Beyond "Bad Science": Skeptical Reflections on the Value-Freedom of Scientific Inquiry

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It is often implied, suggested, or presupposed that science and values are rather like oil and water—distinct sorts of things that are not expected to mix. The relation of values to science either does not exist or is highly problematic. Without denying the problematic nature of this relation—and with the hope of clarifying or beginning to clarify it—I will suggest in this essay another way of thinking about some of the issues included under the rubric of "science and values."

Loren Graham, in his book *Between Science and Values,*[1] has addressed the impact on and relevance to human cultural and personal values of twentieth-century scientific theory. I shall, in this essay, instead raise some questions concerning the reverse relation: how do human cultural and personal values relate to scientific practice (and, indirectly, to the results of that practice). I will not defend any particular theses about the integrity and autonomy of science from social and other values or about the lack thereof, but do seek to foster some skepticism toward conventional understanding of these attributes.

Constitutive v. Contextual Values

It is, in some ways, nonsense to assert the autonomy of scientific practice from values or normative issues. Science is governed by quite real values and normative constraints that are generated by the goals of scientific activity. Thus, if the goal of scientific activity is to produce explanations of the natural world, then the values and constraints involved in considerations of what counts as a *good* explanation will govern such activities. What makes an explanation a *good* explanation is a combination of properties such as truth, accuracy, and precision on the one hand, and simplicity, breadth of scope, and problem-solving capacity on the other.[2] These constitute values by which to judge competing explanations and from which to generate normative constraints on scientific practice. From this point of view, to study the methodology of science is to study the normative constraints on scientific practice. "Scientific practice" and "scientific method," it becomes clear, are ambiguous terms, referring sometimes to the actual practice of scientists, and at other times only to practices condoned or recommended by the above values and constraints.

Those who assert the autonomy of scientific practice from values obviously do not have these normative constraints in mind. Rather, they refer to the beliefs that scientific practice ought to be independent of personal, social, and cultural values, and that the practice of scientists as scientists is independent of their subjective preferences regarding what ought to be. For terminological clarity, I will call these preferences

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contextual values to indicate that they belong to the social and cultural context in which science is done. I will call the values governing scientific practice *constitutive values* to indicate that they are the source of the rules determining what constitutes acceptable scientific practice or scientific method. The issue of the autonomy of scientific practice from values can then be reformulated as two questions—one having to do with the extent to which contextual values influence actual scientific practice; the other, with the relative independence or interaction of the constitutive and contextual values of scientific practice.

Contextual values affect the practice of both pure and applied science in a number of ways. The first has to do with influences on emphases and trends in areas of inquiry. In the United States, for instance, although a certain amount of research, especially biological field research, can be purused at the inclination of the researcher, most work requires major financial support from sources other than the individual, such as from corporate or governmental sources. The pure or applied research funded—and hence pursued—is that which is believed to further governmental, societal, or corporate goals. According to the Mertonian school of history and sociology of science, even before the establishment of this direct and crass connection with goals external to the pursuit of understanding alone, social needs and cultural values (e.g., the interests of the seventeenth-century bourgeoisie) had an impact on the kinds of research pursued. The kinds of questions judged to be important to investigate therefore are determined as much by the social and cultural context in which science is done as by problems and puzzles internal to scientific inquiry.

In contrast to this sometimes imperceptible and sometimes painful channeling effect by the social and cultural context of the research are the explicit policy decisions about the application of technological developments of scientific knowledge. The debates over nuclear energy and genetic engineering, for example, have involved both factual and normative disagreements. The perceived conflict or conformity of these technologies with a number of different values has generated dissonance between the values and thus between social groups which assign different weights to them. To a major extent, the future of the technologies will be determined by resolution of the normative disagreements, that is, by the ascendancy of certain values (e.g., health or popular control of energy sources) over their competitors (e.g., centralized and corporate control of energy sources).

A third major type of interaction involves the potential conflict between moral values and research methodologies, in particular, research with human subjects or research that could have harmful effects. As the risks of harming subjects or of violating their rights (e.g., invading their privacy) have become better appreciated, professional associations have developed guidelines for their members. Morally-based restrictions on experimentation are not new (as the old prohibition on dissection of human cadavers reminds us), not always imposed when they should be (as the fate of syphilitic black men in Tuskegee reminds us), and not always obvious (as the histories of both the Milgram obedience experiments and the NIH guidelines on Recombinant DNA research make clear).

Varied as they may be, each of these interactions between science and values shares in common the externality of the relation. Although those points of contact between science and the values of its social and cultural context may determine the directions of research or its applications, within the boundaries so determined, scientific inquiry itself proceeds according to its own rules. The points of contact with the social and cultural context determine where the rules may be applied—for example, broadly, what questions shall be investigated, or which practical applications of knowledge shall be pursued and which repressed, and, narrowly, what paths to knowledge shall be followed, or which tests and experiments are permissible. The rules are a function of the constitutive values of science, which are themselves a function of the goal of science (assumed here, for the sake of argument, to be the development of an accurate understanding of the natural world). Although the social and cultural values determine which areas or aspects of this world will be illuminated by application of the rules, the conclusions, answers, and explanations reached through the rules are not governed by those values. This represents, I believe, the classical understanding of the relation of knowledge and values, of the value-freedom of science. Specific scientific inquiries are like oil floating on water; they form cohesive pools whose direction of motion is determined by the current, but whose substance remains distinct from its supporting medium.

