

American Moderns: On Sciences and Scientists

We shall set to work and meet the "demands of the day," in human relations as well as in our vocation. This, however is plain and simple, if each finds and obeys the demon who holds the fibers of his very life.

(Max Weber, "Science as a Vocation")

HAVING just finished writing the book *Making PCR: A Story of Biotechnology*, I thought it was a good time to reflect on the process and the stakes of the experience, to return to some of the original questions I had wrestled with in choosing and defining the research.¹ PCR stands for the polymerase chain reaction, a technology that provides the means to make genetic scarcity into genetic abundance through exponential amplification of specific sequences of DNA. The story is about the emergent biotech milieu in which it took shape—the mid-1980s at Cetus Corporation in the San Francisco Bay Area.

I intended to co-author the book with my main informant, Tom White, a biochemist, formerly a vice-president of research at Cetus, currently a vice-president of research and development at Roche Molecular Systems, a subsidiary of the Swiss multinational company Hoffmann-La Roche, which bought all the rights to PCR from Cetus in 1991 for over \$300 million. Because ultimately I wrote the book myself, strictly speaking the experiment in collaboration across the "two cultures" failed to attain its original objective. I do *not* conclude from this fact that the collaboration was a failure. As the book manuscript neared its completion, I re-posed a question to Tom that I had previously put to him on several occasions. "Why had he wanted to work with me?" Typical of his

mode of operation, he provided a written reply, as it would enable him to formulate his thoughts more accurately. White's response does provide reasons why our joint project stalled, but more interestingly it provides insight about how it had been sustained. Tacitly, it also highlights the course of interactions between two Americans, both resolutely modern—but more of that at the end.

As I learned more about how collaborative research operated in the biosciences, I began to realize that there are many ways to shape a project, and, more subtly, diverse ways to receive credit. As the story of how PCR was conceived, invented, coddled, and pushed into becoming a workable technology demonstrates, White was an expert in managing, facilitating, and contributing to the work of others in both direct and indirect manners. In this light, then, let me re-pose the question: What can be learned from this ethnographic experience about the "two cultures," or, more accurately—as the word "culture" is overly general and rather worn out—about two practices?

I have divided White's response into three parts. Each begins with a section of his response and is followed by my commentary. They are entitled "Ethical Substance," "Mode of Subjectivation," "Telos." The divisions parallel in a loose fashion those employed by Michel Foucault in his last writings on ethics and the "technologies of the self." Readers familiar with Foucault will realize that a fourth category, "ethical work," is subsumed under "mode of subjectivation." The essay's fourth section uses Max Weber's 1917 address to students, "Science as a Vocation," as a device to connect these fieldwork reflections to a larger problematization.

ETHICAL SUBSTANCE: EFFICIENCY, CURIOSITY

TOM WHITE: My original contact with Paul Rabinow was via Vince Sarich, who had been a collaborator of Allan Wilson's at U.C. Berkeley while I was a graduate student there in the early 1970s. Sarich explained that Rabinow was interested in learning more about biotechnology and genetic engineering with respect to its current and future cultural implications. Our first meeting

occurred in early 1990, about a year after I had left Cetus Corporation to work for Hoffmann-La Roche, where I managed their joint program with Cetus to develop diagnostic applications of a powerful new technology: the polymerase chain reaction. My initial interactions with Paul concentrated mainly on the state of the Human Genome Project and its effect on human identity and forensic analyses. There was also some discussion of the gap in time between genetic diagnosis and the development of new therapies. I also unexpectedly became involved in reviewing Sarich's lecture notes for his Anthro 1 course, where he drew parallels between evolutionary models and contemporary behavior and social policies. During this time, I gained trust in Rabinow by reading one of his books and several articles that grew out of our discussions. He was open to criticism and not intrusive as an observer of seminars and lab meetings.

Over several years, projects that I was involved with provided a rich source of material for Paul's study of scientific cultural practices: the issues and testimony from the scientific community on the novelty of the conception of PCR during Du Pont's challenge of Cetus's patents, writing articles for the AAAS on issues surrounding gene patents, using PCR to test (at the request of NIH's Office of Research Integrity) for the presence of HIV sequences in archival samples from the Gallo and Montagnier labs from the beginning of the AIDS epidemic (and to characterize them), and writing letters requested by the nomination committees for the Japan and Nobel prizes. In turn, I became engaged in some of Paul's projects: the Rice University series, a conference at MIT, and his research at the CEPH. These I found very stimulating both from the subjects being discussed as well as the range of people, interests and perspectives that were so very different from those of my colleagues in biology and medicine.

The "Sarich Affair"

Tom White had known Vincent Sarich, a professor of physical anthropology at Berkeley, from 1971 to 1975, while both were working in the lab of Allan Wilson. During the 1960s and 1970s, Sarich

had collaborated with Wilson on breakthrough work on "molecular clocks." They developed new methods of analyzing molecular data, of calculating the divergence times of species such as humans, chimpanzees, and gorillas. Their work fundamentally challenged the prevailing wisdom that the divergence among the great apes was ancient, and provided empirical support for the theory that most mutations are selectively neutral. They were among the first to provide data that changes in the regulation of genes, rather than the steady accumulation of simple mutations, was the cause of major changes in morphology. During the 1980s, Sarich abandoned his scientific research and began to assemble "a worldview," or "philosophy." Sarich labored mightily to combine a variant of libertarianism with an encompassing evolutionary framework. In a fashion typical of autodidacts, Sarich was prepared to explain almost everything. As a venue for his opinions, he began regularly teaching the large (about one thousand student) "Introduction to Physical Anthropology" course at Berkeley—a course usually devoted to primate evolution—and infusing it with his views of society and life.

