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TOWARDS A NEW RHETORIC OF SCIENTIFIC DISCOURSE: READING STEPHEN JAY GOULD

A Thesis

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Degree

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Whatever errors remain within this document are solely my own, but much of what is right is the good fortune of having had the right teachers.

ABSTRACT

The character of modern scientific discourse is the legacy of a disciplinary and philosophical tradition in science of equating its discourse with the natural world that it describes. Science claims the availability of an egoless transparent discourse in which authorial presence virtually is effaced, leaving visible only the empirical world to which it refers. The practical effect of the success of this position, both in science and culture, is that science has become privileged as a special source of objectivity and infallibility.

Science's privileged status establishes two difficulties for both science and culture. First, it invites scientific error as a result of scientists' remaining unaware of the impact of their values and disciplinary commitments on their practice of science. Second, it invites cultural misappropriation of and mismanagement of the technological products of science as a result of culture's reliance on science's apparent infallibility and objectivity.

Recently, Stephen Jay Gould has challenged the foundations of scientific discourse by arguing that its claims to objectivity and infallibility are inaccurate and misleading. Gould's own rhetoric of science is a self-conscious rhetoric that both reveals and critiques

its own self-interest. His position has provoked a defense of scientific discourse among its practitioners that demonstrates not only that Gould's position is valid, but that his new rhetoric provides a viable alternative for redefining the relationship between science and culture.

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CHAPTER 1

INTRODUCTION

The history of science invokes controversy where we might assume that only fact would count. In Western culture, science is only one discourse among many. Since Galileo and the seventeenth century, it nevertheless has cast itself successfully, in the mold of "the scientific method," as a special source of truth and infallibility (Rorty 1982). Matthew Arnold's famous lecture on literature and science, from the position of a man of letters, clearly defers to science's authority, and later, C.P. Snow's equally famous lecture on the same topic, from the position of a man of science, clearly accepts that deference. In both cases, the articulated conflicts are about the proper relationship of science and letters to each other, not about the validity of, or the source of the authority for, science's claim to superior knowledge. Recently, however, Thomas Kuhn (1970) and other historians of science have raised substantive questions about science itself, calling into question its claim to special privilege as a discourse and expanding considerably the inquiry into the deep epistemological issue concerning the source of scientific authority.

It is well beyond the scope of this thesis to attempt to resolve now what scholars have been unable to

do so far in Western scientific history. It is within the scope of this writing, however, to investigate the nature of the privilege, the special status, which science has claimed for its discourse and the content and implications of current challenges invoked by those claims. I will argue that, although challenged by modern theories of discourse and the history of science, science has continued to refuse to acknowledge the problems created by the cultural, rhetorical dimension of its practice and that such refusal inevitably creates the invitation to mistake facts for values. When scientists make such a mistake, it may lead to serious scientific error. When culture makes such a mistake, it may lead to a misappropriation of technology and subsequent loss of control of its uses.

We can understand the problems that scientific discourse's claims to privilege pose for science and culture through five explorations. First, a general description of science's attempts to position its own discourse (its institutional conventions for how it will represent itself) as having special authority reveals broadly the extent to which science has been successful as a discourse. Further, this exploration will identify the particular disciplinary and cultural misprisions created by the success of that discourse. Second, an examination of the cultural critique of science in the

writings of Stephen Jay Gould (Harvard professor of geology, biology, and history of science) describes specific ways in which science has created problems for itself and for culture and reveals his special rhetorical position in scientific discourse. Third, an investigation into the responses to Gould demonstrates the crisis which his work provokes in scientific discourse. Fourth, an analysis of Gould's version of scientific discourse suggests a third alternative to the either/or options of current scientific discourse. Finally, a discussion of the crisis invoked by Gould's rhetoric of science reveals a positive alternative to the current rhetorical relationship between science and culture.

1.1 "Science"

The use of the term "science" itself is not without problems in this discussion. More monolithic, perhaps, than most disciplines, science nevertheless embraces a very large number of fields and activities. As Derek J. de Solla Price says:

> . . . we now begin to recognize that the scientific and the technical manpower groups do not form any sort of unity nor even a continuum. They are quite different social groups, comprising, on the one hand, the people who create new knowledge--the scientists,

> theoretical and applied--and, on the other, those who make new things, new chemicals, new machines: the engineers and technologists. The dividing line is by no means clear; there are many people with scientific and technical skill and training who make nothing new, adding neither to our knowledge nor to our artifacts, but work, with their know-how, well behind the research front. (137)

In addition to the distinction between the hard sciences and technology, some would make another between the hard sciences and the soft, such as philosophy, psychology, and social science.

To say that all of these fields of science speak with only one voice or adhere uniformly to identical theoretical or practical values would simply be wrong. They do, however, share certain disciplinary commonalities, including a commitment to the investigation and explanation of the natural world and the real (those physical objects and phenomena and those abstract phenomena that exist materially, independently of our thoughts about them). In addition, they traditionally have shared a profound belief in the efficacy and the necessity of what is commonly called the "scientific method," that is, their rules for <u>how</u> they will investigate. These shared commitments unite a large

number of fields with very different subject matters into the broader discipline of science.

In this discussion, it is necessary that I refer to all of those fields under the one rubric of "science," knowing full well that such a strategy invites risk. This usage is neither a reductionist attempt to level the tremendous diversities to be found in science, nor a willful attempt to avoid the particular through generalization. Instead, it serves several purposes: first, as a practical matter, it avoids bogging down every point under discussion in non-crucial, but lengthy, explanations of exactly which fields are being referenced. Second, and more important, I mean to focus attention on certain specific and very troublesome aspects of recent scientific traditions of discourse that cut across virtually all fields of science. In doing so, I am aware that some fields (or areas of some fields) will be included at times in judgements that apply only generally, not specifically.

Third, I mean specifically to contrast "science" to "culture" and to distinguish them as representing two distinctly different systems of thought. In the context of this discussion, Clifford Geertz's description of culture applies: the framework of beliefs, symbols, and values that we use to interpret human behavior and interactions (144-145). In addition, however, "culture"

refers also to the nexus of social, political, religious, and ideological belief systems that distinguish societies from each other. Such systems not only need no grounding in scientific thought to be considered valid in a culture, but as we shall see, culture often resists such grounding. It is the rhetorical relationship that exists between science and culture that makes it necessary, and useful, to oppose them categorically here.

Finally, I consciously use "science" here to mean only specific patterns of scientific discourse as an echo of a rhetorical strategy common in scientific discourse. Even among discussants with highly competitive claims (or with claims that have nothing whatsoever in common) it is not at all unusual to find the term "science" used as if it refers to all scientific activity when context makes clear that it cannot. When highly respected historians of science, notably Thomas Kuhn (1970) and Derek de Solla Price (1986), routinely refer to science as a monolith, we may fairly read such usage as an indicator of how science is commonly conceived. Price begins the chapter in which he contends that scientific "groups do not form any sort of unity nor even a continuum" this way:

> In the atomic and space era, science is rapidly becoming far too important to be left to the scientists. (135)

As we shall see in looking at the writings of some

scientists from several different fields, Price might not get consensus on his specific argument, but he should find little objection to his hypostatization of science itself.

1.2 Science's Traditional Discourse Claims

For more than a century, the discipline of science has exhibited a suspicion of rhetoric, insisting on the availability of a transparent discourse with which to describe its field of inquiry. This kind of discourse supposedly suppresses all authorial presence and foregrounds only the empirical world it describes. Language itself, however, as the only medium available to create such an effect, has been long a source of genuine difficulty for science. Historical and sociological studies have called into question the validity of discourse claims to transparency, revealing language as culturally mediated in such a way as to suggest that transparency may be a hopeless goal (Kuhn 1970, Rorty 1982). Science, however, faced with what it often sees as unmediated and "pure" nature, has been unable to accept fully the results of such studies.

Even corollary scientific evidence has not been persuasive. Quantum physics' discovery that the mere presence of an experimenter precludes some observations, while ensuring others, convinces only some quarters of

science that it probably is useless to hope for unmediated descriptions of nature. Instead, science has continued to insist that language is flawed but can be rid of its worst imperfections and can perform, in the right hands, as science wants it to. In The Consequences of Pragmatism, Richard Rorty contends that

> [t]he moral which seventeenth-century philosophers should have drawn from Galileo's success was a Whewellian and Kuhnian one: viz., that scientific breakthroughs are not so much a matter of deciding which of various alternative hypotheses are true, but of finding the right jargon in which to frame hypotheses in the first place. But, instead . . . they drew the moral that the new vocabulary was the one nature had always wanted to be described in. (193)

Rorty, among others, has noted that this insistence that the language used to describe science is "natural," and by extension, therefore, value-neutral, is part of a long-term strategy which has characterized science's attempts to ensure its autonomy from culture.

In Rorty's description, science traditionally has, at least since Galileo, defended its interests as empirical, not interpretative, thereby distinguishing itself from such disciplines as history, literature,

philosophy, and the social sciences. This distinction between the empirical claims of science and the interpretative claims of other cultural systems of thought has become known as the fact-value split. The resulting dichotomy has been the source of science's claim to a status for itself as capable of revealing the real in a particularly truthful and, by extension, infallible way. That claim is strengthened by a subsequent, general cultural acceptance of the legitimacy of science's claim to an autonomy for its content, outside of and independent of culture, and by the triumph of technology.