Recent studies of contemporary research by scientists and by professional observers of science (historians, philosophers, and sociologists of science) suggest that the classical view may need revision. These studies have begun to expose the role of contextual values in the development and acceptance of answers, that is, of hypotheses and theories in the sciences. It is often thought that if such contextual values influence the development and acceptance of hypotheses and theories in science, then the rules of research are transgressed, that is, that such influence represents "bad science." But this judgment can only be made in light of some account of either scientific method or the constitutive values of science which persuasively separates the constitutive from the contextual. Without presuming to be presenting any such comprehensive account, I wish to describe briefly some cases in which values of the context seem to have infiltrated, in increasing degree, the actual practice of scientists as scientists, that is, where contextual values are functioning as or seem to have displaced traditional constitutive values. I shall then discuss the significance of these episodes for our understanding of the (contextual) value-freedom of science.

Some Cases

Interferon

Industrial microbiology has spawned the phenomenon of small firms founded by biochemists, stock in which is owned in part by their founders and in part by large pharmaceutical corporations. These firms have been developed in order to manufacture and market commercially the biological substances produced by the new technologies of Recombinant DNA. In January 1980, interferon was being tested for effectiveness against cancer and as an anti-viral agent generally. In that month, the microbiological firm Biogen announced, in a press conference featuring its director and one of its active researchers, that it was the first laboratory to achieve the bacterial production of human interferon.[3] This announcement was followed by a jump in the value of Biogen's stock, by a major increase in demand for the substance on the part of cancer victims and their families, and by a flurry of corporate-sponsored research as other microbiological and large pharmaceutical firms attempted to climb aboard the "interferon bandwagon." Unmentioned at the time was a similar experiment in Japan that had been published several months earlier, without the fanfare. Six months later, U.S. studies published or presented at oncological conferences suggested that interferon was only marginally more effective and, for some cancers, even less effective than therapies already in use.

This episode involved transgressions against at least two folk traditions in science which, although not of the same status as methodological rules, are connected with the constitutive ideal of truth as well as with considerations of justice. The first holds that scientists do not or ought not profit commercially from their scientific activity. Just as no individual scientist deserves sole credit for her or his discoveries because each stands on the "shoulders of giants," so no one scientist should profit from discoveries made possible by the work of others. The epistemological justification is that scientists ought not to have a stake in the outcome of their research because such a stake might bias their interpretations of results. When scientists own commercial firms formed precisely to profit from the new advances in the biological sciences, the traditional ban on profit-taking is violated.

The second tradition is a rule about the announcement of results: research should be presented first in professional journals or in papers read at conferences. The justification is again twofold. Standard publication provides an easy way to adjudicate priority of discovery. From the epistemological point of view, it is better to submit claims to the scrutiny of those capable of (and interested in) evaluating them before presenting them to the general public. When results are presented at a press conference, peers do not have the opportunity to evaluate the soundness of the results before they are absorbed into public discourse. The dramatic style of presentation required for newsworthiness also diminishes the possibility of critical understanding or evaluation by nonscientists.[4]

In the 1980 press conference, the potential of interferon was, it seems, highly overestimated. Had the announcement of its bacterial production been made by normal or traditional procedures (and possibly had it not been a commercial undertaking in the first place), the information would have reached the public—if at all—along with disclaimers about interferon's therapeutic value. The announcement of results is an activity engaged in by scientists as scientists. The choice of a mode of announcement warranted in the ethic of profit-making over a mode warranted in the ethic of truth-seeking is an instance of the displacement of constitutive by contextual—in this case, commercial—values. In this instance, values of the context are not so much directing research from the outside as entering into and affecting the professional practice of scientists.

Biological Risk Assessment

Another area notoriously permeated by social, cultural, and personal values is that of biological risk assessment. This includes the testing of pharmacological, chemical, and biological agents designed for therapeutic and commercial use and of substances such as asbestos and radioactive gases and metals. Both the widespread development and use of such substances and the appreciation of the need to test them are recent. Thus, the understanding of the logic of testing is developing on the heels of, rather than in advance of, testing procedures. Statements regarding the *safety* of a substance are distinguished now from statements regarding its *risks*.[5] The former are held to be evaluative judgements based on weighing alleged benefits against alleged risks in light of the judger's own goals and preferences. Decisions about safety are properly made by the well-informed consumer—whether individual or government agency. Ascertaining the risks, on the other hand, is not an evaluative but a factual matter, determined by the techniques of scientists rather than of policy-makers.