While over the years there had been some controversy about some of his assertions, especially on intelligence differences between racial groups, these remained isolated incidents. In 1991 Sarich's class was disrupted by students, some in the class and some not, charging him with being racist, sexist, and homophobic. The students objected to Sarich's claims that more hairdressers were homosexual than heterosexual, that there were demonstrable and significant genetic differences in intelligence between groups and genders. Sarich, true to his libertarian principles, always simultaneously maintained that his generalizations never applied to individuals. A public controversy erupted over freedom of speech, the limits of teaching, and the substance of Sarich's claims.

Within the anthropology department, colleagues cast the debate as exclusively a matter of free speech—did one have the absolute right to teach anything in any manner one pleased? The overwhelming response was "yes." Posing the question in this way seemed to me to be overly abstract, formalistic, and juridical. I also knew that once the debate was cast in those terms, it would turn in

circles. When I asked if there would be any reason that I should not be allowed to teach a course in molecular biology—I knew the basics just as Sarich knew the basics of philosophy although neither or us had formal training or credentials in the area—the response was an impatient, and barely tolerant, silence.

Among the physical anthropologists at Berkeley, Sarich was the only one who would engage in any public discussions on the substantive scientific claims. The others defended the principle of absolute free speech, defended their turf through appeals to tradition, and went to the local press with inflammatory and fictitious charges of censorship. As a group, they were riven with ferocious rivalries, barely on speaking terms, and were generally all too ready to criticize each other pitilessly, but under these circumstances they closed ranks. The affair *could* have been the occasion to debate what the new configuration of biological and cultural sciences would look like; however, at Berkeley, it didn't turn out that way.

I had undertaken the ethnographic research at a biotech company in part as a kind of political gesture. As older issues of racial inequality were resurfacing in new guises, it seemed important to understand how much the advances in molecular biology could legitimately contribute to these debates. Further, our department (among others) was engaged in a pitched battle over the future construction of the field of anthropology: were there any *intellectual* reasons to believe that the emergent biological and cultural sciences should be in the same department? Berkeley had been the home to the last major synthesis of cultural and physical anthropology. Sherwood Washburn's work on tool making and evolution, for example, was an inspiration and fit snugly with the cutting-edge cultural anthropology of Clifford Geertz, whose article "The Impact of the Concept of Culture on the Concept of Man" announced what seemed to be a new holistic anthropology but proved instead to be the setting sun of such interdisciplinarity. The growing importance of molecular biology, feminism, textual approaches, poststructuralism, and the like opened a new period from which no plausible and sustained interconnections, to say nothing of synthesis, has yet been forged.

Partially for my education and partially as a direct preparation for a panel discussion that we organized around the controversy, I purchased two copies of Sarich's lecture notes for Anthro 1 from a local note-taking service, "Black Lightning." White methodically worked through them, indicating the points he found scientifically questionable, as did I. We might well have been laboring under Max Weber's injunction, laid down in 1911, "What we hope for from racial biologists, . . . is exact evidence of well-defined connections in individual cases, and so of the decisive importance of completely specific hereditary qualities for particular concrete social phenomena. That, gentlemen, does not exist as yet."² Except that I hoped for nothing from racial biologists. Regardless, eighty years later, Weber's challenge and conclusion remain pertinent.

During the evening forum on Sarich's work (with several hundred people present) a good deal of political rhetoric was displayed. Afterwards, White and I agreed that the forum and its antecedents were more about the uses and abuses of authority than the specific claims of purported relationships or lack of relationships between genetics and behavior. Dispositionally, we were inclined to share Max Weber's admonition: "Ladies and gentlemen, in the field of science only he who is devoted *solely* to the work at hand has 'personality.'"³ A cool, decibel-monitored, focus on "the facts" was for White what it meant to "act like a scientist." However, it seems fair to say that my performance at the forum was far less effective at raising issues of broad import and moving the audience to "take a stance" than several of the other panelists. To that extent both White and I were distancing ourselves from overt political action. Though our dispositions and affective temperaments converged, our goals remained unspecified. A degree of mutual trust and acknowledgment of the other's skills and capacities was beginning to take shape between us.

Curiosity

Arising in part out of the forum, White and I thematized an interest in the question of limits (of teaching, of authority, of arenas of

investigation, of constraints on inquiry from ideology, of institutional, business or legal constraints). We shared a sense that there was something important at stake in these and related events and developments. One of the things we found missing from the imbroglia was a sense of emergence, of the new knowledges and powers at play, that there might be a new set of problems emerging, ones that would pose different demands. This shared sense led, among other things, to an exchange about "curiosity."

PR: What role does curiosity play in science?

TW: To me curiosity is an extremely powerful motivating factor. You know, food, sex, and shelter and stuff like that. Some of the things we are doing here we don't really know where they lead, you could call it instinct or gut level, but we don't know. Henry Erlich [a senior scientist at Cetus] will justify his work on diabetes [as having commercial potential], and that's the right thing to do, but he just wants to know about how the whole thing works. He doesn't give a damn about whatever else is involved in it. That's why David Gelfand [another senior scientist] has boundless curiosity which takes over what he does.

PR: What are the limits to curiosity?

TW: Boredom. I've seen curiosity end for some scientists. When it does end it is a totally recognizable element in them. They no longer have the curiosity. They go home at five o'clock. Or they say, "Well, if you want me to write up the paper, I am going to have to take some time off from work," rather than write it at night or on the weekend like everyone else does. Or when some peculiar result is presented at meetings, they yawn and aren't interested. It is the strangest thing. It's like death in a scientist. They can be productive in a certain sense, but the ability to solve new problems isn't there.