Nevertheless, science has deemed it necessary at various times to defend itself from perceived encroachments by culture. While from the 17th century on science has insisted on the legitimacy of a natural vocabulary, by the end of the 19th century the positivists found it necessary to embark on a full-scale project to fix language systematically and finally. Positivism's aim was to demonstrate that "science is the only valid knowledge and facts the only possible objects of knowledge" and to deny "the existence or intelligibility of forces or substances that go beyond facts and the laws ascertained by science" (Abbagnano 6: 414). Everything else is mere

value, interpretation, or wish-fulfillment, hence a reaffirmation of the fact-value split.

Bertrand Russell, the early Ludwig Wittgenstein, and the Vienna circle of logical positivists pursued the positivist project into the 20th century in an attempt to validate the claim that there is an ideal language that can describe the real world as it is. At the end of the 20th century, positivism's legacy to science is a continuing assertion that a transparent rhetoric is available which reveals the natural world as it is, effacing the author so that such revelation appears to be without human agency, that is, free of human motives, values, or intentions.

1.3 Culture's Response to Science

Culture's response to science borders on a near-schizophrenia, which is the source of science's intermittent attempts to defend itself from culture. On the one hand, culture has embraced science with open arms, endorsing its disciplinary claims to autonomy and a special authority to speak for and about the empirical world. Culture has been particularly zealous in its encouragement of technology, appropriating science's discoveries at every turn for the ostensible betterment of all humankind. Microwave ovens, better stereo sound, cheap power, and improved (by decades) longevity are all

testaments to cultural approval of scientific enterprise.

On the other hand, culture often resists definition of itself on science's terms. Where science's claims have encountered important cultural belief systems, culture repeatedly has demonstrated itself willing, when threatened, to reject science without hesitation. As long as the relationship produces convenience, high-quality leisure, a reduced cost of living, or good health, culture has no quarrel with science. When, however, science proposes that humanity's antecedents include simians, that man's best friend makes a perfect laboratory test animal, that it should be policy of the United States to explore the solar system, or that American scientists should teach Russian scientists what they know about computers, culture's love affair with science can turn quickly to hostility. Suddenly, science finds that it has run afoul of cultural values as expressed in religion, society's relationship to its pets, national spending policies, or patriotism/nationalism.

Culture's seemingly unpredictable relationship with science makes no sense in one way. At one moment, it appears that culture accepts completely science's claim to describe accurately the empirical world, but in the next, it uneasily charges the discipline with lying, or at least obfuscation. That contradictory response, however, is a fairly accurate indicator of the genuine

ambiguity with which science and culture do at times regard each other. Culture, however, has been unable to rise above its greediness for science's expertise, its authority, and its technology. From time to time, anxiety may cloud culture's pursuit of a relationship with science, but those instances produce insufficient distress to justify giving it up completely. As a result, and as we shall see, the partnership has been an extraordinarily successful one, although not without cost to both parties.

1.4 The Success of Scientific Discourse

As a discourse, science has been successful on a scale that virtually no other discipline enjoys. It has achieved the unique distinction of laying claim to possession of reliable and verdical knowledge of the natural world and the real. In addition, science has seen culture, in spite of its spells of doubts, commonly translate that claim into scientific infallibility. By establishing the language of its discourse as identical to that of the vocabulary "nature had always wanted to be described in," and by tenaciously defending its autonomy, science has exempted itself from certain rhetorical pressures imposed on other discourses. In particular, science has freed itself from the constant demand that it prove its own legitimacy. The resulting freedom has

allowed the discipline, for most of its history, significant latitude to pursue its interests relatively unconstrained by cultural norms and priorities. Close examination of scientific discourse's evolution, however, reveals its success to be more troubled than either most popular or disciplinary assessments might suggest.

At least two developments of the 19th century were responsible for fostering the modern success of scientific discourse and the rise of the modern scientific worldview. One was the Benthamite debates, and the other was the tremendous explosion of scientific activity into the public arena. By the time the Benthamite debates were over near the midpoint of the century, science had achieved the imprimature of university approval and a place in its curriculum. The university not only opened up a new forum for the transmission of science's mission and content to the wider culture, but it also struck a formal relationship between the two. That relationship has not always been easy, but it has served nevertheless to seal in the public mind science's authority as a discipline.

While science's entry into the canon of academic disciplines was vital to its success as a discourse, that event alone assured little more than a status in the public mind as arcane knowledge. It took the

additional force of science's eruption into everyday life to guarantee its popular success. The history of the changes that scientific discourse underwent in the process is the history of the emergence of the modern scientific worldview. In tracing scientific discourse's development over the last four centuries, Bob Coleman concluded that it has been a long time since that discourse could make an honest claim to being free, at least relatively speaking, of cultural involvement. In a 1987 article, he argues that scientific publications originally, in the 17th century, were very different than those of the modern era:

> . . . early scientific papers . . . included something essential about the nature of scientific endeavor--its difficulties, the prospects for failure and the flexibility necessary to do scientific work. While a frank discussion of troubles was fundamental to early scientific writing, modern science favors an image of perfection. (1)

Coleman cites a number of examples to demonstrate that originally scientists had few qualms indeed about admitting in writing to error, both possible and real, giving the impression of a kind of conversation that rarely occurs in modern scientific journals (1, 45). The closest that we get in modern discourse to this

model is the rebuttal article printed in response to criticism.

Coleman accounts for the change, which had fully emerged by the mid-1800's, as having several sources, but none more powerful than economics. By the end of the 1840's, the integration of the scientific and the industrial communities had occurred (46). He recounts Dirk Struik's analysis of the development of New England science during this period, finally concluding:

> Certainly commerce shaped scientific prose during that decade. In 1847, as part of the groundwork for establishing the Smithsonian Institution, the American Academy of Arts and Sciences reviewed a plan for grants to scientists. The academy, while warning of the dangers of a scientific bureaucracy, approved the plan in language derived as much from industrial capitalism as from public-spiritedness: 'Narrow circumstances are too apt to be the lot of genius when devoted to scientific pursuits; and the necessity of providing for personal and domestic wants too often absorbs the time and faculties of those who might, if relieved from cares of this kind, have adorned their age and benefited mankind . . . . The efficacy of market upon

> production is not limited to the creations of physical labor.' That is a long way from the image of the benevolent gentleman-scientist created by Franklin a century earlier. (47)

Not only is that characterization a long way from Franklin, it is also a very long way from the dream of a scientific endeavor uncontaminated by culture. Science, at this early juncture of the Industrial Revolution, was already appropriating the language of industry to defend its other appropriation of managerial technique (bureacracy) in support of its own goals.

Moreover, Coleman suggests that instance was not the only appropriation of managerial technique. Following industry's lead, science streamlined the way it reported scientific discovery. Suppressing difference and error became common: getting to the bottom line, the results, was the new goal. The time-honored system of refereeing hid in private communication "the most caustic disputes . . . thereby completely suppressing discussion of research difficulties" (47). Public understanding of science in the age of commerce changed radically. As Coleman notes:

> Once science entered the public arena, its powers were taken for granted (even wildly

> exaggerated), as polemicists began debating its social role. Increasingly, science was seen as a means for creating wealth (and political power). Controlling its products, and claiming its prestige, became more important than understanding how it worked. (47)

One important effect of this trend was the Victorian era's transformation of science along the lines of class. Whereas science was a gentleman's pursuit and "pure research" a province of the upper class in England, in America it was the middle class, its imagination seized by technology, who became science's most vocal champion. That distinction's effect, if not always its specific form, has prevailed into the late 20th century with applied science and technology the darling of industry and culture and "pure research" the province of the academy. Coleman observes:

> . . . we have, I believe, faithfully preserved the Victorian view that success, not struggle, is the essence of scientific endeavor. (47)

It is easy to believe that Coleman's evaluation is correct in light of the inordinate pressure currently being exerted on University of Utah scientists to prove

without delay the merits of their claims about fusion power (Begley 54).

The long-term effects, however, of science's public success and the resultant changes in its discourse have powerful implications for those who would argue that popular, and even disciplinary, conceptions of science's private struggle are completely valid. Coleman underscores the assessment of Connie Osborn, a neurophysiologist who "has argued convincingly that nowadays even students with undergraduate science degrees seem unaware of the limits of science" (47). He concludes:

> Taken together, modern scientific writing, both the popular and the professional type, makes science look far more precise and reliable than it really is. In an age of immense gambles on the powers of science, it seems crucial to understand accurately scientific fallibility or infallibility. But modern scientific prose (for all its virtues) conceals much more than it reveals. (47)

Coleman suggests that the vaunted transparent language of scientific discourse may be deceptive simply <u>because</u> it appears to be otherwise.

Irrespective of Coleman's claims, however, and the instances in which culture has rejected science's

claims, its discourse continues to enjoy extraordinary cultural success. The triumph of technology has reinforced science's authority to make more arcane claims. No educated person seriously doubts science when it claims that two atoms of hydrogen mixed with one atom of oxygen produces water, that the sun is the center of the solar system, or that the universe is expanding, although we may have no personal knowledge on which to base our acceptance. More important as an indicator of our cultural belief in science's authority, however, is our willingness to accept science's word for it that $E=mc^2$, that gravity may not be a force but a function of the space-time continuum, or that time dilates as it approaches the speed of light, although there may be only ten people in the entire world who genuinely understand all the complexities involved. We even find it believable when science predicts the existence of subatomic particles, names them quarks, and proceeds to categorize them into flavors, although no one, nowhere, has ever seen a quark (Davies 23-25).

