent of the theory; it is a product of the theory's success to date. Those we trust the most as experts are often the least likely to recognize and identify anomalies. For instance, Air Force pilots were not the early proponents of unmanned aerial systems.[33]

Second, the perspective and openness required to identify potential anomalies, the willpower to create a context for revealing them, and the moral courage

to point them out may be correlated with personality characteristics that make such sources more susceptible to messenger-killing tactics. Those with the confidence and imagination to propose anomalies frequently lack the social intelligence and savoir faire required to persuade audiences of the importance of their insights. Iconoclasts like American military-aviation pioneer Billy Mitchell and British strategist Basil Liddell Hart often fail to differentiate between potential allies and enemies within the organization, alienating those who would support their positions. Anomalies are likely to come from sources that are vulnerable to questions of credibility and legitimacy. Leaders must serve as advocates for such challenging perspectives.

Leaders can foster the development of officers who are sensitive to external viewpoints by institutionalizing brokerage. In social networks, a broker is a node in the network that connects groups that are not otherwise connected. The social theorist Ronald Burt has argued that brokers are more likely to have good ideas because they see actual and potential combinations that others cannot.[34] The highly cohesive and almost cloistered networks of military personnel do not lend themselves to creating mental and organizational environments that challenge conventional wisdom.

Institutionalizing brokerage means creating opportunities for members of the organization to work with other entities, particularly those with different theories of competition. This requires that military organizations reward officers with high-quality "broadening" experiences, including not only joint and interagency work but also exchanges with foreign militaries and meaningful service in think tanks, universities, laboratories, and even private firms. Brokerage is about building ties to communities that are more likely to see the environment differently. In this way, the organization is more likely to perceive changes that would usually be filtered out or ignored, and to be exposed to different theories of competition.

Deliberately Seek and Explore Anomalies

Leaders should use internal organizational resources to seek out and explore anomalies, to anticipate and drive change. The two recommendations above seek to increase the probability that organizations will recognize anomalies in the external environment. Yet leaders are not only at the mercy of events—they can shape events, as well.

Experiments are one of the most powerful tools available to organizations, yet strategic leaders seldom take advantage of them, because they lack understanding of experimental design and how to operationalize experimental principles in nonscientific contexts.[35] A good experiment must have at least three characteristics: it must vary from normal organizational practices in some fundamental way, and the variance must be isolated to allow for measurement or comparison;

it must have the potential to produce a surprising result; and it must be relevant to the theory—it must have the potential to call into question the existing theory, or to suggest that a new theory offers a better explanation of the phenomenon.[36] We recommend the following two practices to leverage the learning opportunities in experimentation.

Create Space for Planned and Unplanned Variance. The more variance we tolerate, the more likely we are to learn. This fact must be weighed against the risks that accompany experimentation. Where we introduce different treatments and how much variance we tolerate are questions requiring serious consideration. We cannot perform experiments that jeopardize lives or undermine public trust, for example. But without variance, there is no experiment.

Leaders are responsible for creating and fostering both formal (planned) and informal (unplanned) variance. Pilot programs, war games, simulations, and experimental units are examples of mechanisms for planned variance. Strategy development is well suited to simultaneous implementation and evaluation of several different experimental interventions—rapid prototyping. Not everything will be fruitful, but what works and what does not work are both learning opportunities. Persistent and widespread experimentation increases the probability that you will find a subset that improves your understanding. Clearly, the wars in Iraq and Afghanistan created opportunities (and necessities) for constant experimentation. The question left unanswered is, to what extent did the results of these experiments change the deeply held assumptions of any of the military services?

A second means to introduce variance is through informal experimentation. The potential for learning from user innovation is tremendous, though it presents its own set of difficulties. Linus's law of software debugging, as interpreted by Eric Raymond, posits, "Given enough eyeballs, all bugs are shallow." By "shallow," Raymond means that a problem is susceptible to solution. More people dealing creatively with problems of discovery "increases the probability that someone's toolkit will be matched to the problem in such a way that the bug is shallow to that person."[37] User innovation leverages a similar community of exploration and experimentation, and it requires that leaders tolerate unplanned variance.

