Vignettes: Medical Calculation

Consider a precise number that is well known to generations of parents and doctors: the normal human body temperature of 98.6° Fahrenheit. Recent investigations involving millions of measurements have revealed that this number is wrong; normal human body temperature is actually 98.2° Fahrenheit. The fault, however, lies not with Dr. Wunderlich's original measurements—they were averaged and sensibly rounded to the nearest degree: 37° Celsius. When this temperature was converted to Fahrenheit, however, the rounding was forgotten, and 98.6 was taken to be accurate to the nearest tenth of a degree. Had the original interval between 36.5° Celsius and 37.5° Celsius been translated, the equivalent Fahrenheit temperatures would have ranged from 97.7° to 99.5°. Apparently, dyscalculia can even cause fevers.

—*John Allen Paulos*, in *A Mathematician Reads the Newspaper* (BasicBooks)

If you took a sample of one million children and counted their digits at birth, you would find that the vast majority of them had twenty in all, while some might have more and some less. If you displayed this digital variation graphically, you would come up with what statisticians call a normal curve. . . . A practicing doctor would call all of the ten-toe-ten-finger kids normal. In fact, they are a statistical mean.

—*Anne Fausto-Sterling*, in *How Things Are: A Science Tool-Kit for the Mind* (John Brockman and Katinka Matson, Eds.; Morrow)

little more than that they were subject to selection of an unknown sort. Dobzhansky's explanation for chromosomal and genic polymorphism, the superior fitness of heterozygotes, languishes without proof; and it is now clear that his methods could not distinguish among competing hypotheses. Moreover, his work was sometimes sloppy and his methods hardly Popperian. As Lewontin notes, many of Dobzhansky's "experiments" were actually demonstrations of his preconceived notions, and "the conclusions were already in existence *before* the experiments were done." Although Dobzhansky can hardly be faulted for taking on a such a difficult research program, he failed to abandon it when it became intractable, and his lack of success has tarnished his image.

The decline of Dobzhansky's reputation, however, also reflects a general attribute of evolutionary biology: mathematical theory has always been more durable than empirical research. This is not because of any inherent superiority of theory over experiment—our progress has always depended on their interaction—but because much of our history consists of methodological innovations that allow us to apply the same old theories to ever more sophisticated data. So, for example, Dobzhansky's unresolved arguments about the selective basis of chromosome polymorphism became, with the advent of electrophoresis, unresolved arguments about allozyme variation, and are now, with sequencing technology, unresolved arguments about DNA polymorphism. (It is no coincidence that the last two methods were introduced to our field by a Dobzhansky student and a grand-student.) There is, moreover, our curious reluctance to abandon mathematical constructs, such as Sewall Wright's shifting balance theory of evolution, that are attractive but untestable. Such theories linger in the literature for years, nodding at nature but refusing to make her acquaintance. Dobzhansky himself has suffered from the transience of experimentalists. Over the past 30 years, in a burgeoning scientific literature, citations of his work have dropped from 300 to 150 per year, while those of Wright have risen from 200 to 600. One of my colleagues, who has considered his various options for immortality, likes to proclaim, "Why have children when you can have reprints?" But he is an experimentalist, so I always advise him to hedge his bets and procreate.

Reputations decline and citations drop off, but a great deal of inspiration remains in the life and work of Theodosius Dobzhansky. Evolutionists should read him for an education in the history of our field, for his enlightened views on genetics and society, for the sheer joy of his graceful prose, and above all for his approach to studying evolution, now so widespread that we forget its source. Reading Dobzhansky is, however, more than just a dutiful bow to the past. In the midst of the turmoil of World War II, Winston Churchill was rebuked for his preoccupation with British history. His response was that "the longer we look back, the farther we can look forward." The problems raised by Dobzhansky still beset the field, and his works offer refreshment when, weary and befuddled by algebra, we forget that our goal is to understand populations in nature.

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Advice to the Government

Impacts of the Early Cold War on the Formulation of U.S. Science Policy. Selected Memoranda of William T. Golden, October 1950–April 1951. WILLIAM A. BLANPIED, Ed. Directorate for Science and Policy Programs, American Association for the Advancement of Science, Washington, DC, 1995. xliv, 97 pp. Paper, $14.95.

William T. Golden, an investment banker, has long been a devoted and knowledgeable public servant for science. In September 1950, three months after the outbreak of the Korean War, he was asked by the Bureau of the Budget to prepare a report for President Harry S Truman on several key issues in national policy for research and development (R&D). Particularly important were the approaching activation of the National Science Foundation; whether to create an agency for the Korean emergency like the Office of Scientific Research and Development (OSRD) of World War II; and the degree of control that civilians should exercise over military research.

