The method, an inquiry submission form, and scenarios describing open-ended problem situations are available on our website, http://onlineethics.org/helpline. Although many advantages of the method can be realized only by having the discussions within departments, the Online Ethics Center could use the given scenarios, or others that users submit, to discuss a problem-of-the-month in an open, moderated Web forum.

Caroline Whitbeck
(cwrcr@onlineethics.org)
The Online Ethics Center for Engineering and Science
Case Western Reserve University
Cleveland, Ohio

Book Review on Fire

Peter Zimmerman's review of Lynn Eden's book *Whole World on Fire: Organizations, Knowledge, and Nuclear Weapons Devastation* (PHYSICS TODAY, April 2005, page 62) strikes me as throwing the baby out with the bath water. Zimmerman appears to condemn the book's real message about organizational dysfunction because he dislikes "her and [Theodore] Postol's diatribe against the atomic establishment," which he has labeled "her conspiracy theory." My reading says that Eden provided useful and verifiable history about portions of the development of US strategic targeting procedures.

If there had been a conspiracy within the Department of Defense (DOD) to exclude fire damage from the development of US targeting plans and procedures, I would have had a role in it. There was no conspiracy. What are my credentials for making that declaration? My career in nuclear weapons effects research testing and analysis began in 1951, when I was present at Operation Buster/Jangle at the Nevada Test Site, and continues to the present. Until 1974 I was responsible for planning several DOD nuclear tests. I still consult for the Defense Threat Reduction Agency on matters related to the nuclear effects database compiled by that agency during the entire nuclear testing period that ended in 1992.

My recent review of the 1946 reports of the US Strategic Bombing Survey and my personal involvement with nuclear weapons effects are consistent with what Eden has described. In fact, the survey team's extensive documentation in 1946 was soon put on the back burner by DOD scientists and engineers. The researchers at first used analyses of Japanese building responses to infer the weapon yields. Their analyses also suggested a simplified model for calculating a critical building-element response—a model that is still used in target damage assessment. However, when the Atomic Energy Commission (AEC) began atmospheric nuclear testing in 1948, DOD engineers quickly attached themselves to those tests.

Although we had experiments on thermal ignition and so forth, they were separated to avoid unwanted synergy in systems response. In addition, most structure-response test items were theoretical analysis models, not models of Japanese buildings. Consequently, as the Japanese structural-response database was replaced for US test planners by the sterile-structures tests at the Nevada and Pacific Proving Ground test sites, a unique feature of the Japanese data—the fire damage—faded in importance.

In reading Eden's book and recalling my own experience during those years, I can visualize how our nuclear weapons effects community lapsed into a sort of "group-think" programmatic decision-making process. We could not demand that the AEC test at a rate that would satisfy requirements for good scientific method for instrumentation development, for preliminary scale-model tests, and for thorough analysis of test data before conducting the next test. The DOD engineers were not in sufficient control of their test beds and the timing of events, nor did they have enough time between tests to understand the data.

Conspiracy? Absolutely not. A historically relevant story from which to learn and move on? Yes! And, move on we did.

The real value of Eden's book is in her thesis that entrenched organizational thinking can lead to unwanted results or ignore important factors, and in her suggestions about change. Zimmerman seems intent on finding inconsistencies in engineering and scientific details about the fire damage in Japan and in targeting in general. Certainly, fire damage was devastating in Japan. I started reading Eden's book because I wanted to know what she had written about that, since I was actively reviewing the data. I soon found that her real message was more important and wasn't just a criticism of military

planners for not incorporating fires in target damage predictions.

John G. Lewis
(jglewis1@verizon.net)
Marina Del Rey, California

Because Peter Zimmerman has an impressive record of publications and achievements, his words carry considerable weight, and so his characterization of Lynn Eden's work and objectives will not be ignored.

I cannot pose as a disinterested bystander, since Eden makes liberal use of my work, but I do wish to establish my support for what I view as a thoroughly documented and carefully researched book.

Many academics suspect that science done under military sponsorship is less than pure, and I encountered that thinking in my early dealings with Eden. But to her credit, she listened to my explanations, studied my work with care, and went on to research her subject extensively over many years. Her characterization of the government's efforts to deal—or not deal—with fire from nuclear weapons is more complete than any other source I am aware of. For that alone her book deserves careful reading and a place on library shelves.