Create Conditions in Which Surprising Experimental Results Are Not Just Possible—but Desired. When organizations are biased in favor of confirming their strategic assumptions, experimentation is concerned with validating existing concepts, which may be done through highly engineered exercises that only fail to produce the expected result when somebody makes an error. This is a gross misuse of the term *experiment*. For example, the 2002 U.S. military MILLENNIUM CHALLENGE war games and joint exercises illustrated some effective aspects of experimental design and procedure. The opposing force in MILLENNIUM

CHALLENGE introduced battlefield conditions that varied considerably from normal practice, and the results of this variance were surprising. In these two respects, the experiment was well conceived and executed. Instead of interpreting this information as a signal that the organization should scrutinize its conceptual assumptions, it responded to the surprise by resetting the game, eliminating the problematic variance, and producing the validating result that it desired.[38] This was a squandered opportunity. Avoiding this error requires a shift of mind in how we assess success and failure in experimentation. We need to recognize that some failure is noble, and some success is empty.

The challenge for innovative leaders is engineering the competitive context in a way that reveals problems with the organization's dominant theories. In war, the enemy provides that context. In peace, the military must manufacture it artificially through war games, simulations, exercises, etc.[39] Effective experimentation requires reframing "winning" in strategy development.[40] When the goal of exploration and experimentation is innovation, objectives should emphasize outcomes that test established strategic concepts and challenge existing assumptions. In this context, failure in an experiment, whether a war game or a product pilot, should be valued for the opportunity that it provides to revise and refine the concept under examination or to suggest a viable alternative. A leader must persuade the organization that a surprising outcome in a war game or simulation is not just acceptable but desirable. Such surprises are the means by which we identify new problems, create an impetus for change, and develop a structure for innovation. By reframing the objectives of experimentation, organizations will be more likely to discover those new problems.

Experiments should create variance in areas that pertain to meaningful strategic assumptions. In the event that a result contradicts expectations, the experiment should provoke theoretical revision and further experimentation. Leadership owns the strategic assumptions in a military service, and leaders should be intensely interested in challenging those assumptions.

Revise the Theory the Right Way, or Replace It with Something Better

In our first recommendation, we proposed a thought experiment for identifying the conditions that would prompt us to change or modify our assumptions. But what happens when those conditions arise? Our final two recommendations concern how leaders can foster the development of new theories and turn those theories into operational reality.

Make the Old Theory Work for a Living. The way in which we articulate ideas influences our subsequent search for the truth. Recognizing an anomaly and supporting innovation do not require that we abandon established ways of doing things; we always have the option of modifying our current theories with an

appropriate auxiliary hypothesis. The key is to use auxiliary hypotheses not to shore up established theories by making irrefutable statements but to refine theories by increasing their testability, or to develop alternative theories. For example, "All weapons-release decisions require a human input" is a universal statement of a principle governing the use of drones. It expresses a theory about the limits of full autonomy, and it creates a condition in which the theory will be challenged. If we identify a case in which we would accept a fully autonomous kill decision, we have invalidated the principle of universally limited autonomy. At this point, we have two valid options for reformulating our theory.

First, we can add a testable auxiliary hypothesis to the old theory: "All weapons-release decisions require human input when conditions *a, b,* and *c* hold." This creates conditions in which the revised theory is still falsifiable, as will occur when we show that a drone can be fully autonomous under conditions *a, b,* and *c*. This is crucial to learning.

The second option is that we scrap our old theory and begin to develop a new theory to explain full autonomy. Innovation becomes a matter of building a new and vaguely defined theory of automation, not a defensive withdrawal to protect the old theory of human control. The move from a theory of human control of kill decisions that has auxiliary hypotheses to a preliminary theory of full automation may take a long time, or it may be swift. Much depends on whether change begins during peace or war.

If we modify a theory in response to anomalies, the modification should always be in the direction of greater testability—that is, the statements that we derive from our modified theory should be more refutable. As noted earlier, organizations tend to do the opposite, making theories impervious to refutation by layering on untestable statements. Leaders set the tone for organizational inquiry. When it comes to challenging our assumptions, the Fabian approach (a military strategy that avoids decisive conflict) to theory defense should not be tolerated. We must be relentless in subjecting our old assumptions to new tests.