PR: So, curiosity can die and become routine and boredom. But what about the other side: can you have too much curiosity?

TW: Yes, some people are so curious that they never complete anything. One idea after another idea but all at a level that's not very

deep so you can't determine the complexity. What's workable or not. The science fiction mode sets the limits of curiosity when humans mate with apes and meddle with God's work kind of thing. The limits for scientists are that scientists' visions are limited socially. Many of them never even conceive some issues, for example, how the family is defined. These people are thinking about how to detect hemoglobin S from hemoglobin A; they don't think how this will affect families.

PR: Is curiosity a good thing?

TW: It's getting the answer to your curiosity. The mouse pushing on the button to get more cocaine. There is something intensely gratifying about satisfying your curiosity. Scientists just want to know the answer to something. That's why David Gelfand is in the lab every Sunday; he just wants to know how the thing works. Those who are motivated by curiosity have the problem of stopping. They ruin social occasions.

PR: I've written a paper called "The Curious Patient," which was inspired by Hans Blumenberg's chapter on curiosity in *The Legitimacy of the Modern Age*. Blumenberg talks about curiosity as one of the great motive forces of the Enlightenment. He shows how curiosity is something that has been consistently under attack by Christianity and other authority structures. But modernity faces the question of what are the limits to curiosity? There were the German medical and scientific experiments and so many others in the United States and elsewhere which obviously crossed the line of acceptable research or clinical practice. Perhaps there are no self-limiting principles within science itself to tell you not to do a particular experiment? Since curiosity and modernity combine to drive endlessly toward producing something new, perhaps the combination of newness and curiosity's boundlessness is the problem? Perhaps these German scientists who worked on living patients were horrible human beings, but we now know that they were not all horrible scientists. This disjunction is troubling. The core of the distinguished German medical establishment went along with the Nazis. Curiosity has its thresholds. Perhaps it is

ethics or religion or politics or aesthetics (as Nietzsche thought) which limits what one can and cannot do—not science.

tw: That boundary where curiosity goes over into something unethical could also be an element in some aspects of scientific problems. They are always ascribed to power and priority issues, but there is an element of curiosity affecting the ability to interpret your data. Sometimes, people see the results they want to see. Others falsify their experiments, others simply ignore the data that doesn't fit.

Curiosity does get to a point where judgment is required. One boundary to examine is, when does curiosity reach a limit? How would that decision be made? Since there isn't an independent referee, what sort of process does one go through to arrive at a stopping point? What would you draw on to make that decision? Not a simple question: what to do to access resources? That what you do might be unethical? Or socially advisable?

Tom was quite right that molecular biology has no principle internal to its field of practice by which to pose the question of limits. For the human sciences, it is possible to practice them in such a fashion that the question of limits, as well as the reflexive thematization of that concern, constitutes a central dimension of the project itself. Reflexivity, however, like rationality, means many different things. Just as one could formulate a practice that foregrounds political awareness and action, so, too, one could engage in a practice that attempted to make "ethical" action calculable and rationalized. Reflexivity could mean methodologically searching for a normative scale that could be cast in operationalizable terms; work in many areas of bioethics is involved in constituting such a practice. Another direction, the one I pursued, cast reflexivity as an experiential and experimental "problem," one not amenable to the kind of bureaucratic requirements many bioethicists faced, one not directly "useful." This stance entails being curious about scientific curiosity and curious about one's own curiosity. It leads one to thematize the form of life that surrounds, sustains, and undermines curiosity. Thus, even when claims are made to have

discovered "the curiosity gene," the question of what kind of society has posed such questions to itself, why it has sought to produce this type of knowledge, will remain open. So, too, the question will remain of how best to situate oneself in relation to that knowledge, that society, and those goals.

MODE OF OBJECTIVITY: ETHICAL WORK

Tom White: [a]rticles were beginning to appear in the popular scientific literature about PCR's "uncommon" origin [by Kary Mullis, its "inventor"]. These were counterbalanced or paralleled by other accounts from Cetus's management and public relations office. From my perspective, as the former VP of Research at Cetus, none of the accounts gave an accurate picture of the circumstances and milieu that had led to PCR. In fact, they reinforced certain stereotypes about scientists (the unappreciated genius working alone) and science in industry (closed, unimaginative, plodding) that bore no relation at all to the way science was done in one of the first biotechnology companies.

I had made a preliminary effort to write my observations about the history of PCR, but also felt I was too close to the events to portray them objectively. What was needed was someone with a different background than those involved directly, i.e., not a typical memoir from a retired authority figure, nor a journalistic account that emphasized gossip or rivalries. An anthropologist seemed about right to me. Furthermore, an anthropologist would be able to place current scientific practice into a broader framework of other cultural practices and theory, so that whatever was truly unique about the PCR experience, if anything, would be visible.

Consistently unharried amidst a multitude of responsibilities White is not casual. He is simultaneously goal-oriented and systematically flexible in finding appropriate means to attain his objectives. Emblematic of this stance to me was a complex multidimensional chart White had on his office wall, outlining the step

necessary (over the course of several years) to coordinate work toward commercializing a set of diagnostic tests. The chart had replaced an equally large cross-cultural "timeline" of world history. The charts functioned as a map in the sense of portraying objectives and functional juncture points; White never took them as rigid guidelines for action, nor as "filled-in." He prides himself on maintaining close contact with those directly involved in working out technical details, since experience shows him that these are the people who have the most precise knowledge of difficulties and solutions. White coordinates and manages, he is responsible for the larger picture. As he indicates, White was dispositionally prepared for someone to propose a project on the invention of PCR, even if he was not actively searching for such a person. My proposal fit a space on some imaginary chart. That is one reason White responded so rapidly to my overtures. He was clearer than I about the project, at least initially and in the sense outlined above. But again, he left the strategy and details of working it out to me, almost never initiating research directions but constantly being available for responses and help.