Science benefits, then, from a very high degree of cultural consensus that when it speaks it does so from a position of veridical knowledge. In contrast, other disciplines do not enjoy such givens. Most interpretive disciplines, on virtually any weekday in virtually

any university classroom, can be found defending variously their claims to relevance, importance, or resemblance to the real. Literature, history, and art may all be competing to demonstrate the validity of their respective representations of a given event, each arguing the merits of its special insights. The discussants involved, irrespective of how closely individual personal experience corresponds to the claims made, may very well fail to reach a consensus that any of these disciplines are sufficiently reliable to warrant labelling their claims as "the truth."

Conversely, armed with no other evidence than a Mr. Wizard demonstration, a library book, and their notes from a Physics 101 lecture, these same skeptics could very well agree to set about the business of making an atom bomb, with every confidence that they could succeed. Westeners everywhere routinely choose to enact the claims of science to truth and the real over those of virtually all other disciplines. That tendency does not prove science's validity over other disciplines, but it demonstrates that its discourse has been incredibly successful in a way that others have not.

In addition to having established its descriptions of the natural world and the real as veridical and reliable, science has also claimed that the natural and the real are value-neutral, or objective. That which

exists in nature is neither inherently good nor bad, moral nor immoral, but simply is (Gould HT 32-46). Whatever value we humans attribute to that which we find in nature is a function of our desire, not of the object itself. Science observes, records, and analyzes the destruction wrought by the predation of the strong species on weak, lightning fires in virgin forests, and lemmings' seaside suicides and concludes that they are all necessary to the very existence of the natural order. As such, these phenomena are inherently value-neutral, and science goes to some lengths to describe them in such a way as to confirm that neutrality.

By extension, science has appropriated for its discourse the same value-neutrality that it claims for the natural world. If descriptions of nature are in the language "it has always wanted to be described in," then that language must be as objective as that which it describes. When Richard Feynman observes that antimatter particles may just be ordinary matter that moves in reverse time (Noble 167), the layperson's reaction (as well as many a scientist's) may be, "If you say so." However incomprehensible quantum physics' descriptions of the subatomic world may seem to us, we nevertheless do not imagine that science has misspoken itself.

The very incomprehensibility of some aspects of the natural world (to both scientists and laypersons) makes its description all the more impressive. When Heinz Pagels characterizes properties of the subatomic world as "quantum weirdness" (48), the phrase seems incredibly, singularly apt. He does not appear to us to claim too much when he titles his book The Cosmic Code: Quantum Physics as the Language of Nature. Scientific discourse really does describe the real.

From such difficult leaps of acceptance, then, we may have little difficulty in finding plausible Arthur Jensen's far more accessible claim that his studies indicate that blacks' poor performance on IQ tests is caused by genetics, not environment. Even if we immediately suspect a bias in his methodology, we see quickly that because of their apparent authority, his claims can be neither casually, nor easily, dismissed Even if we are not certain that he is operating in a genuinely autonomous area of science, the continuing positivist claim that a transparent discourse of science is possible bolsters Jensen's conclusions as value-neutral, couched as they are in the discourse of science. When science speaks, people really do listen.

1.5 The Rise of the Two Cultures

In the process of establishing its disciplinary autonomy, science has refused to admit a cultural, rhetorical dimension of scientific practice. That is, as a discipline it does not acknowledge any influencing of science by its social context or that any nexus of social, political, psychological, and ideological contexts make and authorize its discourse. From the onset of the changes in its discourse in the 1800's, science's acknowledgement of culture has been limited to pointing out the ways in which it is clearly distinct from and apart from culture. Its acknowledgment of rhetoric has been limited to recognition of "mere rhetoric" only. As culture has found so far neither need nor grounds on which to challenge science's claim to an absolute autonomy, science and culture speak to each other across an unbreached gap. This practice has its modern apotheosis in an ongoing conversation that has been popularly characterized as the Two Cultures debate.

For an 1880's tour of the United States, Matthew Arnold revised his "Literature and Science," first published in 1882, as a lecture in which he asked

> whether the present movement for ousting letters from their old predominance in education, and for transferring the

> predominance in education to the natural sciences, whether this brisk and flourishing movement ought to prevail, and whether it is likely that in the end it really will prevail. (482)

Arnold's address is significant both for its timing and the choice of location for this particular address. It indicates that the general effect of the burgeoning interest in science was already a concern in academic circles. Arnold himself concedes the extent to which, in the popular mind, science already had lain claim to an authority superior to letters:

> And there is always a tendency in those who are remonstrating against the predominance of letters in education, to understand by letters belles lettres, and by belles lettres a superficial humanism, the opposite of science or true knowledge. (483)

Arnold did not, of course, resolve the question, although he strove mightily to create a common ground upon which both letters and science might meet. Bob Coleman's description of the way that scientific discovery and the industrial explosion of the 19th century fueled each other's growth, with science heartily and publicly endorsing the economic appropriation of its products, makes clear that Arnold

had every reason to think common ground could be found. In referring to an 1887 London Times article on science, Coleman says,

> the pre-eminent scientific journal, Nature, reprinted [it] with approval. It left no doubt about the course of 19th-century science: 'The keynote of the Victorian era is the development of scientific research, the concomitant growth of practical invention, and the expansion of industry which these have brought about . . . . An unprecedentedly large army of inquirers has simultaneously pushed the interrogation of nature in a thousand directions, and has attained unprecedented results. But beside them has been working an army larger, and equally keen, of men eagerly seeking to utilize for practical ends every crumb of available information.' (46)

What Arnold did not see, however, was that science meant to hold on to its prerogative as autonomous and separate from culture, even as it dined at the same table.

It was almost eighty years later that C.P. Snow took up the issue from the scientist's perspective, coining the phrase, "Two Cultures." With a century of

extraordinary scientific accomplishment separating him and Arnold, Snow is convinced that science and letters are two separate cultures that never understand one another and never fully intersect. It is, in Snow's view, because letters do not see what is abundantly clear to science:

> They are impoverished too--perhaps more seriously, because they are vainer about it. They still like to pretend that the traditional culture is the whole of 'culture', as though the natural order didn't exist. As though the exploration of the natural order was of no interest either in its own value or its consequences. As though the scientific edifice of the physical world was not, in its intellectual depth, complexity and articulation, the most beautiful and wonderful collective work of the mind of man. . . .
>
> As with the tone-deaf, they don't know what they miss. . . .
>
> . . . the great edifice of modern physics goes up, and the majority of the cleverest people in the western world have about as much insight into it as their neolithic ancestors would have had. (14-15)

The twain, perhaps, shall meet, on neutral ground, but Snow is not hopeful about the existence of common ground.

We can see easily in Snow the legacy of logical positivism. Inherent in the Two Cultures conversation is an assumption, fostered in scientific circles and acquiesced to by the interpretative disciplines, that there are, indeed, two entirely different cultures operating here, two different sets of tribal practices, and two different genealogical fathers. By the time of Snow's lectures, he simply is reflecting assumptions commonly shared by both science and letters, that the fact-value split represents a real, significant separation of science and culture: facts belong to the one, values to the other. Given the intensity of Snow's comments on letters, and contrary to his protestations and the evidence of science's deep involvement with culture, it seems arguable that by the mid-20th century, science wanted to reject Arnold's quest for common ground and had moved to an insistence on difference, based on the fact that there is no common ground to share.

In the same moment, however, that science has been attempting to fend off cultural encroachments on its authority and claims to objectivity, its mutual seduction with culture has continued, undermining its

feigned rejection of culture's blandishments. In the late 1880's, it was perhaps understandable that the scientific community could not foresee the extent to which science immediately, and necessarily, would become dependent upon industry's economic largesse to support its research efforts (Coleman 47). Given that myopia, it is certainly asking too much that the same community could have foreseen the extent to which it eventually also would become involved inextricably with government support of its efforts. (Robert Kanigel's account of public funding for the National Institute of Health alone, for example, makes clear how incredibly involved science has become with government [148-151].)

1.6 Practical Effects of the Fact-Value Split

That science remains uneasy with entanglements outside its own discipline, but that it nevertheless finds them necessary, is attested to amply in the contemporary academy. In the technical writing field for example, the issue of apparent incommensurability between science and the humanities has created a suspicion about writing theory that has significant implications for both science and letters. Moreover, the way in which the issue has been treated by technical writing as a discipline reveals how

unprepared science yet is to confront its own external involvements.

Technical writing as a discipline is frequently located within traditional English departments. As a result, it often has found it necessary to distance itself from the traditional domains of the broader profession in order to reassure its consumers that technical writing students are not being contaminated by belles lettres. Carolyn Miller's 1979 article, "A Humanistic Rationale for Technical Writing," in which she simply argues for the existence and acknowledgement of common ground between science and letters, created a debate within the pages of College English that went on for nearly a year.