By April 1951, when his inquiry ended, Golden had interviewed about 150 scientists (mainly physicists), military officers, and public officials, some more than once. At the end of each day's conversations, he used a dictaphone to record the details of what was said, accumulating some 200 memoranda, all of which he had transcribed. The transcriptions of the recordings amounted to almost 400 pages, and they are a treasure trove of information bearing on the contemporary state of the military's scientific capabilities and on related issues in federal policy for science.

In *Impacts of the Early Cold War*, William Blanpied has reproduced 27 of these memoranda, provided a list of all the people whom Golden interviewed, and written a useful introduction to his activities that rightly stresses the considerable consequence for science of the Korean War. Blanpied selected for publication those memoranda that supply insight into ongoing issues in science policy and that summarize conversations with figures who are likely to be familiar to readers of the 1990s. We are treated to Golden's encounters with many of the era's key figures in policy-making for weapons and national security—Blanpied thoughtfully provides brief biographical sketches of them—including J. Robert Oppenheimer, Vannevar Bush, Robert F. Bacher, James B. Conant, Lee A. DuBridge, Karl T. Compton, Theodore von Karman, and I. I. Rabi.

Golden's investigation led him to write a report to the President on 18 December 1950—Blanpied reprints the document—that a new OSRD was not needed at the moment, though Golden added that one might be required in a future emergency to provide a place for innovative scientists who might devise radically new weapons yet feel uncomfortable in a military organization. He urged that a different initiative was required—the creation of a regular science adviser to the president. Such a person could bring civilian scientific expertise more strongly to bear on problems of national defense and could initiate a new OSRD should one become necessary. Partly following Golden's recommendation, Truman, on 19 April 1951, established a Science Advisory Committee in the Office of Defense Mobilization, to provide advice not only to the director of the office but to himself on scientific matters, particularly in connection with national defense.

Blanpied also reprints the memorandum that Golden submitted, on 15 February 1951, concerning the new National Science Foundation (NSF). Here Golden dealt in part with the pivotal issue of whether the NSF should conduct military research, an activity that Vannevar Bush's original plan for the agency envisioned it would undertake. Golden had discovered that the military itself was already fostering an enormous variety of research. He recommended that the NSF stick to the strictly civilian task of basic research and training. Policy-making scientists agreed unanimously with that position, and the National Science Board made it the NSF's own.

Golden was a probing inquirer and lucid summarizer, and one wishes that Blanpied had published a broader range of his conversational memoranda. The memoranda Blanpied has chosen to reproduce do illuminate the origins of the scientific advisory system, but only in part. The omitted memoranda reveal in often vivid detail the attitudes toward defense research and civilian scientists in various military agencies at the time of the Korean War, including a resistance to civilian experts on the part of many military officers. They are not only historically valuable; they also undergirded Golden's recommendation for a civilian science adviser. What Golden learned about the military's resistance to civilian scientists, not to mention about diversity and competition within defense R&D, significantly affected his conclusion that it would be advantageous for a civilian scientist to have the president's ear.

Military possessiveness of research also disappointed the early fortunes of the National Science Foundation. Bush, along with many of his colleagues, had expected that once the NSF was created military agencies such as the Office of Naval Research would turn over the basic research projects they were sponsoring to the care of the new civilian agency. Golden endorsed that expectation, but it quickly became clear that the military agencies would not likely cede any research projects (or monies) of consequence to the foundation. That refusal, combined with the lack of new resources for nonmilitary research during the emergency, left the NSF with little to do until after the war.

No matter for basic research and training: Although before the Korean War many academic scientists might have welcomed transfers from ONR to NSF, after it most did not. Defense R&D funding had skyrocketed during the conflict and continued rising after it. Most academic scientists believed, as did Lee DuBridge, the president of the California Institute of Technology, that Congress would not appropriate to NSF the sizable funds that it gave the military and that their universities would "go broke very promptly" if they had to rely on that (see this reviewer's "Cold War and hot physics: science, security, and the American state, 1945–1956," *Historical Studies in the Physical Sciences* **20** [no. 2], 259 [1990]).

They are not going broke in the current post–Cold War circumstances, but they are worried. For better or for worse, since World War II military considerations have been crucial in science policy-making and the science advisory system. Golden's memoranda provide a rich perspective on how, during the Korean emergency, they figured in shaping the future of both. The full set of them can be consulted in three presidential libraries, the Library of Congress, and the Center for the History of Physics of the American Institute of Physics.

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Books Received

Atlas of Volcanic Landforms on Mars. Carroll Ann Hodges and Henry J. Moore. U.S. Geological Survey, Denver, CO, 1994. vi, 194 pp., illus. Paper, $17. U.S.G.S. Professional Paper 1534.