Build Organizational Units That Succeed with a New Theory. At the individual level, it is almost impossible to review evidence without bias; our theories are powerful lenses that distort our reading of the facts. Yet leaders can guide exploration at the organizational level to ensure that someone seeks in good faith to verify, reproduce, and expand on the findings of anomalies. Essentially, the leader needs to engineer competition such that at least one alternative theory competes with the established view, and that the two (or more) theories have a level playing field. Adjudicating a theoretical competition is hard, but not having any competition at all is worse.

The creation of organizations that are free to construct a new culture based on new ideas about what works is a powerful leadership tool. In his theory of disruptive innovation, Clayton Christensen describes how established firms can overcome resistance to innovations by creating semiautonomous units that succeed by finding a viable business for a new product.

Established firms are often driven out of business later by new, smaller firms that develop what appear to be inferior or irrelevant technologies in new markets. The smaller firms move into the dominant firms' core market when the innovation improves enough to meet customer requirements in the core business. To avoid this fate, Christensen recommends that established firms spin off an independent organization that lives or dies by the new (and for the time being, inferior) technology. He writes, "Creating an organizational context in which this effort can prosper will be crucial, because rational resource allocation processes in established companies consistently deny disruptive [innovations] the resources they need to survive, regardless of the commitment senior management may ostensibly have made to the program."[41]

Successful organizations usually excel at innovating in areas that are relevant to core strategic concepts. However, when an innovation does not conform to the organization's dominant concept, leaders can create organizational structures that support the new approach. This involves creating and resourcing organizations that survive by doing things that meet current market needs and, in the process, identify new markets for their products. Such organizations must do more than just produce good ideas. They must find a market (in military terms, a valid/current mission) for the innovation, an application that demonstrates—even on a small scale—the effectiveness of the innovation sufficient to guarantee a flow of financial resources. Absent this demonstration, the innovation will be deprived of resources, resulting in underinvestment (if not abandonment). It is not enough to create new organizational structures around innovative ideas. Markets are what organizations resource, and the leader must create a structural context in which potentially disruptive innovations are linked to current mission requirements.

We need not highlight how difficult it is to execute these ideas. There are no simple answers to the key questions. Who will be assigned to the new organization? What implications will that assignment have for their careers? Who will fight for resources in the budget for the new entity, especially after its creator or advocate retires or is reassigned? These are challenging problems, but a supportive organizational design is a powerful tool for developing strategic alternatives, as Admiral William Moffett demonstrated in setting conditions for the development of the naval aviation community in the 1920s.[42]

None of This Works without Educated Officers

Underlying all of these recommendations is an inescapable reality: we need intelligent, open-minded leaders—men and women who understand the fundamental principles of logic and evidence, are nimble enough to recognize the significance of strategic anomalies, and have the mental tools to think of what to do next.

At each level of officer development, we must demand that officers learn and relearn core principles of epistemology: logic, scientific reasoning, and research methods. Instruction on logic should include deductive (rational) and inductive (empirical) reasoning, as well as inference to the best explanation. Instruction on scientific reasoning should review the basics of the philosophy of science. What is a theory? What are the characteristics of a good theory? How do we choose among competing theories? What is the scientific method and how much does it correspond to science as practiced? What constitutes scientific proof or contradiction? What are the prominent historical models of scientific discovery? Instruction on research methods should include both inferential and Bayesian statistical approaches, probability, measurement, experimental design, and natural experiments. We do not argue that this material must be covered in great depth. But at present the overwhelming majority of senior officers have no background in these concepts.

Sustaining dominance requires significant exploration of the emerging competitive environment. The crucial point is not whether the U.S. military has the correct competitive theories right now but what it does when confronted with innovations that suggest its theories may be incomplete or wrong.

Systems of denial cause organizations to persist in their comfortable assumptions, despite evidence that the world is changing and that these changes may be dangerous. The systems of denial—killing the messenger, questioning the data, and resisting refutation through constant theoretical change—are powerful ways in which organizations ignore inconvenient information. Overcoming them demands that a leader focus on the counterfactual work of organizational change—understanding what is not happening and which possibilities are not being discovered, explored, developed, evaluated, or implemented—and why it is not happening. This requires understanding how inconvenient facts are resisted and developing a strategy for overcoming that resistance. As military leaders pursue initiatives that increase sensitivity to new conditions in the external environment, as they encourage experimentation, and as they invest attention and resources in developing new theories, they will find that the system of denial gives way to a different system—one of learning, of insight and foresight, and of change. This is the way of progress.