Given the alleged heresies of much of current poststructuralist critical theory, Miller's claims ten years later seem quite modest. She simply questions whether it is possible for technical/scientific writing to claim its proper place in the academy as long as it addresses itself only to its (scientific/technical) practice aspect and fails to account for its theoretical grounding in the larger network of language itself. Her challenge, however, is a direct assault upon science's assertion that it has no such involvement to explain. She contends that science cannot continue to mistake its dependency upon culture as an

accident or contingency, arguing instead that it is a member of culture (its protestations not withstanding). As such, science has a responsiblity to respond to culture's other members. Her premise is that science cannot have it both ways: it cannot claim autonomy from the larger culture and yet inextricably involve itself in an unmistakable pattern of social intercourse.

Miller's article provoked heated responses. Rather than continuing the Two Cultures conversation, however, respondents appeared to caution Miller, and like-minded others, to lower their voices lest they be heard in the halls of science. In those quarters of the field where practice is the raison d'etre for technical writing, the contention was that broader "literary" concerns are ruinous, scaring off the very consumer that made the trade in the first place: science. Such concerns have been sufficiently persuasive so as to continue to dominate technical writing's discourse. A cursory review of the Proceedings of the Society for Technical Communication's conferences during the 1980's, the Society's journal, Technical Communication, and the Journal of Technical Writing and Communication make it clear that Miller's concerns were clearly not then, nor have they come to be, in the mainstream of the profession's interests which continues to focus almost wholly on practice.

In the end, then, that very domain of the English department that seemed the most likely place to engage in conversation about the deep, inevitably rhetorical character of scientific discourse's relationship to culture ultimately elected to allow the debate to take place elsewhere. The debate goes on in the journals of rhetoric and language, without little indication that technical writing itself is aware of its progress. Such a choice cannot be interpreted, however, simply as some failure of academic will on the part of the technical writing profession. It more properly may be seen as an expression of the profound ambiguity about its relationship to culture and language that besets science as a profession, as well as a discourse. The failure of science's chief cultural exponents, the specialists who write about it for the widest possible array of audiences, to take up the issue widely is a strong indicator that science itself is unready to do so. Theoretical physicist John Ziman's 1968 description of scientists' attitudes and training may clarify why:

> The average scientist will say that he knows from experience and common sense what he is doing, and so long as he is not striking too deeply into the foundations of knowledge he is content to leave the highly technical discussion of the nature of Science to those

> self-appointed authorities the Philosophers of Science. A rough and ready conventional wisdom will see him through. (6)

The apprenticeship system of training scientists, according to Ziman, teaches young scientists to rely on implicit assumptions about shared values. Mentors may ask proteges all sorts of questions about procedural matters but not about deep assumptions:

> His examiners may ask him why he had made some particular assertion in the course of his argument, or they may enquire as to the reliability of some particular measurement. They may even ask him to assess the value of the 'contribution' he has made to the subject as a whole. But they will not ask him to give any opinion as to whether Physics is ultimately <u>true</u>, or whether he is justified now in believing in an external world, or in what sense a theory is verified by the observation of favourable instances. The examiners will assume that the candidate shares with them the common language. . . . (17)

Science's "neglect of--even scorn for--the Philosophy of Science" in its training of young scientists and in the attitudes they eventually may develop strikes Ziman as "strange" (6). His description, however, reveals a

profoundly inarticulate disciplinary resistance to examination of science's own norms and values, even though such examination might save itself from the detriment of proceeding according to hypotheses it has failed to test adequately.

1.7 Shifting the Scene of Conversation

Whether or not science is prepared to go under the microscope itself, in the last twenty years the conversation about scientific discourse has assumed new prrportions and a new locale. No longer just the province, or interest, of a few scientists and philosophers of science, the scope of the discussion has been enlarged to include the pages of journals and books devoted to critical and literary theory, and to the history of science, as well as numerous other venues. (Technical writing's absence is noticeable.) The advent of Kuhn's theories about science's history has effectively redefined the issues and the limits of the discussion in such a way as to invite nonscientists routinely to join the conversation.

As a result of such diversity, there has been a growing sense that the Two Cultures debate is an artifical construct that specifically obscures the ways in which science and letters are indeed culturally implicated, not however as separate cultures, each of

which somehow has access to the truth in some particularly valid way. Instead, the growing consensus is that a full exploration of scientific rhetoric and discourse will reveal science to be embedded inextricably in culture and therefore bearing responsibility to culture. This is not an argument for reduction of science to rhetoric, but it is an argument for acceptance of accountability for its rhetorical relationship to culture.

Few serious participants in the current conversation about scientific discourse want to argue that science can no longer claim an autonomy for itself that distinguishes it from other disciplines. What is at stake, rather, is a growing conviction about the necessity for science to acknowledge the extent to which it uses its authority as autonomous knowledge to make value judgments. As other disciplines identify the extent to which their interests are parallel, and even identical, to those of science, the concern grows that science has created significant problems for itself and for culture by attempting to use its autonomy as a covering rule for practices that seem to lie further outside its autonomous limits than it will admit.

The noisy, and at times violent, intersection of the claims of animal rights activists with those of scientists using animals in experimental laboratories

exemplifies the problem. Each side in the debate resists the other's position, but science's denial that culture can have any stake or any right to an interest in the issue prevents the possibility of genuine dialogue. To some extent, this issue is a touchstone for science's refusal to acknowledge the relationship between its work and cultural self-interest.

1.8 The Consequences of Success

If culture's attitude towards science sometimes appears schizophrenic, no less can be said at times about science's attitude towards culture. On the one hand, science has a necessary and self-evident value in culture. On the other, it resists debating exactly what that value is, distancing itself from the pressures of culture, such as those presented by antivivisectionists for example, at first opportunity. Further, science's continued successful insistence on and fostering of a specific public conception of itself has established two significant problems. Both originate in the same disciplinary and cultural misconception that science is as infallible and pristinely value-neutral as its discourse presents it to be.

The problem that science has created for itself is the more subtle of the two, which is an invitation to scientific error and a concomitant necessity to explain

such error as aberrant and unpredictable. Science has created conditions in which it is not only possible, but probable, that scientists will make serious errors because they unconsciously allow personal values and prejudices to guide their professional decision-making and problem-solving. Moreover, science has made it possible for scientists to admit willingly that such errors have occurred in the distant past but then to deny the probability of repetition. Steve Blinkhorn, for example, describes as "admirably disturbing" Stephen Jay Gould's account of the "rush to press IQ tests into the service of social engineering . . . in the first half of this century" (506). He then proceeds to defend the field as having progressed well beyond such abuses and to excoriate Gould for focusing on those problems to the exclusion of current advances. As is typical with such claims, Blinkhorn confuses science's technical improvements with improved control over human agency.

The second problem that science has created is more obvious: the invitation to an easy cultural appropriation of its products and subsequently uncontrolled technological development, leading to significant policy problems for the culture. In the 20th century, as science has asserted its control over its relationship with culture, culture has picked

science's pocket. Culture has liberated from science the technology for nuclear weaponry, Star Wars, bacteriological warfare, teflon-coated flakjacket-piercing bullets, test-tube babies, maternal surrogacy, clones, CFC's, leaded gasoline--all the necessities for a brave new world.

Science's response to the problems it has helped create generally has been defensive. Rorty, Miller, and others make clear that the positivist project has failed elsewhere. Science, however, has been unable to let go, selectively forgetting its discourse's history, speaking as if the modern incarnation of its discourse is the way it has always been and as if science stands now, and always has, outside of culture. It continues to argue that it cannot be held responsible for culture's irresponsible misappropriation, misuse, and mismanagement of its products.

Those preferences leave science in the uncomfortable position of appearing to make slogans--"Guns (bombs, bacteria, lasers) don't kill: (lay)people do!"--rather than taking seriously the risks that inhere in certain lines of research and development and in encouraging its practitioners to act as if they have special immunity from the pressures of culture. A major premise of this thesis, however, is that science has other options besides blindly holding onto its no

longer defensible claims to infallibility and value-neutrality. We may discover some of those options in a new discourse of science emerging within its own boundaries.

CHAPTER 2

STEPHEN JAY GOULD'S CRITIQUE OF SCIENTIFIC AUTHORITY

During the last two decades, science has begun to lose its immunity to questions of cultural values and ideology. As science's impact on society and culture has filtered into public consciousness, concerns about its proper role have become the focus of a number of debates about such policy issues as health, the military, education, and the environment. The entrance of Stephen Jay Gould into the arena of scientific discourse has further altered the scene of critique of scientific authority to include an internal as well as an external voice. In his monthly essays in Natural History and in other scholarly scientific publications, Gould has addressed the relationship of science to historical and cultural contexts. In the process, he has revealed some judgments of science to be at best suspicious and at worst tainted by cultural bias, thereby questioning the extent to which science can continue to claim successfully an overarching infallibility and value-neutrality. He has continued, however, to affirm explicitly the right of science to make significant claims in respect to its explanations of the natural world and the real. As perhaps the most

important geologist/biologist of the century, as holder of Harvard's prestigious Alexander Agassiz professorial post, and as a recipient of a MacArthur Prize Fellowship for genius (JCP 253), Gould can be neither easily ignored nor casually dismissed. Science, however, has demonstrated itself singularly unprepared to respond adequately to the kind of critique of the profession that he offers.