Determining the risks to humans of the personal or commercial use of certain substances is a task fraught with difficulty. Usually there is no history of use, either at all or in the form or scale proposed, and so epidemiological investigation is not possible. The risk to humans must therefore be estimated on the basis of the substance's use in other contexts—usually through study of its effects on animals of other species—or through comparison to the effects of similar substances or the same substance in other forms. In the past forty years or so, advances in the physical, chemical, and biological sciences have made it possible to introduce many new substances whose benefits are dimly perceived but whose potential harms are quite unknown. When there is no sense of what risks may be involved in the use of a substance or what interactions could occur with other substances, then, as the following discussion shows, this ignorance can have curious results.

In a study of the role of values in testing toxic substances,[6] Carol Korenbrot argues that during the development of oral contraceptives, the selection of risks to be measured was a function of the extra-scientific values of those performing or supervising the testing. For example, in *The Control of Fertility*,[7] Gregory Pincus, who was deeply involved as a major researcher and developer of Enovid, gives an account of the history and biology of oral contraception. While it is true that Pincus's research was supported by a drug company and so may have been influenced by commercial considerations, the one social/cultural theme to which Pincus continually harks in his book is the danger of unchecked population growth and the necessity for its control.[8] Korenbrot suggests that Pincus's explicit commitment to the need for an effective method of limiting population growth strongly influenced how he tested Enovid for effects other than its inhibition of ovulation.[9] Despite available data that show a relationship between estrogens and reproductive tract cancers and between estrogens and blood coagulability, Pincus's chapter "Some Biological Properties of Ovulation Inhibitors in Human Subjects" emphasizes the prophylactic and therapeutic properties and minimizes the hazards.[10] The tests and data he presents are concerned, to a great extent, with conditions that improve (or might improve) with use of oral contraceptives, conditions such as dysmenorrhea, endometrial dysplasia, endometritis, even breast cancer. The data included on conditions that may deteriorate—cervical erosion and thromboembolism—are presented with extensive qualifications and explanations tending to exonerate Enovid as a causal factor.

Korenbrot claims Pincus's extra-scientific commitments biased him in favor of oral contraceptives. This bias in turn led him to look for positive rather than negative effects—that is, for additional inducements to use oral contraceptives rather than possible reasons to be wary of them. This approach makes sense when one remembers that those concerned with population growth are primarily concerned with population growth in Third World societies[11] where there are often strong cultural inhibitions against limiting births or against "artificial" birth control. If users find relief from painful or life-threatening conditions then the case for use is strengthened, especially if relief from pregnancy or reducing the number of children are not immediately perceived as benefits. Perceiving the issue as one of potential opposition to what one believes to be eventually beneficial will lead one to make the strongest possible case. Such attitudes may have contributed to the inadequate testing of oral contraceptives before they were commercially distributed. But certainly, this instance is one of the cases that made the need for more rigorous control of testing by an independent agency apparent.[12]

Plutonium

How to determine risks once they are selected for measurement is another area in which extra-scientific values and commitments can be influential. When we know little about how a given substance interacts with living systems, but there are nevertheless societal or other pressures to know, we often adopt assumptions influenced by values rather than facts. The plutonium controversy in the early 1970s—a controversy regarding the validity of methods for determining exposure standards for inhaled plutonium—provides an instructive example.

Plutonium is known to be highly carcinogenic in very small amounts. Although, under normal conditions, the body has effective barriers against plutonium's entry into the blood stream via the gastrointestinal tract or via the skin, the lungs are highly vulnerable. Plutonium metal, when exposed to ordinary air, ignites spontaneously, forming many small particles of plutonium dioxide, which when inhaled are deposited deep within the lung. Because of plutonium's long half-life, these particles, once embedded, will subject the lung to radiation until they migrate to other parts of the body.

The plutonium controversy centered around how to measure the radiation dose to lung tissue, given that an individual had received a certain exposure. This question involved assumptions about the distribution of plutonium in the lung. As expressed in a *Lancet* editorial, it was a question of "hot spots" versus "hot lungs."[13] The "hot spots" model involved the assumption of a non-uniform distribution of the inhaled plutonium in the lung, while the "hot lung" model produced calculations of radiation dose on an assumption of uniform distribution of the plutonium throughout the lung. Using the "hot lung" model, the radiation for the inhaled plutonium is averaged over the entire surface of the lung, so that a particle of average size is estimated to deliver a dose of 0.0002 rem per year per square centimeter of lung tissue. In the "hot spots" model, the radiation dose is calculated by assuming very intense radiation of the small area of the lung surrounding the inhaled particle. For an average-sized particle, this results in estimates of 500 rem per year average dosage to the irradiated tissue and of 3,000 rem per year to tissues closest to the particle. If calculation of cancer risk is based on radiation dose, the risk will vary tremendously, depending on the model chosen.

In the early 1960s and early 1970s, there was simply not enough information about the toxicity of plutonium at doses low enough to approximate genuine potential exposure. What experimental data there were seem to have been at best equivocal.[14] The scientific grounds for using the hot lung model were the analogy between plutonium and radium, which does produce uniformly distributed radiation. In support of the hot spots model were the facts that plutonium is an intense α-emitter and that α-particles travel no more than three to four millimeters. It was nevertheless necessary to set standards for exposure if the nuclear industry was going to continue to use and produce the substance. The standards set by the International Commission on Radiological Protection (ICRP) were based on the uniform, or "hot lung," distribution model with *caveats* about the uncertainty regarding which model was more appropriate. These standards were also adopted by the U.S. Atomic Energy Commission (AEC).