Zimmerman's description of weapons of different yields as radiation, blast, or thermal weapons is an oversimplification if not misleading. Even subkiloton weapons carry an impressive blast wallop and create an intense but brief thermal pulse. Megaton weapons also generate vast amounts of nuclear radiation and an impressive blast wave as well as a long if less intense thermal pulse.

Eden's example of a burst at 1500 feet over the Pentagon is not so unreasonable as Zimmerman asserts. It would indeed put about 10 atmospheres or 140 psi on the Pentagon, which has multiple levels below ground that house vital functions. That yield and burst height would go a long way toward collapsing and blowing away the entire structure. At the same time, the blast and the thermal radiation across the rest of Washington, DC, would be devastating, even if a higher burst height might cause damage to urban structures and civil facilities over a larger area.

Zimmerman gives the impression that blast does not lead to fires, that only thermal ignitions do. Of course that is not the case. There is ample evidence that disruption fires, which are caused by blast interference with flammable or ignitable systems, are inevitable in any city bombing. The British learned this the hard way in early World War II, when the Germans bombed their cities and the damage was spread by fires; damage from spreading fires could be far greater than the direct damage from high-explosive bombs.

A recent review of the damage done at Hiroshima and Nagasaki confirmed the prevalence of fire damage. In several instances the authors of the US Strategic Bombing Survey reports on the atomic bombings expressed annoyance that fire damage hindered their assessment of blast damage to structures. It is clear that the survey instructions were to correlate and quantify the blast damage from the bombs. The reports, however, did document in detail the damage done by fire, and that damage is in agreement with the modern predictions Eden frequently mentions.

The early analysts who planned the use of atom bombs faced many difficulties and uncertainties. Largely because of the emphasis on physical or blast damage, I spent the first few decades of my professional life defining and refining our understanding of the blast from nuclear explosions.

I believe Eden is correct in pointing out that, had the same effort to understand blast damage been applied earlier to fire damage, the initial attempts to plan targeting for atom bombs might have been different.

In her book, Eden seems to have grasped many of the factors that influenced or guided the evolution of the US planning doctrine and the computational tools that dictated the use of what grew to be a vastly expensive and potentially devastating nuclear force. She alludes to the fact that civil engineers with experience in structural dynamics played important roles in the early development of the targeting methodology, but that no comparable experts in fire damage were included. Intentional? The result of a conspiracy? I doubt it. I would like to think that I was too much a part of the process to have missed a conspiracy.

Eden has acknowledged the progression toward more comprehensive planning for the use of nuclear weapons, and in the process has highlighted several important aspects of the functioning and potentials for failure in organizations, bureaucracies, and large-scale systems. Her account contains many useful lessons, even if one discounts the importance of fire damage in nuclear planning.

I reviewed with care Eden's use of my material, so I feel qualified to assert that she has produced a carefully and fully researched and referenced work whose findings, although arguable, are difficult to refute.

Harold L. Brode
(harold.brode2@verizon.net)
Pacific Palisades, California

In his review of my book, Peter Zimmerman has made some egregious errors. He says that I discuss the detonation of a 100-kiloton bomb at ground level at the Pentagon and criticizes me for choosing a ground burst. In fact I discuss the detonation of a 300-kiloton bomb 1500 feet above the Pentagon. He then says I compare the resulting damage with a 10-kiloton bomb. I do not. Despite Zimmerman's claims to the contrary, I carefully discuss the relative damage done by blast and fire at Hiroshima (15 kilotons) and Nagasaki (21 kilotons), and in chapter four I present a table showing in detail blast damage by distance to various structures for both cities.

I do not argue, as Zimmerman says, that fire damage has not been incorporated into US nuclear targeting calculations because of "some conspiracy to deny the truth." On the contrary, I argue vigorously against a notion that organizational interests—which, full-blown, could be understood as conspiracy—explain why fire damage has been ignored.

Zimmerman compliments me when I depart from what he has divined as my "conspiracy theory" to ask how a new mode of thinking gradually replaced entrenched patterns of thought. He laments that it's too bad I did not expand on that question. But the entire book is about entrenched organizational ways of thinking and doing and the possibilities for organizational change. I wrote a careful scholarly book. He has written a polemic.

