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Review

Dishonesty in Science

By [Richard C. Lewontin](#)

Scientific Integrity in Policymaking: An Investigation into the Bush Administration's Misuse of Science

a report by the Union of Concerned Scientists

February 2004, 42 pp.

[*The Great Betrayal: Fraud in Science*](#)

by Horace Freeland Judson

Harcourt, 463 pp., \$28.00

1.

The founders of the American state understood that the proper functioning of a democracy required an educated electorate. It is this understanding that justifies a system of public education and that led slaveholders to resist the spread of literacy among their chattels. But the meaning of "educated" has changed beyond recognition in two hundred years. Reading, writing, and arithmetic are no longer sufficient to decide on public policy. Now we need quantum mechanics and molecular biology. The knowledge required for political rationality, once available to the masses, is now in the possession of a specially educated elite, a situation that creates a series of tensions and contradictions in the operation of representative democracy.

The problem of the role of elite knowledge in a democracy is an old one. A version of a story in the Babylonian Talmud tells of four rabbis walking in a field, engaged in a dispute over whether an oven of a particular design can be purified. Three hold one opinion, while the fourth has the opposite view. The lone holdout appeals to God, asking that He send first thunder, then lightning, and then that the lightning strike a lone tree in the field. Although each request is granted, the others are not convinced. After all, thunder and lightning are usual natural phenomena and in a lightning storm what is more natural than that a tree standing in the middle of a field should be struck? In desperation the dissenter calls on God to speak directly to them. Sure enough, a voice from above is heard proclaiming "IT IS AS HE SAYS." "So," asks the dissenter, "what do you three have to say now?" "All right," they answer, "that makes it three to two."

Science has replaced Jehovah as the source of privileged knowledge, but the problems remain. How is the knowledge in the possession of the scientific elites to be factored into a process of decision in which considerations of economy, ideology, and political power also enter? Is elite knowledge to be given absolute priority?

Why should we trust scientists, who, after all, have their own political and economic agendas? On the other hand how can we decide by vote when the voters and their representatives have no understanding of the facts of nature?

The American government, like others, has attempted to solve the problem by co-opting scientists into the apparatus of the state in three ways. Most directly it has built an executive apparatus including the president's science adviser, the Office of Science and Technology, and regulatory bodies like the Environmental Protection Agency. Second, it has created quasi-governmental bodies made up of senior scientists, like the National Academy of Sciences and the National Research Council, that are obliged to provide expert scientific advice and evaluation on request from any government agency. Finally, after the Second World War, the state became the chief patron of science, currently committing about \$35 billion annually directly to basic and applied research.

Because of fears that federal support of research would result in political interference with the research process, the pattern established by Congress for the funding of research gave the representatives of the scientific community itself the day-to-day power to decide what research is to be done. Even in funding from the Department of Defense and the Department of Energy (formerly the Atomic Energy Commission), decisions about grants are made by a peer review system in which the reviewers and agency administrators are drawn from the research community and share in its general culture.

As a consequence, instead of producing a mass of grateful recipients of state patronage, public research support has created a large and prospering community of independent investigators—most of them affiliated with universities—with immense public prestige and with effective control over the distribution of funds for research. It is no surprise that attempts by various administrations to make science serve political and economic policy have been met by public opposition from prestigious scientists speaking in the name of disinterested objectivity. The most recent example is the report issued in February 2004 by the Union of Concerned Scientists, whose signers include twenty Nobel Prize winners and, ironically, nineteen recipients of the American government's version of a knighthood, the National Medal of Science.

S*cientific Integrity in Policymaking* accuses the Bush administration, first, of deliberately suppressing scientific findings in the interest of its own ideological and political ends and, second, of packing various regulatory and review boards with unqualified members who can be counted on to favor industrial profits or conservative ideologies over public health and safety. Manipulation, distortion, and suppression of scientific findings in the interest of industries, the report shows, have affected research results on climate change, on mercury emissions and other pollutants, on airborne bacteria, on endangered species and forest management. The government's evidence about Iraq's famous aluminum tubes is said to have been misrepresented in the interests of building a case for war.