2.1 Gould's General Claims

Gould has made two types of arguments that have placed him at the center of a controversy in which his critics have questioned severely his expertise, personal motives and politics. First, he has questioned the influence of culture on science and the way that certain social and ideological assumptions have caused important scientific error. His attempts to describe and characterize this problem are present in his earliest publications, starting with his most influential early piece of scholarship, a 1972 paper that he co-authored with Niles Eldredge in which they challenged the prevailing evolutionary theory of gradualism as a product of cultural, not scientific, values. The essence of the claim is that science allowed itself to be fooled by culture: Darwin's preference for gradualism as a model of social change

led him to prefer gradualism as a model of evolutionary change. Gould's subsequent tireless championing of their theory of punctuated equilibrium as a more plausible reading of the evidence of the geologic record has fueled an ongoing debate within the profession about the Darwinian view of natural selection and about Gould's role in that debate (Lowenstein 73).

Second, Gould has questioned the way culture's misappropriation of the products of science distorts those products by imposing nonscientific values on them. His concerns arise from a broader, and perhaps even more rigorous, critique of various scientific projects that he characterizes as so inherently flawed that their persistence can only be explained as unconscious cultural bias on the part of the investigators. Although he has written about a variety of specific projects in this connection, Gould's most unrelenting criticisms have focused on investigations of human intelligence, in which he claims that flawed scientific products are appropriated for cultural uses that are themselves as flawed as the judgements that produced them in the first place. In this case, culture has allowed itself to be misled by science, extrapolating a host of implied values and social policies from certain "facts" when such implications do not inhere at all.

Gould's interest in the science-culture connection

is not desultory. By the time of a mid-1980's interview with Frank Lowenstein, fully a third of the essays Gould was writing centered on "'the way scientists record the biases of their times'" (73). His last two books, The Mismeasure of Man and Time's Arrow, Time's Cycle are in-depth investigations of the phenomena of cultural influence on scientific enterprise. Gould's ongoing commitment to exposing the scientific errors that result from cultural influences is founded on a profound conviction that science is embedded in culture and that its practitioners cannot remain immune from its influences, although they must try to do so. While some of his critics argue that such exposure destroys the purity of science, Gould's project seems to be located instead in an Enlightenment desire to protect science's integrity by preventing error from taking root as fact.

Superficially, Darwinian gradualist views and studies of intelligence testing appear to be radically different, and in an empirical sense, they are. The first more obviously falls within the domain of geology, while the latter is more properly a function of biology/psychology. Gould argues, however, that the one feature they share in common, albeit a subtle one, unites them in the nexus of the same error. In his view, gradualism and intelligence are both contingent

products of cultural influence on scientific practice, not intrinsic products of the natural world or the real. A review of key Gould arguments on punctuated equilibrium and on human intelligence demonstrates how and why he sees a connection between cultural and scientific issues.

2.2 Punctuated Equilibrium

Prior to the articulation of punctuated equilibrium, commonly abbreviated as "punk eke" (Tierney 38), most evolutionists accepted that the geologic and paleontologic records were simply incomplete. If they could find no evidence of "missing links" between ancestors and their descendants or between whole populations and the sudden appearance of changes within species, it was because the evidence had been lost, somehow, in time. Moreover, they had generally accepted Darwin's adoption of Charles Lyell's geological theory as an integral part of evolutionary theory. That is, like Lyell, Darwin believed that the pace of change in nature is so slow and orderly that we may never see it.

Fundamentally, Gould and Eldredge proposed a new paradigm of rates of evolutionary change in their original article. Gould continues to elaborate on that theme by pointing to the deepest problems of his profession and the extent to which the current paradigm,

Darwin's version of gradualism, has failed to solve any of those problems. Instead, he offers a solution that accounts for the major difficulties. Whereas Darwinism insists on gradual evolutionary change, Gould argues for rapid (in geologic terms) rates of change.

Gould's proof consists of two main strategies. First, by arguing that the fossil record is complete, he both offers a different (and, he argues, a more plausible) interpretation of the sudden Cambrian explosion of life and opens the door to a solution of why the fossil record is inconsistent with gradualism. Second, by describing a neo-Darwinian version of the mechanism by which evolution actually most often occurs, he rids Darwinism of excess baggage (an insistence on gradualism) while keeping the critical elements of the theory intact.

A substantial portion of Gould's career has been devoted to demonstrating his claim that the geologic and paleontologic record are indeed adequate to an understanding of evolution. He suggests that what has been inadequate to that understanding are the cultural biases that we bring to the reading of the record. Gould maintains that his profession's history for the last hundred years has been one of looking at the fossil record and baldly denying that what it is

looking at could be a true reflection of nature's processes. As he says:

> We fancy ourselves as the only true students of life's history, yet to preserve our favored account of evolution by natural selection we view our data as so bad that we almost never see the very process we profess to study. (PT 181-182)

In this view, scientists have persisted in reading the fossil record as they think it should be, not as it is. Western culture, he contends, not geologic evidence, has shaped the scientific view of evolutionary rates of change:

> For although rapid bursts of evolution and massive waves of extinction are not inconsistent with Darwinian theory, a deeply rooted bias of Western thought predisposes us to look for continuity and gradual change: natura non facit saltum ("nature does not make leaps"), as the older naturalists proclaimed. (ED 120)

Hence, the first evidence that Gould sees for cultural influence on science is located within his own discipline.

His second collection of essays in toto appear to be structured consciously to reveal the extent to which

Gould has committed himself to the geologic record as adequate. In the "Prologue" to The Panda's Thumb, his preoccupation with amending Darwin's theories to separate the cultural bias from the scientifically demonstrable is clear. He describes the book as a sandwich, "impaled . . . with toothpicks" intended to prick "conventional comfort," such as, "why science must be embedded in culture, why Darwinism cannot be squared with hopes for intrinsic harmony or progress in nature" (13). Most of the collection is devoted to further explication of his own evolutionary theories, which in all other respects square with Darwin, irrespective of the differences about gradualism.

In "The Episodic Nature of Evolutionary Change," Gould makes a compelling case for the accuracy of the geologic record and for the distortions to that record by cultural bias. He reminds us that Thomas Huxley, from the beginning, admonished Darwin to rethink his insistence on gradualism as integral to evolutionary theory. He concludes:

> The modern theory of evolution does not require gradual change. In fact, the operation of Darwinian processes should yield exactly what we see in the fossil record. It is gradualism that we must reject, not Darwinism. (PT 182)

Darwin apparently included gradualism as a part of his theory only because he himself was unable to see how it could be otherwise, not because it was necessary. That apparently only Huxley foresaw it as a potential problem is an indicator of the extent to which gradualism reigned as the prevailing paradigm.

In addition to real professional resistance, Gould's punk eke claims raised some extraordinary theoretical possibilities that had to be confronted in order to make plausible a jerky, rather than an evolutionary, progress. On the one hand, it solves the difficulty of interpretation created for biologists by the great Cambrian explosion when "most of the major phyla of invertebrate animals made their appearance within the short span of a few million years" (ED 120). This had been a deep problem in evolutionary theory, because there was no geologic evidence in the Precambrian period to account for the new forms. The early gradualist versions of this phenomenon were based on the assumption that the record was a "false appearance," and that there must be missing evolutionary steps in the Precambrian that the geologic record simply failed to catch. Recent, new evidence from the Precambrian had revealed no such "missing links," leaving paleontologists with a vast array of still inexplicable Cambrian sudden appearances.

On the other hand, punk eke also required an explanation for the explosion itself. Gould recognized that simply challenging traditional explanation as biased by cultural preference was inadequate: he had to offer another, more empirically plausible explanation in lieu of the accepted paradigm. He found such an explanation in Steven M. Stanley's proposal that the Cambrian explosion can be explained ecologically by the principle of cropping (ED 121-124). That is, the introduction of a "cropper" into the ecosystem resulted in species explosion in every direction. Stanley's reasoning, supported by a number of field studies, is that croppers do not feed on other species to extinction, which would result in starving themselves into extinction. Instead, their cropping makes ecological room for other species to emerge in abundance.

Additionally, Gould's insistence on reading the record as it is opened the door to explanation of two other important features of the geologic record that are inconsistent with gradualism: stasis and sudden appearance. Stasis describes a common phenomenon in which the record preserves most species without "directional change during their tenure on earth" (PT 182). That is, there is no apparent change, however slow, recorded in geologic time. Sudden appearance

describes another relatively common feature of the record wherein a species arrives without warning and "fully formed" (PT 182). There is no fossil record to warn of the impending emergence of the species.

Those two observable contradictions to gradualism in the fossil record lead quite directly to another important strategy in Gould's work, a recasting of Darwinian argument. He describes the two modes by which evolution proceeds: phyletic transformation and speciation. Darwin argued almost exclusively from phyletic transformation, the mode in which "an entire population changes from one state to another" (PT 182). Gould's contention is that this form of evolution contains no mechanism for diversity, and thereby virtually guarantees eventual extinction of the species. If this were the dominant mode of evolution, he contends, it is safe to assume that homo sapiens would not have survived to the 20th century.