White has formulated a set of evaluative benchmarks in order to judge the performance and character of scientists and their work. Upon meeting me, White began evaluating my person and character (credentials, strengths, and weaknesses, personality in terms of potential collaboration, idiosyncrasies, etc.), just as he would with anyone with whom he had or might have a working relationship. After a series of formally arranged interviews about general issues in molecular biology and genetics (the Human Genome Project, etc.), he extended his observations to my preliminary ethnographic work at Roche Diagnostic Research, the complex of labs whose research he directed. I was under scrutiny at the lab meetings I attended (highly technical discussions about diagnostic tests in a variety of stages of development) as well as in my follow-up discussions with individual scientists. He discreetly—and appropriately—monitored both. He and the other scientists and technicians concurred that I was learning enough molecular biology to follow the discussions, and that I was acting responsibly (not pursuing confidential materials on probe design, sharing re-

actions of one scientist with others, etc.). White strongly demurred when I remarked that the "techs" were so responsive to my questions mainly because of my connection to him; he was adamant that although his authorization was necessary for me to be in the labs at all, it was not sufficient. Each of his colleagues and technicians was exercising his or her own autonomous judgment. They knew his management style of monitored independence, maximized flow of information, and critical evaluation of people and procedures up and down the hierarchy. White strongly believed that maximizing autonomy (within a project-oriented structure) produced better results; it was more efficient, it was better for human relations, it maximized responsibility at each level. Modern to the hilt. I showed that I was willing and able to conform behaviorally to this normative structure, and White was wagering that he was correctly evaluating my character as well.

White had three important objectives: he wanted to arrive at an *accurate* picture of the circumstances and the milieu of contemporary biotechnology from which a very important invention had emerged; he represented himself as being too close to the events and the actors to be in a position to portray them *objectively*; in his view, an anthropologist had the right distance and the right perspective to make the event's uniqueness *visible*. White is fully aware that an *accurate*, *objective*, and *visible* account could be put to many different purposes. In his statement he does not make reference to the fact that the meaning of each of these terms is highly contested in the human sciences. Consequently, White's framing of the project is simultaneously, and characteristically, transparent and opaque. It is transparent insofar as it is impelled by a desire to have a literally correct and appropriately coherent account of a major scientific and technological breakthrough. White's project is opaque to me in its unadorned, confident choice of "an anthropologist" to produce such an account. White had been in the Peace Corps in Africa during the 1960s and had learned a good deal (partially from reading anthropological accounts but mainly through his experiences) about the language and culture of the Loma people in Liberia (he was especially intrigued by the

Loma's different use of parabolic language: representations of dimensional space, on a system of counting). He has even published an ethnohistory by a Loma elder.

White was not naive about anthropology; he knew that anthropology was embroiled in major disputes about the status of representations, textuality, and power. His familiarity with this state of affairs extended to *Writing Culture*. Shortly before I began working with him, his wife, Leslie Scalapino, a well-known poet and publisher of an avant-garde press, O Books, had been engaged in a series of polemic skirmishes with the editor of *Socialist Review* that paralleled some of the debates within anthropology that surrounded *Writing Culture*. I invited Scalapino to express her views on these debates. Although I was interested in her views per se, I was also curious about the discussions she and Tom had about the prospects of his working with me.

LS: There were many things I found interesting about your article ["Representations are Social Facts"]. For example, when you are talking about Fredric Jameson's analysis of postmodern culture, it is interesting to me that Jameson, in what has now become a rather famous essay, attacked one of the language poets, Bob Perelman. It's interesting to me because much of what's being done in poetry now is very similar to the kinds of questions you're raising in your article. Much current writing has to do with analyzing perception itself, one's own subjectivity, as the placement of the writer or the viewer vis à vis what's being written. Jameson's argument has been regarded by many people in the literary community as an example of a very conservative, yet Marxist, argument in which he criticized contemporary poetics as dislocated in the direction of being merely fragmentary. Meaning that which is modern is seen as chaotic.

The language group of writers, who are themselves of a Marxist orientation, are proponents of form scrutinizing itself. Jameson is regarded in his essay as demanding a very hierarchical and centralized view of writing where there would be no room for any kind of varied perspective or examination of perspective itself.

Tom told me that you have read an exchange between the language poet, Ron Silliman, and myself whose subject was feminism, gays, and so-called minority perspective as incompatible with avant-garde or experimental work. In this exchange, I was answering an essay by Ron that was published in the *Socialist Review* about six poets, including myself. Actually, our exchange was much larger than what you saw, having occurred over a period of about a year. It was impossible to get any answer of mine to his essay to be published in the *Socialist Review*. They described my initial reply as being too poetic and rejected it on the basis that it was not political discourse. To which I objected that they could not, should not, determine the form and thus the nature of political discourse. Before our exchange could be published later in another journal, the male editor wanted us to rewrite it in a more orderly format. He disagreed with Ron's argument and considered my tone to be hostile. We did not revamp our exchange but shortened it. My original tone in our correspondence was stronger, but this gradually changed. Ironically, the editor was criticizing the later, softer tone.