NOTES

1. Andrew Hill and Charles Allen, "Military Innovation through 'Brilliant Mistakes,'" *Army Magazine* 64, no. 8 (July 2014), p. 29.
2. U.S. Defense Dept., *2014 Quadrennial Defense Review* (Washington, D.C.: 4 March 2014), p. 59.
3. Sebastian Raisch et al., "Organizational Ambidexterity: Balancing Exploitation and Exploration for Sustained Performance," *Organization Science* 20, no. 4 (July–August 2009), pp. 685–95.
4. According to the structure of formal logic, a "sufficient number" of anomalies should be just one. That is, a theory makes universal statements, and a single contradiction of a universal statement should be enough to discredit the theory. For example, a single red raven would refute the theory that all ravens are black. In practice, refutation is much more complicated and unpredictable.
5. See William Whewell, *Novum Organum Renovatum* (London: J. W. Parker and Son, 1858), and Thomas Kuhn, *Structure of Scientific Revolutions* (Chicago: Univ. of Chicago Press, 1962). The question of how to evaluate competing theories is profound. Kuhn describes five criteria: scope, accuracy, consistency, simplicity, and fruitfulness; see Thomas Kuhn, *Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: Univ. of Chicago Press, 1977), pp. 320–29. The problem is that competing theories will be better in some areas and worse in others. Although we address aspects of the problem of theory selection, a thorough analysis is beyond the scope of this paper.
6. Thomas Kuhn, *Structure of Scientific Revolutions: 50th Anniversary Edition* (Chicago: Univ. of Chicago Press, 2012), pp. 92–110.
7. Stephen Jay Gould, *Ever since Darwin: Reflections in Natural History* (New York: W. W. Norton, 1977), p. 161.
8. Michael T. Hannan and John Freeman, "The Population Ecology of Organizations," *American Journal of Sociology* 82, no. 5 (March 1977), pp. 939–46. See also Elaine Romanelli and Michael L. Tushman, "Organizational Transformation as Punctuated Equilibrium: An Empirical Test," *Academy of Management Journal* 37, no. 5 (October 1994), pp. 1141–66, and Boyan Jovanovic, "Selection and the Evolution of Industry," *Econometrica* 50, no. 3 (May 1982), pp. 649–70.
9. Mary Tripsas and Giovanni Gavetti, "Capabilities, Cognition, and Inertia: Evidence from Digital Imaging," *Strategic Management Journal* 21, nos. 10–11 (October–November 2000), p. 1154. Polaroid described the desire for a physical print as "a basic human need."
10. Gautam Mukunda, "We Cannot Go On: Disruptive Innovation and the First World War Royal Navy," *Security Studies* 19, no. 1 (2010), pp. 124–59. See also Holger H. Herwig, "Innovation Ignored: The Submarine Problem—Germany, Britain, and the United States, 1919–1939," in *Military Innovation in the Interwar Period,* ed. Williamson Murray and Allan R. Millett (Cambridge, U.K.: Cambridge Univ. Press, 1996), pp. 227–64.
11. Herwig, "Innovation Ignored," pp. 244–50.
12. See "U.S. Would Not 'Admit' the Insurgency in Post-war Iraq," *BBC News,* 15 December 2009, news.bbc.co.uk/; Thomas Ricks, *Fiasco: The American Military Adventure in Iraq* (New York: Penguin, 2006), pp. 168–71; and Bradley Graham, "Zarqawi 'Hijacked' Insurgency: U.S. General Says Foreign Fighters Now Seen as Main Threat," *Washington Post,* 28 September 2005, p. A17.
13. See, for instance, Nicholas Nassim Taleb, *Black Swan: The Impact of the Highly Improbable,* 2nd ed. (New York: Random House, 2010).
14. See Kuhn, *Structure of Scientific Revolutions: 50th Anniversary Edition;* Karl Popper, *Logic of Scientific Discovery,* 2nd ed. (London: Routledge, 2002); and Imre Lakatos and Paul Feyerabend, *For and against Method,* ed. Matteo Motterlini (Chicago: Univ. of Chicago Press, 1999).
15. A literature review found no citations of Popper or Lakatos (or, for that matter, the terms *falsification* or *refutation*) in reference to military or business innovation.
16. Popper's description of the conventionalist response to challenges to scientific theory influenced how we constructed our framework. See Popper, *Logic of Scientific Discovery,* pp. 59–61. The systems of denial we outline

are a modification and adaptation of Popper's analysis.