In Gould's view, speciation is more important to evolutionary change. That is, "speciation, replenishes the earth. New species branch off from a persisting parental stock" (PT 182). He argues that Darwin's insistence on phyletic transformation was directly connected to his insistence on gradualism. The result is that stasis and sudden appearance in the fossil record must inevitably and erroneously, in Gould's

view, lead to the conclusion that the record is inadequate:

> . . . for if new species arise by the transformation of entire ancestral populations, and if we almost never see the transformation (because species are essentially static through their range), then our record must be hopelessly incomplete. (PT 182-183)

Phyletic transformation, then, makes obvious the circularity of gradualism: the fossil record looks the way it does because of gradualism, and gradualism must be true because of the fossil record. Gould argues instead for speciation as the primary mode of "almost all evolutionary change" and in some detail explains why, as a result, we can expect that "sudden appearance and stasis shall dominate the fossil record." He concludes: "Thus the fossil record is a faithful rendering of what evolutionary theory predicts, not a pitiful vestige of a once bountiful tale" (PT 183-184).

In his last book, Time's Arrow, Time's Cycle: Myth and Metaphor in the Discovery of Geological Time, Gould discusses at length Charles Lyell and his influence on current understanding of the geologic record. In the final analysis, Gould believes Lyell "doth bestride my

world of work like a colossus," but rejects one of his most important theories:

> Thus, when Eldredge and I developed the theory of punctuated equilibrium, we tried, above all, to counteract both Lyell's bias of gradualism and his method of probing behind appearance to defend the uniformity of rate against evidence read literally--for punctuated equilibrium, as its essential statement, accepts the literal record of geologically abrupt appearance and subsequent stasis as a reality for most species, not an expression of true gradualism filtered through an imperfect fossil record. (179)

By the time this book is published, Gould has challenged some of the most important claims of his own profession and offered a plausible alternative.

It should be noted that one unpredictable outcome of Gould's insistence on a careful reading of the record has resulted in the rehabilitation, at least in part, of the reputation of some scientists who were excommunicated by the scientific community because they had challenged the prevailing paradigms of evolutionary theory and gradualism. His articles on Richard Goldschmidt's "hopeful monster" and J Harlen Bretz's theories about the channeled scablands have not only

supported further Gould's views about the dangers of unchallenged dogma in science but have resulted in a positive reevaluation of the contributions of these individuals (PT 186-203).

Gould explicitly believes that science investigates that which is knowable and that which is real. Otherwise, he sees no point in participating in the endeavor (TI). He is not, however, prepared to concede that science is without its own limitations or that scientists can operate within its boundaries without an acute awareness that the membrane which marks science's disciplinary borders is constantly permeated by cultural influences. His inquiry into the investigation of human intelligence makes clear just how strongly Gould is committed to that position.

2.3 Human Intelligence

Gould's writings about evolution proceed directly out of his disciplinary subject matter. His discovery of the way in which even giants of the profession may be misled by their cultural biases into erroneous explanations of data is a product of his interests in geology and paleontology. In contrast, his writings about human intelligence seem to proceed specifically out of his observations that cultural influences actually produce certain lines of research, as well as

the social appropriation of science for culture's own nonscientific agenda. He avoids the dilemma of an ad hominem critique of IQ testing as racist by carefully detailing the ways in which the science itself is questionable.

Among the earliest of the essays on the subject of human intelligence testing and research, "Racist Arguments and IQ" most clearly establishes a set of specific issues that Gould repeatedly returns to time and again in his work (ED 243-247). In a point-by-point analysis, he refutes Arthur Jensen's famous 1969 article in the Harvard Educational Review, "How Much Can We Boost IQ and Scholastic Achievement?". More important than his refutation of Jensen's individual arguments that whites' IQ is superior to that of blacks' is Gould's assessment of the entire question of IQ:

> I do not claim that intelligence, however defined, has no genetic basis--I regard it as trivially true, uninteresting, and unimportant that it does. . . . a specific claim purporting to demonstrate a mean genetic deficiency in the intelligence of American blacks rests upon no new facts whatever and can cite no valid data in its support. It is just as likely that blacks

> have a genetic advantage over whites. And, either way, it doesn't matter a damn. An individual can't be judged by his group mean. (ED 246-247)

Despite his contention that IQ "doesn't matter a damn," he does, however, within the same discussion note that

> . . . it is not a matter for clever cocktail party comments about the human animal. It is a general notion with important philosophical implications and major political consequences. (ED 247)

It is probable that this conviction is what has led Gould to continue to address the problems associated with research into human intelligence in a variety of ways.

Subsequent to the essay on Jensen's position, Gould continued to write about IQ on other occasions, continuing to focus most often and most particularly on the policy implications of the issue. In The Panda's Thumb, for example, he reports Paul Broca's and the Anthropological Society of Paris's scientific imbroglio over the size of George Cuvier's brain, its implications for theories of IQ and individual worth, and the resulting application of those theories to women, to their detriment (145-159). He also records Elizabeth Agassiz's censorship of her husband Louis Agassiz's

letters to conceal the extent of his prejudice against blacks and Indians (169-176).

In Hen's Teeth and Horse's Toes, Gould further recounts attempts to apply theories of human intelligence to public policy, some of which were devastatingly successful. As a result of H. H. Goddard's application of his theories of intelligence to immigrants (he invented the term "moron"), 83% of the Jews tested were classified as feeble-minded (291-302). Gould also unveils the story of how the 1840 census was used to argue that slavery helped prevent insanity among blacks (303-309). In The Flamingo's Smile, he continues this saga, returning to Jensen's faulty arguments in an essay about policies of the Singapore government that treated better-educated citizens as more intelligent (319-322). Finally, in another essay in that volume, Gould further explores the use of IQ scores to defend policies of forced sterilization among the feeble-minded (306-318).

At the time that he was writing some of those essays (almost exclusively for Natural History), Gould also wrote book reviews in which he addressed the same issue in response to others' writings (US). Ultimately, however, his interest in the entire subject of IQ and its connection to culture was sufficiently strong that writing about it at shorter lengths no longer offered

sufficient scope to investigate the subject. In 1981, he published The Mismeasure of Man, a book-length treatment of the question of human intelligence and heredity.

What distinguishes Mismeasure from his other essays on the same subject is the breadth of investigation and detail that he brings to bear on the issues. In his essays, Gould has been forced by sheer length limitations into a piecemeal inquiry into problems associated with the investigation of human intelligence. The focus of each has been a bad piece of science first and that which drives and sustains it second. In Mismeasure, Gould is able to expand his own analysis into a convincing demonstration that the entire project of investigating human intelligence is suspect from the outset. Both the form and the substance of the text confirm that much of the work done in the field of human intelligence has its methodological origins and its rationale in elitist and racist cultural values.

Gould reveals his commitments at the outset. He begins the "Acknowledgments" by thanking

> Ashley Montague, not only for his specific suggestions, but also for leading the fight against scientific racism for so many years

without becoming cynical about human possibilities. (15)

A review of the "Contents" of Mismeasure indicates Gould's intent to treat all aspects of the history of the study of human intelligence. He discusses the treatment of blacks and Indians as "separate, inferior species," the measurement of heads and bodies in failed attempts to prove the superiority of one group over another, the problematic nature of American hereditarian theories, and the errors of Cyril Burt. The last chapter's title, "A Positive Conclusion," is not meant to suggest that the errors revealed in the previous chapters have been done away with or that future investigations of intelligence will offer any more positive outcome than those of the past. Instead, Gould articulates what he believes biology can teach us about ourselves, as an antidote to what we wish it would tell us about ourselves.

2.4 Gould's Agenda

To those who would argue that Gould is pursuing merely his own cultural agenda, he would surely accede but go on to say that there is nothing "mere" about it and that he has been as clear as he can be about the precise nature of that agenda. In a BBC interview published in 1988, Gould candidly acknowledges the

existence of personal biases in his own scientific interests:

> I'd like to be honest enough to admit that everything I'm doing is filtered through my psychological presuppositions, my cultural vices, and I think that honesty is very important because you have to subject yourself to continuous scrutiny. If you really believe that you're just seeing the facts of nature in the raw you'll never be aware of the biasing factors in your own psyche and in your prevailing culture. (Wolpert 147)

That he takes this commitment to be more than mere rhetoric is evidenced by the frequency with which he reveals his prejudices and motives in print. At various times, he has admitted to "a personal, stubborn consistency of viewpoint" (US 11), an "allegiance to cultural relativism" (FS 327), and "my own, strongly biased opinion" (PT 193).

Moreover, we need read only the first lines of the first paragraph of Mismeasure's introduction to see the specific motive driving that project, a challenge to the legitimacy of society's claims that meritocracy is a tool of cultural production:

> Citizens of The Republic, Socrates advised, should be educated and assigned by merit to three classes: rulers, auxiliaries, and craftsmen. A stable society demands that these ranks be honored and that citizens accept the status conferred upon them. But how can this acquiescence be secured? Socrates, unable to devise a logical argument, fabricates a myth. (19)

As far as Gould can see, this myth has never been proven to be anything more than a convenience for some individuals' purposes.

Gould more specifically outlines his goals for Mismeasure this way:

> This book is about the scientific version of Plato's tale . . . . called biological determinism . . . . This book discusses, in historical perspective, a principal theme within biological determinism: the claim that worth can be assigned to individuals and groups by measuring intelligence as a single quantity. (20)

He goes on to say that since Plato's time, men have tried to argue for some version of relative "worthiness," and that

> the basic argument has not changed: that social and economic roles accurately reflect the innate construction of people. One aspect of the intellectual strategy has altered, however. Socrates knew that he was telling a lie. (20)

Gould's intent, first, is to demonstrate that this argument continues into the present through a strategy of examining each of the specific lines of research into IQ. As a result, he makes a strong case for the fundamentally flawed nature of the entire proposition of studying human intelligence through quantification and the establishment of biased or arbitrary categories of human behavior, such as homosexuality and phobia (329).