While the culture wars rage in the human sciences ("incommensurability," "post-identity," "post-narrativity," and the like), White and his fellow scientists—several of whom read parts of each of the multiple drafts of *Making PCR*—never once raised epistemological objections to my approach. They corrected details, they debated the applicability of terms like "technocrat," they insisted on "accuracy" but refrained from objecting to my use of form and interpretation. "It's your book," was the common refrain. This reserve is entirely uncharacteristic of the practice of molecular biologists (or other scientists) among themselves where strong criticism is the norm.

This turn of events remains perplexing to me. Does it mean that these molecular biologists are moderns, i.e., nonhegemonic, pluralistic, even perspectival, about things social? The answer, at least partially, is "Yes." White and his colleagues *are* moderns, and several of the senior scientists had an active interest in writings about

science. Several others aside from White have spouses in the art world. Henry Erlich, whose wife runs the ODC San Francisco dance company, is a keen fan of the novels of Richard Powers such as *The Goldbug Variations* or *Galatea 2.2*. They are also Americans; they exhibit none of the pathos or tragedy that for others has accompanied the "diversity of value spheres." Richard Rorty would approve of their nonplussed attitude.

The flexibility about textual form and tolerance for multiple interpretation when it comes to society must be juxtaposed to the standardization of scientific writing and interpretation to which all good scientists adhere. As the ethnography shows, these molecular biologists would not assent to the following assertion from Pierre Bourdieu: "I hold that, all the scholastic discussions about the distinctiveness of the human sciences notwithstanding, the human sciences are subject to the same rules that apply to all sciences.[...] I am struck, when I speak with my friends who are chemists, physicians, or neurobiologists, by the similarities between their practice and that of a sociologist. The typical day of a sociologist, with its experimental groping, statistical analysis, reading of scholarly papers, and discussion with colleagues, looks very much like that of an ordinary scientist to me."⁴ The obligatory flat joke that greeted me in labs in France and the U.S. was always "now we will be put under the microscope" or "he's here to treat us like guinea pigs." The lines came from scientists who used neither microscopes (computers and PCR machines) nor employed guinea pigs (yeast and viruses). These jokes disappeared immediately once our work was underway; they reveal an initial anxiety about being objectified, nothing more. Ethnographically and experientially, the analogy is a bad one and its use as a metaphor is even worse.

Juxtaposing two quotes, one from Pierre Bourdieu and the other from Kary Mullis, both from methods sections of larger works, one from sociology and one from molecular biology, rhetorically underscores the point. First, Bourdieu: "In order to escape the *realism of the structure*, which hypostatizes systems of ob-

jective relations by converting them into totalities already constituted outside of individual history and group history, it is necessary to pass from the *opus operatum* to the *modus operandi*, from statistical regularity or algebraic structure to the principle of the production of this observed order, and to construct the theory of practice or, more precisely, the theory of the mode of generation of practices, which is the precondition for establishing an experimental science of the *dialectic of the internalization of externality and the externalization of internality*, or more simply, of incorporation and objectification."⁵ Although I more or less understand what Bourdieu means, I have not met a single biologist who does and, for that matter, very few anthropologists who do. Second, in contrast, Kary Mullis, the inventor of the polymerase chain reaction, discusses his "methods" in the following terms: "Oligonucleotides were synthesized using an automated DNA synthesis machine (Biosearch Inc., San Rafael, California) using phosphoroamidite chemistry. Synthesis and purification were performed according to the directions provided by the manufacturer."⁶ Mullis's account is transparent to those working in his field and appropriately opaque to those who don't practice it.

Bourdieu works in a pluralistic scientific milieu and he regrets it. Mullis, on the other hand, lives in a milieu that has stabilized experimental practices and textual genres reporting those practices. When Mullis conceived of the polymerase reaction, he was convinced that he had thought of a revolutionary invention; he was slow, however, to produce either experimental proof or to write up his experimental results. White and others put tremendous pressure on him to do both.

In 1985, at the end of two years of intense work by two teams at Cetus Corporation attempting to make the polymerase chain reaction work consistently and efficiently, the scientists finally were getting results that satisfied them. They decided to publish a paper announcing the new method. Following a commonly employed procedure, they re-ran a set of experiments, so that they would have "elegant" results for the paper. I did something similar in the last draft of the book. However, I had a choice of quite disparate

ways of bringing the project to completion, including emphasizing the disparities and blockages along the way or underplaying them. It seems self-evident that the practices of the Cetus scientists and my own differed. The relation of textual form to experimental practice has been stabilized in the biological sciences in a fashion that the human sciences have never achieved.

Even assuming that one could find an equivalent of the laboratory practices of molecular biology among anthropologists and sociologists, the relationship of the experimental situation to the texts that report on that setting present stark differences. There is a great diversity of experimental practice in the human sciences and a great diversity of textual practice as well. Although the coming triumph of a physical science model in the human sciences has been announced for several centuries—what I have called the “cargo cult view” of science—it has never happened. Empirically the only way it could, would be through political means in which all opposition would be eliminated (defunded, detoured, etc.).