Several researchers in the United States disagreed vigorously with these standards. Arthur Tamplin, John Gofman, and Donald Geesamen, among others, urged that the "hot spots" model should be used to calculate radiation dosage.[15] Tamplin and Gofman, in *"Population Control" Through Nuclear Pollution*, reasoned that, in estimating hazards to humans and their environments and in the absence of firm evidence indicating one or the other, the safest course was to proceed on the assumption of greater rather than lesser harm.[16] To err on the side of safety was less likely to lead to harm. They explained AEC acceptance of the model predicting a lower radiation dose as a function of the Commission's dual and inconsistent functions as promoter and as a regulator of the new technology. As promoter it hoped for the least risk possible: "Technologists assume the benefits of technology to be superb," Tamplin and Gofman wrote, "and hope the risks won't be severe enough to impede the wide application of their technology."[17] The destructive capacities of this particular technology were so well-known and so horrifying that there was ample motive for all involved with it to learn how to harness it to peaceful and productive uses.

Tamplin and Gofman suggest that, in fulfilling its functions as promoter, the AEC betrayed its mission as regulator. As regulator it ought to have included the costs in terms of human health and lives into any calculation of the alleged benefits. Tamplin and Gofman were, for their part, accused with scientific irresponsibility. Their critics implied that their anti-nuclear bias affected their scientific judgment on this issue.[18] Whether or not the two sides in this controversy were, in fact, influenced in their choice of models by the contextual values and pressures to which they were responsive, at the time of the controversy the degree of understanding of plutonium's behavior provided little besides such values as a basis for choice.

The irony of the plutonium controversy is that research now indicates that calculations based on the "hot lung" model offer a greater margin of safety than those based on the "hot spots" model, because the α-radiation emitted from a plutonium particle is so intense that most of the irradiated cells are killed rather than damaged.[19] Thus the probability of one cell's genetic material being affected in such a way that it would develop a tumor is much lower than it would be if the radiation dose were uniformly distributed. The assumption of a linear relationship between radiation dose and carcinogenicity was inappropriate. The cancer risk estimated on the basis of this new understanding of α-radiation is lower on the "hot spots" model than on the "hot lung" model. Because most controversies about radiation toxicity tend to be resolved in favor of greater rather than lesser risk, this controversy is interestingly anomalous.[20]

Sex Hormones

Social and cultural values can also influence the assumptions required to mediate between hypotheses and theories on the one hand and observational and experimental data on the other. The connection between the ideal of marketplace competitiveness in the nineteenth-century capitalist ethos and the Darwinian notion that a struggle for survival among individuals was the mechanism of natural selection has been frequently noted. Darwin himself cites the work of Thomas Malthus as the source of this crucial element of his theory of biological evolution.[21] Critics of contemporary studies of the connection between physiological phenomena and behavior argue that cultural values similarly affect much of this research.

One of the areas of research which has attracted this kind of analysis is the role that sex hormones play in human sexually dimorphic behavior. Although human hormones regulate a wide variety of physiological functions, the hormones that have drawn the most attention are the androgens and estrogens, the so-called sex hormones. Differential distribution of these hormones between males and females has been cited as causing or influencing differences in behavior between the sexes, i.e., between so-called masculine behavior (aggressive, assertive, dominant, independent, creative) and so-called feminine behavior (passive, submissive, gentle, dependent, nurturing).[22] The efficacy of testosterone in bringing about "aggressive" behavior is one of the more intensely studied relationships. Of particular value for the purposes of this essay are the criticisms of this work by those who object to the move from observations of the testosterone–behavior relation in laboratory animals and humans to an explanation of both male–female behavioral differences and status differences.[23] Elizabeth Adkins, for example, shows that the hypothesis that human gender-related behaviors are hormonally determined or influenced is not supported by the evidence adduced for it.[24] Feminist scientists Ruth Bleier and Freda Salzman emphasize the ways in which value-laden assumptions shape the conclusions that are drawn.[25]

At one extreme of the debate there are claims about human behavior and social organization: that aggressivity is a feature of male behavior rather than of female behavior, that aggressivity is the basis of (male) dominance in animal and human societies, that aggressivity is biologically determined. From these claims, it is thought to follow that male social dominance is natural and inevitable.[26] At the other extreme, there are the experimental data that serve as the evidence for these claims: that male rodents whose androgen production is curtailed by castration at birth engage in fighting behavior less frequently than males who have not been so treated, and that female rodents injected with androgens at birth engage in fighting behavior more frequently than females who have not been so treated. How can one move from these observations to the rather extravagant claims above?