Lynn Eden
(lynneden@stanford.edu)
Center for International Security and Cooperation
Stanford University
Stanford, California

Zimmerman replies: Let me first apologize to Lynn Eden for misstating the yields and burst heights of the nuclear weapons she compares in her first chapter. I had computed

several cases, and unwittingly carried the wrong printout from table to computer. However, Eden's figures strengthen my contention that she compared apples with oranges.

According to the standard work on the effects of nuclear weapons,[1] blast effects are comparable for weapons at burst heights h_1 and h_2 such that $(h_1/h_2) = (W_1/W_2)^{1/3}$, where W_i is weapon yield. Thus, an appropriate altitude for the 300-kiloton detonation should have been about 5000 feet for it to be compared with the Hiroshima bomb, not 1500 feet. Scaling for thermal radiation effects depends on the distance between the fireball and the exposed ground, because of both the inverse square law and any atmospheric absorption.

John G. Lewis and I are in near-perfect agreement. I stated that there was no conspiracy to exclude fire damage from targeting calculations; he as an insider confirms that. I take his point that there were too few weapons effects tests. Experiments with multiple synergistic effects might have provided early on the tools to compute the ignition and spread of fires. After the 1963 Limited Test Ban Treaty, atmospheric testing ended, and no further large-scale experiments could be done. Even during the days of atmospheric testing, effects shots always took a back seat to the Atomic Energy Commission's testing to improve the weapons themselves.

Harold Brode probably knows more about nuclear weapons effects than any other person alive. But I challenge his comment that my description of the effects of weapons in very different yield categories was misleading. I clearly indicated that I was referring to simple rules of thumb about nuclear weapon phenomenology as made by the effects community. I am aware that even a very low yield, "enhanced radiation" weapon has significant blast effects.[2] Nevertheless, it is appropriate *to first order* to think of 20-kiloton devices as inflicting damage at greater ranges by blast than by mass fires, and to think of very large weapons as primarily wide-area incendiaries. It is also well known that fires can be started by broken gas mains, downed electrical lines, defective bakery ovens (London, 1666), and cows upending lanterns.

Brode also confirms my belief that there was no government conspiracy to prevent fire damage from being included in damage predictions. In my review, I said that I thought Eden perceived a conspiracy, but that I saw none. I do not understand why Brode and Lewis suggest I endorsed a conspiracy theory.

In retrospect perhaps I should not have called Eden's book a diatribe nor indicated that she saw a conspiracy. But how could a sympathetic and knowledgeable reviewer come away with such an impression? It lies in the text of chapters 8, 9, and 10. There, Eden shows the interplay of Brode's system for predicting fire vulnerabilities and damage with the work of the fire research community. On page 236, she quotes Brode as saying that fire predictions could have been incorporated into targeting calculations in 1948, 1954, or 1958. But they were not. Why not?

Eden gives a partial answer, unflattering to the targeting community, which is said to have asked, "Were the differences in resulting damage sufficient to warrant the time and cost?" (p. 248). Always, Eden indicates, decisions on incorporating fire damage were put off "for probably another year. They wanted some additional work done" (p. 251).

On page 261 Eden describes a 1988 letter to Brode that indicated fire damage predictions were still a long way off—even though Brode had convincingly demonstrated that the technique worked well enough. She quotes Vice Admiral Michael Colley (p. 272) as saying, "We [didn't] need the fire thing to help us, because it's already a very, very devastating attack." She again quotes Colley that "fire is gravy. Whatever you can get from fire just makes everything worse."

By choosing such quotes from the targeting community, Eden gave me the impression that scientists like Brode and Theodore Postol were the heroes, while the targeting agencies were scoundrels, not wishing the world to see the additional horror of nuclear fires. She certainly delivers an indictment of the damage-computation community.

Finally, the civilian cases, which Eden treats brilliantly, are not parallel to the nuclear one. Each civilian case involved only one or two events, each was resolved swiftly, and the new information was rapidly incorporated into the relevant organizational frames. In contrast, Eden shows that the nuclear targeting agencies did not change for five decades, even when new and good information was provided.

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Peter D. Zimmerman
Kings College, London

Eden responds: I accept Peter Zimmerman's apology and explanation for misstating the yields and burst heights in the first chapter of *Whole World on Fire*. My own figures, however, strengthen my case, not his, as readers of the book will readily see. I also appreciate Zimmerman's statement that "perhaps" he should not have called my book a "diatribe" nor indicated that I saw a "conspiracy"—themes that run throughout much of his original review, but that do not figure in my book. Finally, I did not write about heroes or scoundrels but about how professionals in organizations, in focusing on certain problems, can undermine their own ability to see and solve other problems that can prove highly consequential.