17. Douglas A. Macgregor, *Breaking the Phalanx: A New Design for Landpower in the 21st Century* (Westport, Conn.: Praeger, 1997).
18. Daniel S. Stempniak, *An Agent of Change or a Colonel Who Just Complained: A Case Study of Colonel Douglas A. Macgregor and His Book,* Breaking the Phalanx[:] A New Design for Landpower in the 21st Century (Fort Leavenworth, Kans.: U.S. Army Command and General Staff College, 2003), p. 9.
19. Patrick Hurley, *A Concise Introduction to Logic,* 8th ed. (Belmont, Calif.: Wadsworth, 2003), pp. 118–19.
20. Herwig, "Innovation Ignored," p. 260.
21. Ibid.
22. Daniel Klein, "Taking America for a Ride: The Politics of Motorcycle Tariffs," *Cato Policy Analysis,* no. 32 (January 1984), www.cato.org/.
23. David Halberstam, *Best and the Brightest* (New York: Random House, 1972), p. 145.
24. Indeed, Sims's experience is almost a model case (albeit serendipitous) for Popper's "conventionalist strategies" for defending existing theories in the face of contrary evidence. See Elting Morison, "A Case Study of Innovation," *Engineering and Science* 13, no. 7 (1950), p. 8.
25. Popper, *Logic of Scientific Discovery,* pp. 95–107, 248–63.
26. Herwig, "Innovation Ignored," p. 249.
27. Ibid., p. 244.
28. Tim Travers, *Killing Ground* (London: Allen and Unwin, 1987; repr. Barnsley, U.K.: Pen and Sword, 2009), p. 43. Travers argues against the applicability of the British experience, which seems comforting, except that the French, British, and German armies developed their offensive strategies for war against modern armies. Thus, the British struggles appear more relevant to continental concerns.
29. Ibid., pp. 43–44.
30. Richard Fortey, *Earth: An Intimate History* (New York: Random House, 2009), p. 130.
31. Paul J. H. Schoemaker, *Brilliant Mistakes* (Philadelphia: Wharton Digital, 2011), p. 72.
32. Philip Tetlock, *Expert Political Judgment: How Good Is It? How Can We Know?* (Princeton, N.J.: Princeton Univ. Press, 2005), pp. 125–29.
33. P. W. Singer, *Wired for War: The Robotics Revolution and Conflict in the 21st Century* (New York: Penguin Books, 2009), pp. 253–54, 367–68.
34. Ronald S. Burt, "Structural Holes and Good Ideas," *American Journal of Sociology* 110, no. 2 (September 2004), pp. 349–99.
35. Jim Manzi, *Uncontrolled: The Surprising Payoff of Trial-and-Error for Business, Politics, and Society* (New York: Basic Books, 2012), pp. 164–65.
36. See Popper, *Logic of Scientific Discovery,* pp. 60–67; Kuhn, *Essential Tension,* pp. 320–39; and Steven Gimbel, *Exploring the Scientific Method: Cases and Questions* (Chicago: Univ. of Chicago Press, 2011), pp. 91–93.
37. Eric von Hippel, *Democratizing Innovation* (Cambridge, Mass.: MIT Press, 2005), p. 94.
38. "The Immutable Nature of War," an episode in *Nova,* www.pbs.org/. A notable historical example is the artificial restrictions placed on the use of aircraft carriers during U.S. Navy war games in the 1930s. See Russell Weigley, *American Way of War: A History of United States Military Strategy and Policy* (New York: Macmillan, 1973), p. 253.
39. For a rich discussion of this topic, see Manzi, *Uncontrolled*.
40. Hill and Allen, "Military Innovation."
41. Clayton M. Christensen and Michael E. Raynor, *Innovator's Solution: Creating and Sustaining Successful Growth* (Boston: Harvard Business School, 2003), p. 217.
42. Stephen Peter Rosen, *Winning the Next War: Innovation and the Modern Military* (Ithaca, N.Y.: Cornell Univ. Press, 1991), pp. 76–80.