The inferential moves tend not to be made by the same individual in the same paper; that is, researchers experimenting with rats and mice are not also elaborating theories of the biological basis of human social roles. Theorists of human behavior, whether biologists, psychiatrists, psychologists, sociologists, or anthropologists, however, rely on the work of experimentalists (and, it must be said, provide a context in which the pursuit of the experimental work on mammals other than humans makes sense). It is the concept of aggression that provides the continuity from animal experimentation to social theory. Although aggressivity is identified in the experimental situation with fighting (among caged laboratory rats), when appealed to in social explanations it includes not only combativeness but also such traits as assertiveness, independence, and intelligence. Frequency of fighting is treated as a measure of aggressivity and thus as the measure of other qualities. These qualities in turn are perceived as generally desirable human qualities and as contributing to success in this society. Therefore, measuring an individual's success by degree of dominance or position in a hierarchical social structure provides the required link between behavior and social position. The fact that males occupy the dominant positions in Western social structures just affords further confirmation of the theory.

The theory itself, which seems to be providing a scientific expression and validation of the belief in male superiority and the biologically determined character of male social dominance, relies on a number of assumptions to endow upon the rodent experiments the status of evidence for hypotheses regarding the human situation. These include the assumptions that combativeness, competitiveness, assertiveness and independence are expressions of the same trait, that an individual's success is appropriately measured by its position in a hierarchical social structure, and that the relation between behavior and hormone levels observed in rats and mice would also hold in humans. None of these assumptions mediating between observations and hypotheses has itself any direct support. That they should enter into theory construction at all can be explained in part by understanding that they (with the exception of the rodent modeling of human nature) are built into Western culture and therefore into the conceptual framework of the theorists. When expressed as matters of fact, the assumptions are thought to justify a particular ethos and system of distributing the benefits and burdens of the society. Unfortunately, their only true justification is that they support the value system and thus, despite their factual disguise, must be regarded as value-determined rather than factually-determined. Until the role of such features of the social context in theoretical reasoning is signaled by such critics as Bleier and Salzman, they remain hidden, underground determinants of the interpretation of observations, just as the path of an underground stream determines the pattern of vegetation above it.

Discussion of the Cases

In the cases reviewed above, non-epistemological, personal, social, or cultural values have affected scientific practice internally rather than externally. Although it is difficult (and not my intention) to make attributions of individual motivation, each of these cases is most plausibly described as one in which particular practices have been influenced by cultural and social pressures as much as or more than they have been by constitutive, epistemological values. Because epistemological constraints were either inapplicable or overridden, the only other alternative to seeing these practices as chosen arbitrarily is to see them as influenced by contextual considerations. The appeal to the influence of the social and cultural context in explaining these cases is compatible with a variety of ideas regarding how such influence is effective. I have not attended to whether, in any given case, overt or covert pressure, internalization of values, or some other factor has been involved, but have instead presented a series of interactions in which contextual values and scientific practice have become progressively more entwined. At one extreme—that of least interaction—are traditions that have sound epistemological (as well as moral) justification abandoned for non-epistemological (commercial or social) reasons. At the other extreme are inferences from data mediated by values often so deeply ingrained that their assumptive character goes unrecognized. In the discussion that follows I wish to draw some lessons about the general character of these science-value interactions.

In the interferon story, although scientific practices are clearly affected by values, it is still somewhat possible to distinguish the two. On one hand, we find professional practices such as the communication of results at professional conferences or in professional journals, part of whose justification—the necessity of peer review—is epistemological. On the other hand, we see their abandonment in favor of good business practice—the quick public announcement of a technological breakthrough in one's own laboratories, thus identifying the company's name with the product—justified by standard commercial values.

This issue is only one of a number of tangled issues involved in the commercialization of industrial microbiology.[27] Trade secrecy, for instance, generates problems similar to those generated by the requirements of public image and identity. The need to establish priority, rights, and, in a sense, ownership, is already stifling interchange among biological researchers just as alleged requirements of national defense have imposed secrecy on weapons-related aspects of physics and chemistry. Such privatization of knowledge cannot help but influence the development of knowledge if only by insulating mainstream investigation from discoveries in classified and "privately held" inquiry. Moral issues concerning the non-reciprocal flow of information are certainly raised.[28] Such a unidirectional flow raises epistemological problems as well. It has been argued that criticism and the availability of research results to criticism are essential to objectivity, a clearly constitutive value of scientific research.[29] The press conference format may protect claims from disputation or refutation long enough to realize some short-term commercial goals, but the withholding of results for patenting purposes prevents that knowledge from being used to enrich, refute, or otherwise alter hypotheses in mainstream research. The dual circumvention of traditional norms and constraints governing the communication of scientific information which is imposed by commercial requirements will surely produce a distortion in the growth of scientific knowledge.

This concern may be dismissed by observing that the supervenience of constitutive values by values of the commercial context effects only temporary interruptions in the development of scientific knowledge, interruptions correctable over time. However, public confidence in the institutions of science certainly will be eroded, as will also the ability of the scientific community to make the distinctions between the true and false, the sound and the unsound, the plausible and the implausible. One solution might be the adoption of professional protocols enjoining scientists from making a commercial profit from the results of their work. The overriding of epistemologically-sound conventions by non-constitutive values is simply a function of role conflict: individual scientists taking on roles governed by non-scientific values, e.g., the commercial values governing the behavior of business executives. The particular difficulty in the interferon case is that both roles, the scientific and the executive, are focused upon the same activity—the production of a substance with possible medical, and hence commercial, value. The lure of discovery and the lure of profit dangle together.