Three examples are given of the way in which education and information about scientific findings have been manipulated to support a conservative religious ideology. In order to demonstrate that abstinence-only programs were effective, the Bush administration instructed the Centers for Disease Control not to follow the actual birth rate for participants in an abstinence-only test program, but only their attendance and attitudes toward the program. In order to hide the effectiveness of condom use in preventing HIV infection, the CDC was directed to emphasize condom failure rates in its educational material. Finally, the National Cancer Institute was directed to post a claim on its Web site that abortion promotes breast

cancer although a large study had shown no connection between them.

The report also discusses a number of cases in which government regulatory and review panels were packed with members favorable to the administration. Moreover, it is reported that many potential nominees for federal scientific advisory posts were questioned about their political views and even whether they had voted for Bush. The most transparent manipulation occurred in 2002 when the Center for Disease Control Advisory Committee on Childhood Lead Poisoning was to consider narrowing the criterion of lead poisoning, so that sources of poisoning that were formerly banned became permissible. A panel of new nominees for the Advisory Committee was proposed by the CDC and, for the first time in the history of the committee, nominees were rejected by the direct intervention of the secretary of health and human services, Tommy Thompson, who replaced them with five persons who were previously known to oppose tightening the standard. Two of the five had financial ties with the lead industry.

In April the President's science adviser, John Marburger, issued a reply to the Union of Concerned Scientists, providing an explanation for each of the claimed abuses, including the defense that reports were not suppressed but held up pending more complete studies.^[1] In the case of the abstinence-only program, Marburger says that it was never designed as a scientific study, but as preliminary to long-range evaluation of sexual abstinence. The accusation that seems least easily dismissed is that judgments about political commitment rather than about scientific competence were applied when people were appointed to advisory panels. Obviously each claimed abuse can be explained away, but whether the explanations are convincing or whether a pattern of politicizing scientific policy makes itself manifest will depend on what the reader is inclined to believe in the first place.

In July yet another case of political prejudice was reported.^[2] When Torsten Wiesel, a Nobel laureate in physiology and medicine, was rejected by Tommy Thompson's office as a candidate for the advisory board of the Fogarty Center at the NIH, the director of the center was told by an official from the Department of Health and Human Services that Wiesel had "signed too many full-page letters in *The New York Times* critical of President Bush." Indeed, the government makes no apology for the use of criteria other than scientific competence in its appointment policy. According to the report in *Nature*, a spokesman for the DHHS has asserted that, in addition to competence, a diversity of gender, race, geography, and political opinion is a valid goal of appointments to scientific advisory boards.

This assertion brings us back to the original problem of the relation of elite knowledge to the political process. Studies of climate change, endangered species, acceptable pollution levels, or the effect of sexual practices have not been called forth by pure scientific curiosity. Like all processes that are of direct relevance to human physical and psychic welfare, the costs and benefits of decisions will fall differently on different people. Any amount of lead is bad for your health. So what should be the minimum acceptable level of lead in the bloodstream? Whose bloodstream? Acceptable to whom? The worker in a lead refinery who lives in a badly polluted neighborhood near the plant whose family will bear the cost to their health and longevity of too much lead? The owner of shares in the refinery who winters in Sedona and summers on Cape Cod, whose health is not at issue but who will bear an economic cost of pollution control? A popular bumper sticker in Vermont reads "Another Vermonter for Global Warming." (That, of course, may be a scientific mistake, since general global warming may make Vermont colder.)

My friends who are lawyers insist that the only general rule for deciding legal disputes is "It depends on the jurisdiction," and that rule applies equally to decisions about scientific questions of public import. It is disingenuous to claim that scientists

come to their scientific work without prior ethical, economic, and social values and motivations. Everyone I know who studies endangered species cares about saving them. One never hears that the malarial parasite is "endangered." To do science is to be political if only because it is a political decision to spend some amount of limited human energy and social resources on a particular question. Most scientists are, at a minimum, liberals, although it is by no means obvious why this should be so. Despite the fact that all of the molecular biologists of my acquaintance are shareholders in or advisers to biotechnology firms, the chief political controversy in the scientific community seems to be whether it is wise to vote for Ralph Nader this time. We might expect, then, that the actions of an administration strongly protective of the interests of the owners of capital and identifying itself culturally with religious fundamentalism should be the cause of protest.