In his remarkable paper entitled “Why Is There No Hermeneutics of the Natural Sciences?” the Hungarian philosopher Gyorgy Markus synthesizes current research in the history, philosophy, and sociology of science. Markus underlines the central dividing point: “Natural scientific activities involve in our culture not only argumentative-discursive but also experimental-manipulative practices. Therefore new knowledge is fixed and accumulated in this field not merely in the form of textual objectivations, but also through incorporation into those lab activities which have the character of craft skills and can only be learned through example and controlled performances in the relevant situations. All the observation terms are linked to that action arena. As a result, an adequate understanding of natural scientific texts cannot be learned/acquired in an intercourse with these texts alone. The craft skill is shared only by the group of specialists.” Markus is fully aware that textual production in the human sciences (itself quite diverse) is linked to other practices as well. His point is that these arts and practices differ. The differences are anything but scholas-

tic. Erasing them through metaphor—Bourdieu’s “experimental gropings”—is ethnographically unconvincing.

Employing categories from “reader-response” theory, Markus asks, who is the implied subject in natural scientific texts? He analyzes the textual devices that contribute to constructing an impersonal subject as the author of the scientific article. Chief among these devices is the imperative to remove all textual traces of the vagaries, accidents, special circumstances, unusual skills, and fortune involved in a piece of work. Markus writes, “The “inscribed author” of the natural scientific texts appears as an anonymous performer of methodologically certified, strictly regulated activities and a detached observer of the results—without any further personal identifying remarks beyond possession of the required professional competence. [. . .] It is essential that the ‘scientific anyman’ could have been the author of the paper.”⁷ The same textual criteria apply to the audience; these devices make possible the complete interchangeability of the author and the audience. It follows that only those who share the experimental practices (often restricted to a subspecialty) are fully capable of understanding and evaluating such texts. Hence, their strength and their limitation are one and the same thing.

Markus concludes that there is no hermeneutics of the natural sciences because there is no need for one. Scientific writing “is culturally defined as of no interest or consequence for a non-specialist reader.” In fact, growing technical mastery and specialization in the natural sciences yield a progressive narrowing of cultural significance because “the view of nature provided by the sciences is no more a world-view.”⁸ This ‘lack of a world view,’ ‘this narrowing,’ this cultural triumph, is itself a condition for the technical efficacy of modern science. In a strict sense, there is no self-questioning within molecular biology. From time to time, there are debates about the ends to which results could be put, political projects that might be dangerous or beneficial; there are occasional discussions about the composition (gender, race, class) of the social body of scientists, but the normative parameters of the textual and nondiscursive practices of sciences like molecular biology are not a question of philosophic debate among practitioners. The

plethora of “worldview” books—with punchy adolescent titles—produced by science journalists and aging scientists underscores the point.

A parallel situation does not exist in the human sciences. No one, above all Pierre Bourdieu himself, has ever mistaken his writings for the social-scientific everyman; their distinction immediately sets them apart. It is true that mimicking the subject and reader positions of the natural sciences is one option available to practitioners of various human sciences. It is, however, only one option among others, one style among others, one rhetoric among others. The utter lack of success in achieving unity in the human sciences (except from time to time under totalitarian political conditions) does not prove that the human sciences will never “come of age,” but it does underscore the distinctive historical and sociological uniqueness in the achievement of such textual unity in the natural sciences. Their strength is their weakness, their weakness their strength.

TELOS

TOM WHITE: “The motivations for my interest in this collaboration are several: there are a number of disturbing phenomena and trends in contemporary science that parallel society at large; there are widespread stereotypes of scientists in industry that are destructive and counterproductive to improvements in health care; there are preconceived notions about the genomic diversity project that are anti-intellectual, patronizing, and perpetuate delusions about our knowledge of the origin and migratory history of modern peoples; an interest in scientific communication and collaboration per se.

As an example of the first phenomenon, leading international scientific journals have increasingly become the vehicles for tabloid news articles on scientific rivalries, misconduct, patent and credit disputes, etc. The use of anonymous sources, leaked confidential documents, erroneous information, and unchecked claims is the new(s) standard for *Nature* and *Science*. These jour-

nals are so influential and reputable in their peer-reviewed articles that credence carries over to their tabloid reports. The editors also occasionally perpetuate the stereotypes of “pure” academic scientists and of industrial scientists only being motivated by money and profit, while conveniently ignoring the corrupting influence of “academic capital”, e.g., membership on editorial boards, grant agency peer-review panels, FDA reviewer of a company’s application while serving as a paid consultant to its competitor, and other conflicts of interest that are not usually designated as such within the academy. These models lead some influential scientists to exhibit behaviors usually associated with creationists or fundamentalists: claiming the absolute moral high ground, a fondness for conspiracy and catastrophe scenarios, and a complete disregard for facts. The trends are certainly rampant in society at large in the form of a willful blindness to societal problems and a delusory momentum to find simplistic causes and solutions while claiming to be pragmatic and revolutionary.

So, one of my purposes in helping Paul write the PCR book was to arrive at an account of an extraordinary genetic discovery that could show how to create an environment for future discoveries. Furthermore, this account would counteract other “histories” that, in my view, perpetuate the very conditions and stereotypes that destroy creativity and the process of discovery. This would be done by providing a cultural anthropologist unique access to the scientists, from technicians to department heads to top managers, etc., of a biotechnology company. Perhaps this would also overcome the misplaced conservatism of private institutions about allowing such access and openness if the company, its scientists, and their anthropological collaborator could conduct themselves in a principled, creative, and productive way. After all, how else can society arrive at the best-informed decisions on the ethical, legal, and social issues arising both from new technologies and information and also from the methods of investigating them?