Disentangling them in this instance, however, will not address the full dimensions of the problem. Commercial values are not the only pressures on the profession. We live in an increasingly technological society, a society increasingly reliant on scientific research for new modes of production and of communication, new materials for consumption, new sources of energy, and regulative guidelines for the use of all. Increased demands for new resources and ways to develop them lead to ever greater pressure on science for immediate answers, regardless of the lack of consensus among scientists.[30] Such impatience will only tend to undercut the time-consuming procedures, such as publication in professional journals, necessary to achieve genuine consensus and relative certainty regarding the possibilities and consequences of particular technologies.

This impact of social needs upon procedures for obtaining consensus is effective in the second set of cases as well. In the plutonium controversy not enough was known or understood about the behavior of radioactive particles in the lung. Yet the ICRP and the AEC were compelled to set standards of exposure because the nuclear energy program was proceeding regardless of the level of understanding. The plutonium controversy itself, which centered about the adequacy of those standards, illustrates the consequences of attempting to exceed the limits of what is known. While the controversy raged, neither of the models for calculating risk was supported by any direct evidence regarding the occurrence of tumors in lungs exposed to low doses of plutonium. According to constitutive norms and constraints, that choice between models ought to have been based on information about the behavior of radioactive plutonium in lung tissue and ought not to have been made in the absence of such information. The inconclusiveness of the factual reasons offered for preferring one model to the other makes it highly plausible that preferences were also influenced by the agreement between the models' predictions and the value-determined expectations of the antagonists. Similar issues arise regarding the legitimacy of extrapolating from animal models to humans whenever the risks associated with human use or consumption of a given substance are determined, whether that substance be saccharin or some internally-administered contraceptive.[31]

Ignorance led to slightly different problems in the oral contraceptive case discussed by Korenbrot. Endocrinology was not, in the early 1960s, sufficiently developed to provide much guidance regarding the potential somatic effects—harmful or beneficial—of estrogen compounds.[32] It is easy to see how belief in the necessity of population control could reinforce testing for beneficial rather than harmful effects. As stricter regulations regarding the testing and release of chemically effective agents are developed, the effect observed by Korenbrot is less likely to occur.

Both the plutonium and the oral contraceptive cases illustrate that where we do not know enough about a material or phenomenon either to predict its activity or to choose appropriate methods for predicting its activity, the opportunity arises for the determination of scientific procedures by social and moral concerns having little to do with the factual adequacy of those procedures. The demand for information about a phenomenon, which originates in the particular context in which research is done, means that choices must be made about what sorts of effects to test for and what sorts of methods will be used in those tests. When ignorance about the phenomenon frees those choices from the constraints imposed by constitutive norms, they are left vulnerable to other contextual pressures—such as beliefs in the social utility of nuclear energy or oral contraceptives—or to skepticism regarding their value or interest in competing concerns, such as health. Constitutive norms and values are not so much displaced (as they were in the interferon case) but replaced, when lack of sufficient initial data makes them inapplicable, by non-constitutive, contextual considerations.

In the final type of case presented, we find values that from the context mediate the relation between hypotheses and data, determining the kind of hypotheses for which the data can be taken as evidence and the kinds of data that can support a given hypothesis. Although the example concerns theorizing about sex differences, the problems it raises have been perceived in several areas of research which attempt to ground social, behavioral, and cognitive characteristics of humans in their biology, whether in their hormones or their genes.[33] Here, too, we can see the role that ignorance plays in providing a route for the intervention of values in scientific argumentation. In the case of sex hormone research, however, the ignorance is often not apparent and the non-factual character of the assumptions involved is obscured because they function for members of the ascendant social group as almost conceptual truths (e.g., about male and female nature, about racial and class differences, about the nature of society). When such interest-laden perceptions of social reality are incorporated into a world view, their role in mediating inferences is not easily perceived.

Not all assumptions that serve this mediating function are equally value-laden. The assumption of continuity between animals and humans which supports inferences from the results of animal experimentation, for instance, is on the surface a value-free assumption. The use of animal modelling in physiological research has made possible a high degree of understanding of human physiology, but the scope and application of this assumption are another issue. Other than desire that it be so, what can explain belief in the appropriateness of animal models in areas (brain structure and behavior) in which humans are so different from other animals? For, in spite of its fact-like appearance, the assumption that animal modelling can support hypotheses about human cognition and social behavior is itself, at this stage, unsupported by evidence.[34]

Without additional sociological and psychological research, it is not possible to ascertain precisely how the various social biases at work in the cultural context affect individual researchers. When, however, a field of investigation relies on otherwise unsupportable assumptions, we might reasonably suspect that contextual values are playing a significant role in determining reasoning in that field.