Lynn Eden

Constructing a Theory for Scaling and More

The developments that Geoffrey West and James Brown review in their article "Life's Universal Scaling Laws" (PHYSICS TODAY, September 2004, page 36) are important, but they fit into a much greater theoretical framework. West and coauthors' first paper appeared in April 1997.[1] I published the basic idea behind their approach to the modeling of tree flows one year earlier[2] as part of the constructal theory of organization in nature. Constructal theory, reviewed in my 1997 book *Advanced Engineering Thermodynamics*, 2nd edition (Wiley) and more recently,[3] is now a growing field, with articles appearing regularly, including in physics journals.[4]

Constructal theory is about the generation of shape and structure in nonequilibrium thermodynamic systems—flow systems—in general. Simply put, the constructal law states, "For a finite-size flow system to persist in time (to live), it must evolve in such a way that it provides easier access to the currents that flow through it."[2,3] The constructal law is not about *what* flows—fluid, energy, momentum, goods, or people—but about how the flow system generates its architecture. The three key assumptions that West and coauthors stated in 1997 and that West and Brown reviewed in their PHYSICS TODAY article are present in constructal theory, not as convenient assumptions to make a model work, but as invocations of the constructal law. In particular, a space-filling tree architecture can be deduced from the constructal law. Constructal trees are not fractal objects.

In a constructal tree there are at least two flow modes. The slow mode, which describes low-conductivity, low-permeability, high-unit-cost processes such as diffusion or walking, corresponds to interstices in the tree architecture. The fast mode, which describes high-conductivity, high-permeability, low-unit-cost processes such as flowing water and moving vehicular traffic, corresponds to channels in the architecture. Interstices and channels optimally connect to form a "tree," in which the resistance across interstices is balanced against the resistance along channels.

Because West's three assumptions are consequences of constructal theory, every successful derivation of an allometric law that West and coworkers make is an affirmation of the validity of that theory.

In return, every successful invocation of the constructal law in domains far removed from the living flow systems of West and coauthors is an indication not only that their model is correct but that it is an integral part of a theoretical framework that unites biology with physics and engineering. In addition to allometric laws, constructal theory covers many phenomena that fall well outside the biological scaling reviewed by West and Brown. Some examples are turbulent flow structure in various flow configurations, cracks in shrinking solids, the structure of animal hair, refraction in geographical economics, flight, atmospheric and oceanic circulation, and the structure of power plants and refrigeration plants—that is, the architecture of "human-plus-machine" species.

West and Brown conclude by asking, Is all life organized by a few fundamental principles? According to constructal theory, the answer is yes.

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Adrian Bejan
(dalford@duke.edu)
Duke University
Durham, North Carolina

Adrian Bejan, in his book, *Shape and Structure: From Engineering to Nature* (Cambridge University Press, 2000), sums up work that started well before Geoffrey West and James Brown's early paper in 1997. Bejan discusses the emergence of shape and structure that derive from the purposes of animate and inanimate systems, which must deal with limited resources and other constraints. Animate systems must survive; inanimate systems—for example, engineered ones—must meet specific objectives.

In particular, the ¾ exponent in the relationship between metabolic rate and body size is proven on the basis of pure constructal theory (see section 10.6 of Bejan's book), which avoids the ad-hoc assumption of the tree architecture. Logically, any animal correlation that West and Brown derived in 1997 is evidence that the constructal law is valid.

Alexandru Morega
(amm@iem.pub.ro)
Politehnica University of Bucharest
Bucharest, Romania

West and Brown reply: The theory we developed with Brian Enquist on the origins, implications, and ramifications of universal scaling laws in biology, and which we reviewed in our article in PHYSICS TODAY, is predicated on the idea that life at all scales is sustained by optimized, space-filling, hierarchical branching networks whose terminal units are invariant. The theory not only explains quarter-power scaling but leads to detailed quantitative calculations and predictions of many biological phenomena.

We suggested that the generic underlying principles of the theory are derived from natural selection. Adrian Bejan suggests, however, that they follow from his constructal theory and that our idea was already