If knowledge about the natural world is to rationally influence the decisions of an informed electorate, then people must believe that scientists tell the truth about nature insofar as they know it. While we might agree that prior political commitment could lead us to ask one question rather than another, or to put more weight on the result of a study that conforms to our prejudice rather than one that refutes it, every scientist must agree that outright fraud is beyond the pale. Putting aside the issue of morality, scientific investigation would be destroyed as a useful human endeavor and scientists would lose any claim on social resources if deliberate falsifications were not exposed. So scientists must be on the alert, ready to detect lies arising from within their institution. But this leads to a contradiction. To survive, science must expose dishonesty, but every such public exposure produces cynicism about the purity and disinterestedness of the institution and provides fuel for ideological anti-rationalism. The revelation that the paradoxical Piltdown Man fossil skull was, in fact, a hoax was a great relief to perplexed paleontologists but a cause of great exultation in Texas tabernacles.

2.

Horace Freeland Judson, a science journalist who had previously written a narrative of the development of molecular biology, has now produced a nuanced and sophisticated yet accessible view of scientific fraud.^[3] *The Great Betrayal* is not simply a narrative of scandals, but places various instances of scientific bad behavior in the context of general social pressures and their manifestation in the scientific community. He reminds us that the drive for economic success, personal power, and the gratification of one's ego has led over and over to dishonesty, fraud, and wickedness in business, the church, and the state. Why do we think that the devotees of Newton's laws will be more saintly than those ruled by Cardinal Law?

Judson discusses fraud of three sorts: fabrication, falsification, and plagiarism. Fabrication is the creation of claimed observations and facts out of whole cloth. These are just plain lies. Falsification is the trimming and adjustment of the results of genuine experiments so that they come to be in agreement with a desired conclusion. Numbers may be "adjusted"; there may be a conscious dishonesty or, more subtly, observations that are not in sufficient agreement with the theory may be discounted, often on the basis of ad hoc criteria which the investigator comes to believe are perfectly valid after the fact. There are, after all, many experimental observations that are flawed for one reason or another and ought to be discounted. The problem of how to cull observations honestly is a constant preoccupation of statisticians and methodologists. Judson includes in the category of plagiarism not simply the copying of others' written texts without attribution, but the appropriation of experimental design or data or experimental material or credit for work that belongs to others. An acquaintance of mine once was refused a strain of virus possessed by a prominent investigator, so he cleverly soaked the letter of refusal in an appropriate liquid and recovered enough virus to start his own cultures.

Judson begins his excursion through the history of frauds with those committed by both acknowledged heroes and villains of science: Isaac Newton, Gregor Mendel, Louis Pasteur, Robert Millikan, Sigmund Freud, and Cyril Burt. Some, like Burt and Freud, simply made up observations out of their heads to justify their theories. Burt's fabricated results on the heredity of IQ were so transparent (he even invented fictitious collaborators) as to suggest real pathology. Millikan's measurements of the electrostatic charge on the electron were a classic case of discounting as aberrant the observations that did not fit well with his theory.

Mendel's is a more interesting case, although we have only speculation about his behavior. His reported numbers of different types of offspring from various experimental crosses were too close to his expected genetic ratios of 3:1 and 1:1 to be the outcome of counting a relatively small number of seeds or plants in a real sample. The toss of a real coin one hundred times is very unlikely to give exactly 50:50, 51:49, or 49:51 heads to tails every time we do it. Mendel was probably not dishonest but an unconscious innocent victim of "optional stopping." If you are counting objects of different types without a determination to count a total of exactly, say, five hundred, then eventually you get tired of the whole thing and decide that you have done enough. But if you have a prior theory about how the results should look and are keeping a run-ning tally of the counts there is a tendency, which must be consciously resisted, to say "Enough!" when the results look good. That is not fraud, just bad experimental practice, easy enough to fall into at a time when neither statistics nor psychology was well developed.