White wants simultaneously to defend the traditional boundaries of modern science as a practice while extending the institutional sites in which such science can be seen to be legitimately prac-

ticed. Many scientists I have talked with (both within and without the university and both in Europe and the U.S.) complain that *Nature* and *Science* are illicitly trading on the authority as leading scientific journals. The depth of the resentment is striking. Its source seems to be boundary anxiety; any practicing bioscientist today is keenly aware of the politics of science, especially the funding priorities, competition, and its discontents, and so on. They seem willing to tolerate, while lamenting, the current state of affairs as long as there is a protected inner sanctum of science played by the rules. White and his colleagues defend the biotech industry as a legitimate and competitive alternative to the university or governmental labs. When part of the Nobel Prize in chemistry in 1993 was awarded for the invention of PCR, White saw this event as a major threshold validating the quality of science done outside the university.

For White, an anthropologist might serve as a situated observer but one who could explore the effect of his partiality on the subject matter. "Part of an experimental exploration is not knowing what you may find. My attitude about an anthropologist studying PCR, and my colleagues notion of not 'directing' him, is in some measure a desire to let the anthropologist discover something (a pattern, process or paradigm?) about what happened. It was intended to see if he might produce a new form." In that sense, he hoped that the collaboration could make him more productive. He never blurred the distinction between the technical and the therapeutic; he never asked me to play a "facilitator" or "therapeutic" role. White remained attentive to possible operationalizable aspects arising from my analysis. One thing he wanted to know was "how to create an environment for future discoveries." White was engaged from the start in an experiment in which I was being deployed as much as the other way around. This experiment was one he could manage and monitor but not control. There were risks involved for him in such a strategy; my presence might have occasioned interpersonal trouble in the lab; his corporate superiors might have disapproved of the whole project. Neither happened. In the last stages of the writing, a lawyer at Hoffmann-La Roche was informed that legal advisers had suggested that Roche scientists modify a few of their own quotations

(about lawyers) in the book. They told White they were "not in the business of censorship." White never asked for a right of veto of my material, nor was he ever offered one.

Why did he engage in this experiment? Partially his strong desire "to set the record straight," partially his curiosity about what I would produce, no doubt some ego gratification, an affirmation of his own self-image as an unconventional facilitator, resolutely operating in dissonance or at least in a productive tension with his well-heeled surfaces. Mainly, White wanted to practice his science in a certain manner, "to work at the limits of one's ability and curiosity with as few resources and restrictions as possible."⁹ He wanted to be working in an environment in which such collaboration would be considered normal. It was a risk worth taking. After all, White and his colleagues were practitioners who spent their lives in environments of calculated risk. It was part of their professional disposition to try things and see what happens. The biotechnology industry occupies a large place in certain sectors of molecular biological research. The fact that as few as one in five grants are being funded by governmental agencies in the U.S. indicates that fundamental changes are taking place in the institutional arrangements for supporting science that emerged after the Second World War. Without money there is no research in these fields, and an increasing percentage of that research is being done in nongovernmental or university settings. White and his colleagues had chosen to work for a biotechnology company where the literal calculation of financial risk was directly on the agenda in a way that it wasn't in a university environment. They were, as it were, professional controlled risk takers operating in a fluid environment structured by availability of funds, technological limitations, legal constraints, and their dispositions.

DEMANDS OF THE DAY: BETWEEN EFFICIENCY AND WORLDVIEW

Max Weber's lecture "Science as a Vocation," delivered in 1917 on the day of the Bolshevik seizure of power and near the end of the First World War, contains the classic statement of the place and

problem of science (*Wissenschaft*) in modernity understood as a cultural and economic formation as well as an ethos. Weber's remarks provide a touchstone for testing the vexed, if allusive, question of the status of science and modernity, postmodernity, and amodernity in the light of specific ethnographic research on contemporary biotechnology and its practitioners. More importantly, they put forth a hypothesis about the limits of the sciences and the demands posed by those limits.

Weber identified the gradually accumulating spread of rationalization processes, from calculative rationality to bureaucratization, to the methodical organization of everyday life, as the key diacritic of modernity. The mark of modernity—and here is where *Wissenschaft* enters—is demagification (*Entzauberung*). Demagification means *principled* disenchantment, not the total control or general flattening of life. Such principled disenchantment does indeed open the cultural and ethical possibility of nihilism, or postmodernity, but does not entail it. On this often misunderstood point, Weber could not be clearer: “The increasing intellectualization and rationalization do *not*, therefore indicate an increased and general knowledge of the conditions under which one lives. It means something else, namely, the knowledge or belief that if one but wished one could learn it at any time. This means that the world is disenchanted.”¹⁰ In the sphere of meaning, the mark of modernity is fracture and pluralism. The gradual institutionalization of science applied the fracturous blow to older worldviews, not forceably destroying them, only decentering them, relativizing them, placing them in a relational position. Scientific practice created a sphere in which the dark and joyous forces of enchantment and meaning were banished, stilled. Although—the point is often missed—Weber is quite clear, such forces continue to flourish: “Fate, and certainly not science, holds sway over these gods [Aphrodite and Apollo] and their struggles.”¹¹ Many other social and cultural instances give shape to “these gods” beyond fate. Normatively, however, science stands against the principled hegemony of such forces. Modernity is the principle of demagification, not its colonial triumph. Weber follows Nietzsche in signaling plurality of value as modernity's fate, its triumph and dilemma.