Gould's additional intent, however, is to demonstrate that those lines of research are driven by biases obvious neither to their holders nor to their observers. He is interested in the problems that emerge in those instances where science has lied but has not known that it was doing so. In the BBC interview, Gould concedes to the interviewer's suggestion that "there is quite a lot of finagling in science" with this explanation:

> Science is done by human beings who are after status, wealth and power, like everybody

> else. That's why I advocate self scrutiny. I say, if you don't scrutinize yourself carefully, and you really think that you are just objectively depicting the world, then you're self-deluding. The capacity for self-delusion is amazing. To me, conscious fraud is not very interesting. Oh, it's human interest, it's fascinating, but the point is, Cyril Burt knew what he was doing. His psychological make-up may have been skewed in a fascinating way, but he knew what he was doing when he invented those twins, and therefore, to me, it's not conceptually very interesting. What's fascinating to me are the people [sic] do not realize what they're doing, and that's where you see the biases of Western thought so clearly portrayed. (147)

The specific example that Gould uses to demonstrate his point is that of Samuel George Morton, who measured skulls in the 19th century, and is the author of "the claim that blacks have the smallest crania, Indians were somewhere in between, and whites had the largest" (147). In recounting the procedural errors Morton made that invalidate his research, Gould is intrigued by Morton's clearly apparent lack of insight into his own motives: "If you're fraudulent, you cover up what

you're doing. He publishes it all and I assume he was simply not aware of it" (148). Had he been aware, he would merely have been a fraud; as it stands, however, he is an icon to an unconscious cultural misprision about human merit.

Finally, it is clear that Mismeasure is meant to fulfill the promise of the position Gould established early in his career in his essay, "Wide Hats and Narrow Minds":

> I am, somehow, less interested in the weight and convolution of Einstein's brain than in the near certainty that people of equal talent have lived and died in cotton fields and sweatshops. (PT 151)

Mismeasure is dedicated to demonstrating that those early suspicions are validated by the very real problems that emerge when science is unable to hold itself to a rigorous self-consciousness about its own motives.

2.5 Gould's Rhetorical Position

Gould does not accept characterizations of himself as a "science writer" (TI), although he does describe himself as "a literary person" (Lowenstein 72). The distinction he seems to be making is that he is a scientist who is attentive to and attracted by the

power of language, not a writer who is attentive to and attracted by the power of science. This clarification turns out to be more than a professional fastidiousness on his part if we read it in the context of his comments on self-delusion and fraud in science. Writing turns out to be a strategy that keeps Gould intellectually honest. Writing enables him to maintain a self-consciousness about his own practices that not only guards himself against scientific error but ultimately opens up scientific practice to cultural critique.

Gould often demonstrates his own self-consciousness, both about his process of writing and about the products of it, by describing writing as a mode of learning. In Diane Dowdey's 1982 interview with Gould about his writing practices, he indicated that he was writing for himself, without a sense of external audience, although he made it clear that then he had thought very little about his writing habits (457, 460). In an interview several years later, he explained furhter that he writes "essays so I can learn things . . . . I learn a lot every time I write. I always choose topics that I don't know a lot about so I can learn stuff" (Lowenstein 72), a position he reiterated in the more recent BBC interview (150).

While Gould's assertions may be read as simple enthusiasm for his subject, they may also be read as a

distrust of unexamined opinions and of his own motives. By continually asserting his own values in writing, he establishes the conditions for their examination by himself. By publishing his writing on a regular basis and for the widest possible audience, he establishes the conditions for a public critique that ensures he will either fundamentally be correct scientifically or be crucified. (Being correct, however, is no guarantee against the latter, as Gould well knows, but in his case, being incorrect is an invitation his detractors could not resist.)

The salient aspect of Gould's writing, however, is its character as a metadiscourse of science in which he calls attention to the rhetorical/political/psychological dimensions of science as a source of two types of difficulty. First, he is a constant reminder of the reality that when those dimensions of science are denied by its practitioners, science ends up committed to positions that do not work ultimately as science. The punk eke controversy points up the way in which science is as vulnerable to anthropomorphic desire as any other discourse. It painfully reveals the way in which we search for explanations that humanize science, so as to make its proportions match our comfort levels and to deny its own implacable nonmoral, i.e., nonhuman, logic.

Second, Gould insists that when culture appropriates science and its products for its own purposes, we end up adopting certain value-laden positions that seem quite clear but that are not in reality. Science claims to be engaged in the project of investigating human intelligence for value-neutral reasons, simply because, as Mt. Everest supposedly, it is there. Now, however, culture's appropriation of the results is meant to serve worthwhile social, government, and educational goals, to enlarge the opportunities available to us as citizens. Yet, Gould's description of those applications makes it clear that while they do often enlarge the opportunities for some, they almost always limit those for others. He insists that we proponents of egalitarianism notice that the project, based on the extraordinary premise that ranking individual worth is worthwhile, promises us science but delivers us into a political meritocracy.

It is necessary to remember that Gould is not interested in science problems motivated by malice or fraud. What his writings direct our attention to is science's and culture's reliance on a discourse that we <u>think</u> is transparent but which is really made opaque by psychological and social commitments. He characterizes the problem as an unselfconscious, and inevitable, transformation of and appropriation of discourse for

purposes which we do not acknowledge. By pointing directly to the rhetorical aspects of scientific discourse, that is, to the nexus of cultural contexts that make and authorize the discourse, Gould introduces into its sphere questions about its value-orientation, and its ideological, political, and social commitments. Those are, of course, the very questions that the discipline has struggled to suppress, and Gould's exposure of their inherence in its discourse invokes a crisis for science.

Similarly, and ironically, however, the crisis he invokes opens the door for his own rhetoric to be questioned. Gould's conviction that writing and individual learning are connected apparently separates him considerably from some of his scientist colleagues. Whereas Gould sees in writing an opportunity to explore the implications of difference, the necessity for candor about personal contexts, and the inevitability of opacity, others see only a threat to science. His admission of ego provokes only more criticism from his detractors. They can see no way in which his admission can make him, paradoxically, seem less egotistical than those who refuse to acknowledge their own desires. Whereas for Gould, rhetorical consciousness (a term he himself does not use, equating it with "mere rhetoric")

can save science, it is also the mechanism by which some scientists indict him, as we shall see.

CHAPTER 3

THE PUBLIC CRITIQUE OF STEPHEN JAY GOULD

If nothing else, Stephen Jay Gould is a lightning rod for the deepest concerns of science about its disciplinary boundaries and the ideals to which it aspires. The criticisms of Gould commonly focus on three substantially overlapping themes: he fails to adhere to the neutrality of scientific discourse, he fails to submerge his self-interest appropriately, and he routinely transgresses the boundaries between science and its social context. While some instances of criticism appear to be disproportionately negative towards Gould's entire body of work (and/or appear to have origins in other concerns), examination of even the more extreme instances offers useful insight into the workings of scientific discourse. In fact, in the case of Gould, the more extreme the response to him, the more clearly it limns issues that are so subtle as to be nearly indistinct in less heavyhanded critiques.

It takes two fairly elaborate steps to develop a complete understanding both of the crisis that Gould invokes for scientific discourse and of the extent to which critics' objections model the very problems that Gould argues exist within the field. First, a review of some negative responses to The Mismeasure of Man

outlines the basis both of typical criticism of Gould and of typical problems that his critics create for themselves. Second, a more detailed review of the arguments of two other Gould critics establishes the most extreme and the broadest possible responses to his position. More significantly, perhaps, this review dramatizes the mechanics by which scientists themselves use the authority of science in order to preserve their own value-laden judgments.

3.1 Some Critical Responses

As Gould himself has pointed out, it is demonstrably not the case that the scientific community's reaction to Mismeasure was uniformly negative towards Gould's science (reviews were mixed in both the scientific community and elsewhere) ("MOMC" 41). Among those who objected to it, or parts of it, however, were Candace Pert, Steve Blinkhorn, and Franz Samelson. Their reviews are instructive, both of typical criticisms of Gould and of the ways in which scientists are as susceptible to unintentional exposure of their personal commitments in their writing as is anyone else.

Candace Pert is one of the world's leading biochemists and is one of the subjects of Robert Kanigel's recent Apprentice to Genius: The Making of a

Scientific Dynasty. Kanigel reveals her as brilliant and no stranger to controversy herself. The general impetus of Pert's review is that she finds Gould's rhetorical style unappealing, but she accedes to his judgment on several issues. For example, she says,

> It is a frighteningly short hop from admitting the possibility of innate human differences to imposing value judgments for the benefit of the ruling classes. Gould demonstrates this in the morbidly fascinating opening chapters. . . . (98)

She later adds that "Finally, Gould convincingly, if soporifically, explains the statistical fallacies used to bolster the false notion that IQ tests measure intelligence," concluding that

> No doubt Gould has successfully documented how bad science--pseudoscience--has been used to support racist arguments and justify social policies as grotesque as the sterilization of Southern black females. (100)

What then is the substance of her quarrel with Gould?