A large part of *The Great Betrayal* is taken up with famous modern cases of fraud, among them the so-called "Baltimore Affair." An assistant professor at MIT, Thereza Imanishi-Kari, was engaged in a collaboration with the laboratory of one of the most prominent scientists of the day, David Baltimore, a Nobel Prize winner, director of a large research institute, and soon to be president of Rockefeller University. The actual work of the collaboration involved some postdoctoral fellows and research associates and one result was a paper, coauthored by several of these dependent investigators together with Imanishi-Kari and Baltimore, which made an interesting general claim about the nature of immune systems. A postdoctoral fellow in Imanishi-Kari's laboratory, Margot O'Toole, discovered one day while looking at some notebooks that Imanishi-Kari had made some observations that contradicted the claims of the paper. O'Toole herself had made similar observations but these had been angrily dismissed by Imanishi-Kari.

O'Toole took her findings to authorities at MIT and at Tufts University, where Imanishi-Kari had just been appointed, and this resulted in a meeting with her, Baltimore, Imanishi-Kari, and administrators of the universities. O'Toole's request that the published paper be retracted or changed was refused and that seemed the unsatisfactory end of the affair. Then the story reached the public press and the affair escalated with an investigation by NIH committees, a congressional hearing, and more attention in the scientific and popular press. During the investigations and appeals Imanishi-Kari introduced new documentary evidence of research records intended to support her claims. A Secret Service examination, undertaken at the request of a congressional committee, declared these documents to be fraudulent constructions, thus adding the accusation of deliberate fabrication to the original suspicion of data suppression. However, a highly qualified forensic expert engaged by Imanishi-Kari's attorney examined the documents and concluded that the Secret Service's analysis was "erroneous" and did not support the accusation of fabrication.

As a consequence of the uproar, O'Toole was not reappointed to her postdoctoral position, Imanishi-Kari was suspended from her job at Tufts, and Baltimore was forced to resign as president of Rockefeller University. Finally, after ten years of dispute, the affair ended with the Solomonic judgment of an NIH appeals committee that the charges against Imanishi-Kari had not been proven "by a preponderance of

the evidence." In the end, O'Toole got a research position at a research institute, Imanishi-Kari was reinstated at Tufts, and David Baltimore was made president of California Institute of Technology.^[4]

The Baltimore Affair and a few others are notorious examples of more numerous cases of scientific dishonesty, most of which do not reach the attention of the public. We do not know how often scientific fraud of various degrees of conscious dishonesty occurs, nor can we ever know. Some scientific work is of sufficient general relevance that false claims will eventually be contradicted by other observations and in the end, after a certain amount of stumbling around, the truth about nature will emerge. Moreover, some falsification is in support of what turn out to be true theories, as in the cases of Pasteur and Millikan.

Most of science, however, is immune to future verification or refutation because the link between the reported findings and other active branches of investigation are too weak to allow for contradiction or because the reported results are in support of an already verified phenomenon, or because the subject is so esoteric and narrow that no one else cares to work on it. Judson gives the data from a few sociological surveys in which respondents were asked if they knew of cases of falsification or fabrication, but the results cannot be used to estimate the frequency of such events among all published scientific reports. The claim by a former editor of *Science*, the journal of the American Association for the Advancement of Science, that "we must recognize that 99.9999 percent of reports are accurate and truthful" is either a fabrication or a falsification, depending on whether he invented it on the spur of the moment or was misrepresenting some actual data by throwing in some extra 9's.^[5]

Despite the sophistication of Judson's analysis he has missed a pervasive dishonesty in the practice of science that makes a certain level of intellectual corruption characteristic of the institution. The dishonesty consists in the way credit for scientific research is falsely ascribed to some of the authors of jointly signed scientific papers. He brushes by this practice by referring to "gift authorship," but, far from a willing gift, it is an exaction that the powerful impose on the weak. Science is carried out for the most part in a collection of cottage industries, work groups called "laboratories," but that is a synecdoche. The group is headed by a senior scientist, sometimes accompanied by a more junior but established colleague, and includes postdoctoral fellows, research associates, graduate students, visiting scientists, and technical assistants all working in offices and laboratory rooms clustered around the laboratory director's own space.