This multiplication of cultural possibilities problematizes the place of the knowledge seeker. Science (*Wissenschaft*) only provides the tools for a growing technical mastery of the world, both natural and social. “Natural science gives us an answer to the question of what we must do if we wish to master life technically. It leaves quite aside, or assumes for its purposes, whether we should and do wish to master life technically and whether it ultimately makes sense to do so.”¹² Weber had nothing but a haughty contempt for those spokesmen (of all political stripes) who believed that science could and ought to play such a role. “Who,” Weber ironized, “aside from certain big children who are indeed found in the natural sciences—still believes that the findings of astronomy, biology, physics, or chemistry could teach us anything about the *meaning* of the world?”¹³ Further, who believed science was the path to the Enlightenment goal of happiness, “aside from a few big children in university chairs or editorial offices?”¹⁴ Those who claim today that the Human Genome Project is the “holy grail” fall squarely within the infantile tradition, as do those who take their ant colonies as metaphors or, worse, metonyms of all collectivities. But, so too, do those who see *only* status striving in human existence.

According to Weber, science (*Wissenschaft*) does three things. It “contributes to the technology of controlling life by calculating external objects as well as man's activities. [It] contributes methods of thinking, the tools and training for thought. [It] helps us to gain clarity.”¹⁵ The demand of self-clarification places the issue of *Lebensführung*, or life-regulation, at center stage both as an object of study and as an ethical problem. It is precisely these issues that Michel Foucault's analytic of ethics was grappling with as well. Foucault defines the “telos” of ethical activity as “that activity in which one finds the self. An action is not only moral in itself, in its singularity; it is also moral in its circumstantial integration and by virtue of the place it occupies in a pattern of conduct.” The key terms are the “circumstantial integration” and the “place it occupies in a pattern of conduct.” These terms are uncannily close to and simultaneously far from “technical efficiency” and “worldview.”

What is that circumstantial integration? And what is the pattern of conduct? To what extent did I or could I integrate Tom's goals

into my pattern of conduct? I had no stakes in or fantasies about improving industry (and was not optimistic about the academy), although the goal of interacting with scientists in “a principled, creative, and productive way,” and hence to aid the task of inventing a milieu where we could do so, was at the core of the project. I share Tom’s desire to counteract the “stereotypes that destroy creativity and the process of discovery,” but not his tenacious optimism about fulfilling that desire. I don’t have any programmatic intention of showing “how to create an environment for future discoveries.” Ultimately, for me the thorniest part of the quotation is found at the end of Tom’s statement: “After all, how else can society arrive at the best-informed decisions on the ethical, legal, and social issues arising both from new technologies and information and also from the methods of investigating them?”

How else? For Tom, this phrase was his basic question, one that underscores how he framed the demands of the day. The phrase made me agitated. My experience has not been that “society” often sought to arrive at the best-informed decisions. Or, more accurately, what funding agencies, federal bureaucracies, or legal instances and parliamentary bodies considered to be “informed” was often completely exotic—and irredeemably alienating. When I applied to the Social, Legal and Ethics division of the Department of Energy’s Human Genome Project for a grant to study PCR, I was told PCR was not relevant to the Genome Project, even though admittedly the project would have been basically impossible without it. Social science should study what happened to the discoveries of molecular biology, not the molecular biologists and their practices; Charles Cantor, then director of the Genome Project at Berkeley, told me that PCR “had no social consequences, just like the transistor.” When I applied to the National Institutes of Health Human Genome Project to study the different approach to genome mapping being undertaken in France by the CEPH, I was told there were no significant differences in approach (this was before the French beat the Americans in producing the first physical map of the human genome). The official letter of rejection informed me that my working “hypothesis” about studying the production of genetic knowledge in different cultures was

poorly formulated because all significant human differences were biological, not cultural. When I asked the anthropologist-ethicist-bureaucrat who was charged with overseeing these evaluations whether he believed all significant differences were biological, he told me their evaluation procedures had been fair. When I asked him if he had fallen off his chair laughing, he didn’t respond. With the passage of time, and disregarding the simple asininity involved, I have come to find these responses almost coherent: given their assumptions, how else could an Ethics Bureaucracy operate “to achieve the best-informed decisions”? It is the assumptions—that one needs to show how to proceed from a worldview (theory) to a technical problem (hypothesis) and that bureaucracies should be charged with ethics—that are curious.

Yet, White *had* responded to my overtures by providing me with the opportunity to do research, in part because he thought such research would help him to make informed decisions and to create and sustain an innovative environment. It would make something different happen that he couldn’t entirely control. White, after all, is a hyperactive optimist by temperament. He also has a career record of making environments from which discoveries do emerge and new forms of experimentation are possible. In this light, it is worth noting that White had been approached in the early days of the Human Genome Project about heading the Department of Energy program. He did not pursue the opening, deciding instead to stay in industry. Although I am a hyperactive pessimist, I *had* integrated the circumstantial opportunity into my pattern of conduct. The incidental movements that led me to White, to the polymerase chain reaction, to Cetus Corporation, yielded, as far as I can tell, neither technical efficiency nor a worldview. It produced a book. I gained some experience and perhaps a certain clarity from the experiment. Who, aside from some big children in university chairs, government bureaus, and editorial offices, could ask for anything more?

NOTES

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6. Kary B. Mullis and Fred A. Faloon, "Specific Synthesis of DNA in vitro via a Polymerase-Catalyzed Chain Reaction," *Methods in Enzymology* 15 (1987): 339.
7. György Markus, "Why Is There No Hermeneutics of the Natural Sciences? Some Preliminary Theses," *Science in Context* 1 (1987): 29.
8. *Ibid.*, p. 29.
9. Tom White, pers. comm., January 10, 1996.
10. Weber, "Science as a Vocation," p. 139.
11. *Ibid.*, p. 149.
12. *Ibid.*, p. 144.
13. *Ibid.*, p. 142.
14. *Ibid.*, p. 143.
15. *Ibid.*, pp. 150-51.

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