Conclusion

Each of these types of interaction is quite different from the standard cases of the influence of values on scientific practice, for example, the simple altering or fudging of data to support a hypothesis one wants to be true. Such deliberate bending of what one knows or believes to be otherwise, although a genuine and increasing occurrence,[35] is a caricature of the influence of extra-scientific values on scientific practice. The uncritical judgment of such cases as "bad science" or as a perversion of science is often accompanied by the idea that "good science" (i.e., research which does not fudge data) is value-free. The cases surveyed in this essay, however, suggest that such a contrast is too simply drawn. The issue was not deliberate falsification of data, but more subtle departures from the internal, constitutive norms and constraints of science. In one case, contextual values mandated the bending of rules justified, in part, for epistemological reasons. Although the rules do not govern scientific reasoning directly, their observance or non-observance ultimately affects the growth of scientific knowledge. In the other cases, which did directly involve reasoning, the problem was, in part, the unavailability of data that might determine an issue, or might give the constitutively determined constraints something on which to operate. When such internal guides are non-functional, inferences are vulnerable to direction from the context, whether this is acknowledged (as it was by Tamplin) or imperceptible, as with social bias reflective of establishment values.

We have become accustomed to thinking about the impact of microbiological research on health and health care, the impact of risk assessment work on the adoption or rejection of technologies themselves capable of fostering major social change, and the impact of research on human capacities and behavior on the formation of social policy. In this essay, I have shown that the social and cultural stakes of the outcomes of such research can themselves affect the norms and constraints governing it. Whether this effect is a function only of the powerful and deeply felt interests in these stakes, or whether it is a function also of the character of science, is a question of the degree to which the realms of science and values really are independent of one another. The question is not merely factual but conceptual. Scientific practice has, in fact, been infected by social and cultural values. To what extent can the constraints imposed by the constitutive values of science insulate and protect scientific inquiry from the interest-laden values of the context(s) in which science is pursued? My analysis has sought to make the articulation of this question possible. To answer it, however, we need clearer accounts of the goals of scientific activity and of the points of departure (e.g., what is given in or can be wrested from experience) for such activity. Only with such accounts will it be possible to develop analyses of scientific method powerful enough to delineate the nature, scope, and limits of the integrity of scientific inquiry.[36]

Notes

1. Loren Graham, *Between Science and Values* (New York: Columbia University Press, 1981).
2. See Peter Achinstein, *Law and Explanation* (London: Oxford University Press, 1971), pp. 78–84, for further discussion of the evaluation of explanations.
3. The basic elements of the Biogen interferon story are available in the news section of *Nature*, **283** (24 January 1980), **284** (13 March 1980; 17 April 1980), and **285** (1 May 1980); in the "News and Comments" section of *Science*, **207** (1 February 1980; 21 March 1980), and **208** (16 May 1980); and in *Science News*, **117** (26 January 1980; 15 March 1980; and 7 June 1980). An account is also available in Joel Gurin and Nancy Pfund, "Bonanza in the Biolab," *The Nation* (22 November 1980): 529, 543–548.
4. This is effectively illustrated by comparing the coverage of the press conference even in scientific journals (see note 3) with the scientific paper describing the achievement: S. Nagota *et al.*, "Synthesis of E. coli of a Polypeptide with Human Leukocyte Interferon Activity," *Nature*, **284** (27 March 1980): 316–320.
5. William Lowrance, *Of Acceptable Risk* (Los Altos, CA: William Kaufman, 1976), pp. 8–11.
6. Carol Korenbrot, "Experiences with Systemic Contraceptives," *Toxic Substances: Decisions and Values, Conference II: Information Flow* (Washington, DC: Technical Information Project, 1979), pp. 11–42.
7. Gregory Pincus, *The Control of Fertility* (New York: Academic Press, 1965).
8. *Ibid.*, p. viii.
9. Korenbrot, *op. cit.*, pp. 17–19.
10. Pincus, *op. cit.*, Chapter 12, especially pp. 252–259, 263, and 281.
11. Carl Djerassi, "Birth Control in the Year 2001," *Bulletin of the Atomic Scientists* (March 1981): 24–28; Garrett Hardin, "The Tragedy of the Commons," *Science*, **162** (13 December 1968): 1243–1248.
12. As Ruth Doell points out, it also raises the question of what constitutes adequate testing of a substance that will have borderline effects at certain concentrations of use. The decisive implication of oral contraceptives in thromboembolism required a study involving 60,000 women.
13. "Hot Spots or Hot Lungs?" *The Lancet* (23 November 1974). A more detailed discussion of the hot particle problem is offered in the German Commission on Radiation Protection, "On the Toxicity of Inhaled Hot Particles with Special Reference to Plutonium," *Radiation and Environmental Biophysics*, **15**, No. 1 (1978): 3–11.
14. W. J. Bair and R. C. Thompson, "Plutonium:

Biomedical Research," *Science,* **183** (22 February 1974): 715–722.
15. Donald Geesamen, "Plutonium and Public Health," in *The Social Costs of Power Production,* Barry Commoner, H. Boksembaum, and M. Corr, eds. (New York: Macmillan, 1975), pp. 167–176. Arthur Tamplin and T. B. Cochran, "Radiation Standards for Hot Particles" (Washington, DC: Natural Resources Defense Council, 14 February 1974); Arthur Tamplin and John Gofman, *"Population Control" Through Nuclear Pollution* (Chicago: Nelson-Hall Co., 1970), pp. 177–189.
16. Tamplin and Gofman, *op. cit.,* p. 71.
17. *Ibid.,* pp. 77–78.
18. *Ibid.,* "Foreward" (by Paul Ehrlich), pp. vii–xi, and also pp. 221–227. See also Robert Holcomb, "Radiation Risk: A Scientific Problem?" *Science,* **167** (6 February 1970): 853–855.
19. Lafuma, *et al.* "Respiratory Carcinogenesis in Rats After Inhalation of Radioactive Aerosols of Actinides and Lanthanides in Various Physicochemical Forms," in E. Karbe and J. F. Park, eds., *Experimental Lung Cancer* (Berlin: Springer, 1974).
20. This discussion should not be interpreted as suggesting that the behavior of plutonium, once inhaled into the body, is understood. There are still many questions; for instance, where is it most hazardous—the bronchial or bronchio-alveolar regions of the lung, or the bone marrow to which it eventually migrates? Or what is the most appropriate dose-response relationship to assume?
21. Charles Darwin, *Autobiography,* Gavin de Beer, ed. (London: Oxford University Press, 1971), p. 71.
22. Cf. John Money and Anke Ehrhardt, *Man and Woman, Boy and Girl* (Baltimore: Johns Hopkins University Press, 1972); and Anke Ehrhardt and Susan Baker, "Fetal Androgens, Human Nervous System Differentiation, and Behavior Sex Differences," in Richard Friedman, R. M. Richart, and R. M. Van de Wiele, eds., *Sex Differences in Behavior* (New York: Wiley, 1974), pp. 33–52.
23. A thorough analysis of the hormonal hypothesis and its criticisms is to be found in Helen Longino and Ruth Doell, "Body, Bias and Behavior: A Comparative Analysis of Reasoning in Two Areas of Biological Science," *Signs* (forthcoming).
24. Elizabeth Adkins, "Genes, Hormones, Sex and Gender," in G. Barlow and J. Silverberg, eds., *Sociobiology: Beyond Nature/Nurture?* (Washington, DC: American Association for the Advancement of Science, 1980), pp. 385–415.
25. Ruth Bleier, "Social and Political Bias in Science: An Examiniation of Animal Studies and their Generalizations to Human Behaviors and Evolution" and Freda Salzman, "Aggression and Gender: A Critique of the Nature-Nurture Questions for Humans," in Ruth Hubbard and Marian Lowe, eds., *Genes and Gender II* (New York: Gordian Press, 1979), pp. 49–69.
26. For examples of such argumentation, see Stephen Goldberg, *The Inevitability of Patriarchy* (New York: Morrow, 1973); Lionel Tiger and Robin Fox, *The Imperial Animal* (New York: Holt, Rinehart, and Winston, 1971).
27. Cf. Gurin and Pfund, *op. cit.*; David Noble, "The Selling of the University," *The Nation* (6 February 1982): 129, 143–148; Nicholas Wade, "Harvard Marches Up Hill and Down Again," *Science,* **210** (5 December 1980): 1104; and "Gene Goldrush Splits Harvard, Worries Brokers," *Science,* **210** (21 November 1980): 878–879; Sheldon Krimsky and David Baltimore, "The Ties that Bind or Benefit," *Nature,* **283** (10 January 1980): 130–131; and "Should Academics Make Money Outside?" *Nature,* **286** (24 July 1980): 319.
28. Sissela Bok, "Secrecy and Openness in Science: Ethical Considerations," *Science, Technology, & Human Values,* **6,** No. 38 (Winter 1982): 32–41.
29. This is argued by Karl Popper in the context of standard empiricist analysis of science in *The Open Society and Its Enemies* (London: Routledge and Kegan Paul, 1945), pp. 205–208. See also Helen Longino, "Scientific Objectivity and the Logics of Science" [to appear in *Inquiry: An interdisciplinary journal of philosophy and the social sciences*].
30. This point was made in conversation by Paul Schulman.
31. For a discussion of the problems of such extrapolation, see Lowrance, *op. cit.,* pp. 64–67.
32. There was data for mice, but not for humans, on the connection between estrogens and reproductive tract cancers.
33. The controversies over sociobiology are discussed in Arthur Caplan, ed., *The Sociobiology Debate* (New York: Harper and Row, 1978) and Barlow and Silverberg, *op. cit.* The controversy over the genetic basis of differential performance by race on I.Q. tests is discussed by Stephen J. Gould, *The Mismeasure of Man* (New York: Norton, 1981).
34. See Longino and Doell, *op. cit.,* for a more thorough discussion.
35. William J. Broad, "Fraud and the Structure of Science," *Science,* **212** (1981): 137–141.
36. I wish to thank Ruth Doell for the discussions of which this essay is the fruit and Valerie Miner for her comments on matters of style.