It is almost always the case that the laboratory director performs no actual experimental work. There is considerable variation from laboratory to laboratory and from project to project within the laboratory in the degree to which the senior scientist participates in the conception, planning, supervision, and eventual writing-up of the work. In many cases the entire project from conception to publication is without any significant input from the director. Much of what is done, however, is supported by funds from various grants and contracts obtained by the director as the euphemistically named "principal investigator."

Regardless of the actual involvement of the laboratory director in the intellectual and physical work of a research project, he or she has unchallenged intellectual property rights in the project, much as a lord had unchallenged property rights in the product of serfs or peasants occupying dependent lands. The chief product of a laboratory is in the form of published papers and the chief manifestation of the director's intellectual property rights is that he or she will be coauthor on every publication from the laboratory, sometimes including even general review papers and book chapters written by subordinate group members.

Such property rights explain how, for example, Professor Eugene Braunwald of the Harvard Medical School came to be an author, at the age of fifty, of over six hundred publications.^[6] Unfortunately for Braunwald, one of his protégés and coauthors, John Darsee, turned out to be a detected fabricator. One wonders how many sleepless nights Braunwald spent worrying about those other publications. But if laboratory directors as a matter of course claim authorship of work to which they have made no intellectual contribution or only a trivial one then they are, year in and year out, committing an intellectual fraud from which they reap immense rewards of ego, prestige, income, and social power. Moreover, by an unconscious affirmation on the part of the scientific community as a whole, these rewards grow autocatalytic. Robert Merton, the founder of modern social studies of science, called attention to a phenomenon he named the "Matthew Effect" after Matthew 25:29:

For unto every one that hath shall be given, and he shall have abundance; but from him that hath not shall be taken away even that which he hath.

Irrespective of the order of authors on a paper, it is referred to informally and sometimes formally by the name of the best-known author. In laboratory libraries papers are filed under the name of the "senior" author and remembered and discussed under his or her name. I was an indignant witness to an extreme case of the Matthew Effect. A graduate student in my laboratory had published a seminal paper, without my name on it, on an enzyme called alcohol dehydrogenase that everyone agrees has revolutionized the experimental study of population genetics. Shortly afterward I gave a lecture on a different subject, at the end of which a colleague came up from the audience and said, "That was very interesting but what I really admire is your paper on alcohol dehydrogenase." There is some justice in the world, however, and the misappropriation of intellectual property occasionally means that one may try to pass a bad check. The Matthew Effect then does its work. The fraud attributed to Imanishi-Kari becomes known as the "Baltimore Affair." To them that hath it shall be given.

Scientists in training are conscious of the appropriation of credit for their work by senior scientists and they resent it but feel that they cannot protest. It is not that they place no value on the details of authorship. They will fight bitterly with colleagues of their own rank about who should be first author on jointly authored publications. Yet when they too become seniors they will engage in the same fabrications of intellectual credit. The fabrications and falsifications of scientific results that we condemn as fraud are carried out from the desire for fame, status, and economic reward. But the misappropriation of credit by senior scientists arises from the same motives. How can we expect scientists to hold literal truth about nature as an inviolable standard, when they participate, en masse, in a conscious everyday falsification about the production of that truth? That is an aspect of what Judson calls "the culture of fraud" that is far more relevant to scientific honesty than the behavior of the executives of Enron on whom most scientists claim to look with disdain.

Notes

^[1] A report of Marburger's reply is given in *Nature*, April 8, 2004, p. 589.

^[2] *Nature*, July 15, 2004, p. 281.

^[3] The book on molecular biology is *The Eighth Day of Creation* (Simon and Schuster, 1979).

^[4] For a much more detailed history of the case, see David Hull's essay in *The New*

York Review, December 3, 1998, and the book by Daniel Kevles that he was reviewing, *The Baltimore Case* (Norton, 1998).

[\[5\]](#) Daniel E. Koshland Jr., editorial, *Science*, January 9, 1987.

[\[6\]](#) See Judson's account on p. 113.

Letters

February 10, 2005: Kurt Gottfried, [On Fraud in Science: An Exchange](#)

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