Pert rejects what she characterizes as the illogic of Gould's claim that there has been no significant "'biological change in brain size or structure'" in human beings in more than 50,000 years. She argues:

> This viewpoint ignores the findings of novel, refined biochemical methods for studying the brain: Differences in the distributions of drug or neurotransmitter receptors, neuronal surface proteins that mediate electrical activity and behavior, have been well documented in genetically distinct strains of mice that display opposite--agitated or sedate--behavior after receiving the same drug. Differences can even be shown in genetically psychotic dogs. . . . (100)

Given Pert's standing as a biochemist, she probably is completely correct here. It is not at all clear, however, that she does any damage at all to Gould's main argument. She does not address his more important claim that even genetic differences between some human beings are inadequate to justify meritocratic public policy. Instead, she claims:

> . . . if modern neuroscience reveals biochemical differences that account for human variability, we must deal with this important knowledge; attacking the historic abuses of those differences or ignoring differences simply because they <u>could</u> become abuses will not make them go away. (100)

Pert, then, qualifies her conclusions with an important

"if", and at the same time recommends a policy for scientists as if the discovery of differences is only a matter of time.

Pert does not deny Gould's assessment of the history of IQ research. She only denies that talking about abuses will have any effect on differences. Precisely what she advocates, however, is unclear. "If" genetic differences between humans are discovered, "we must deal with this important knowledge," she contends, hedging what she includes in "deal[ing] with." Her conclusion is significant for two reasons. First, it reveals Pert's quarrel with Gould to be about an issue he thinks is irrelevant ("dealing with" differences), rather than with the specific argument he does make (the problem of abuses). Her most specific response to his real issue evades a direct challenge to his claim that <u>if</u> differences do exist, they are "trivial" and are unworthy of scientific interest.

Second, the authority of Pert's conclusion is based apparently on her beliefs about the lack of value in talking about certain disciplinary issues. She relies on an assumption (not unwarranted) that her audience shares her beliefs, thereby relieving herself of the burden of anything more than asserting her bias, although it is clearly not the only alternative. She acknowledges a history of abuses but rejects a

discussion of it, apparently proposing that not talking about the issue will make it disappear, either literally, or simply from her and science's domain of responsibility. Neither outcome, however, is self-evidently true.

Further, Pert assumes another shared bias with her audience, which is that if differences between humans do exist, we necessarily must do something about those differences. It is quite possible that Pert believes that what she is advocating here is only disinterested knowledge. That is, Pert may think that she is proposing, not a cultural policy, but something about what questions scientists should ask themselves next. She writes her claim, however, in such a way that it allows a perfectly legitimate cultural reading as an endorsement of the inherent value of such research. She appears to be unaware that because she does not qualify the limits of science, she creates the grounds for justification of a variety of social policies based on intelligence research.

Moreover, if Pert is only advocating continued research, she shifts from an issue of fact (whether differences exist) to an issue of value (all knowledge is equally valuable) without establishing the validity of such a leap. Gould argues that such leaps are precisely the problem (any differences that do exist

"don't matter a damn"), but Pert writes, with some irritation, as if her position is a foregone conclusion at which Gould himself should have arrived. Rather than evaluating the worthiness of the research itself, she is prepared to act as if that fact is established already and to deny that such an evaluation has merit.

Psychologist Steve Blinkhorn is much more critical of the quality of Gould's science than is Pert. His conclusion is,

> The truth of the matter is that Gould has nothing to say which is both accurate and at issue when it comes to substantive or methodological points. (506)

Although he only points out one specific instance in which he says that Gould has science <u>wrong</u>, the closest that Blinkhorn comes to approval is in this admission:

> As Gould rightly emphasizes, many of the uses to which [IQ] tests were put [in the first half of this century] made mockery of their original purpose. (506)

The bulk of the review is devoted to demonstrating the validity of its first sentence:

> With a glittering prose style and as honestly held a set of prejudices as you could hope to meet in a day's crusading, S. J. Gould pre-sents his attempt at identifying the fatal

> flaw in the theory and measurement of intelligence. . . . (506)

He later argues,

> It is a masterpiece of propaganda, researched in the service of a point of view rather than written from a fund of knowledge. . . . you have all the makings of a lively, plausible, opinionated and zesty potboiler.
>
> But verbal fluency is no substitute for good arguments in the long run. (506)

The substantive thrust of Blinkhorn's criticism, then, is not that there are crippling errors in Gould's science, but that his apparent motives and willful misreading of the evidence are the problem. He offers no evidence, however, that Gould's research was motivated by his presuppositions.

It is Blinkhorn's final sentence that poses a real conundrum: "You have to admire the skill in presentation, but what a waste of talent" (506). Given the severity of his critique, it is completely unclear here what gesture Blinkhorn is making in praising Gould's "talent," or for that matter, what he is referring to. He has already indicted Gould for misreading the scientific evidence and for a propagandistic rhetorical style. He appears to be claiming that if Gould had applied his rhetorical style to the project of

endorsing IQ studies, Blinkhorn would somehow have been able to overlook what here he can only describe as excess. He seems to leave open the possibility that Gould has simply employed his skill in the service of the wrong goal or that he is, after all, not a bad scientist but merely wrongheaded.

As in the case of the Pert review, Blinkhorn reveals his own commitments. First, he never resolves his own ambiguity about the relationship between content and style. He wants to indict Gould for his rhetorical strategies as revelatory of a "set of prejudices" but assumes that his own reader will be untroubled by the extent to which his own strategies similarly reveal his prejudices. When he characterizes Gould's writing, for example, as having "the routine flavour of Radio Moscow news broadcasts . . . " (506), Blinkhorn makes an ad hominem attack on his politics, claiming that they taint Gould's position, without clarifying the objection. Ambiguity also surfaces in his suggestion that fluency and good argument are independent of each other, that they can be substituted for each other, as if they do not penetrate each other.

Second, Blinkhorn appears to see no difficulty for himself or his reader in respect to his own stake in the debate about intelligence testing. He appears to be unaware of the tension that exists between the clear

authority of his role as an expert in the debate and the bias of his role as a psychologist defending a formal area of his discipline. Gould has remarked on the difficulty that occurs when a scientist is asked to review a negative critique of his own discipline (TI), but Blinkhorn appears untroubled by the dilemma. He uses his rightful authority as an expert on the facts to cover a number of claims about matters of value, neglecting to be appropriately suspicious of the extent to which he can remain genuinely disinterested or the extent to which his claims can be read as a simple privileging of his biases over those of Gould's. Blinkhorn's major argument ends up being not that Gould has his science wrong but that he has his values wrong.

Franz Samelson, also a psychologist, has the same problem of speaking from a self-interested position. His review of Mismeasure, however, is considerably more measured than Blinkhorn's. He does find fault with Gould on issues of accuracy and interpretation of data, but he misses Gould's position entirely on ranking, apparently thinking that Gould's objection to it is that it represents a "strange obsession of individuals" (657). As a result, his entire review is an apology for what he assumes to be the obvious necessity of science responding to cultural demand. Therefore he rejects

Gould's position on ranking on the basis of its social necessity:

> Not just individual hangups but technological and political decisions of all kinds demand a (unidimensional) ranking of multifaceted realities, together with a legitimation of the ordering used. (657)

Samelson, as Pert, curiously appears not to realize that this position is precisely what Gould is critiquing and adds quite ingenuously that "ability testing is out there, a sizable industry in the 'real world,' and a smaller one in academia" (657). He shares none of Gould's concerns that it should not be out there at all.

In an even more ingenuous gesture, Samelson turns to a recent issue of American Psychologist to authorize his claim that Gould is out of step with recent developments in IQ testing: ". . . a special issue on testing. . . . proclaims a well-grounded science of human abilities to be alive and well" (657). The same issue proclaims the growth of testing and the connection between testing and employee salaries. He is satisfed that psychologists (all of whom have a stake in this thriving industry) are professionals of good will: "There is much concern with legal issues, affirmative action, special education, and the social

responsibility of testing . . . " (657). He sees no irony that IQ testing is fraught with such legal and ethical risks that it requires extraordinary vigilance and oversight.

Samelson's inability to see a possible connection between his professional commitments and the stakes of the argument that Gould makes is a good model of how blindness to bias arises. In the last paragraph he raises indirectly a question for himself that no other reviewer of Gould's work has:

> In all, there is hardly any direct contact between Gould's arguments and the issues occupying the experts. I am not sure just what to make of this contrast; and this is not the place to speculate. (657)

He is simply unable to see that he and his colleagues are interested in other issues because their commitments dictate that they should be. Further, he is completely ambiguous about where such speculation should take place and by whom. If his position is that scientists should not speculate at all, he has failed completely to address Gould's main concerns, and the entire review is an evasion. If his position is that there is some location where such speculation can be done, he gives us no hint as to where or under what conditions.

What makes Samelson's review unique is that he repeatedly puzzles over what he sees as the peculiarity of Gould's position. Unlike most of Gould's other detractors, he recognizes that Gould's concern is somehow disproportionate to his own benign view of his discipline and suspects that is significant somehow. He is finally unable or unwilling to ask himself if his inability to understand is a function of his own motives.

Pert, Blinkhorn, and Samelson all find themselves in the same dilemma, although only Samelson (however vaguely) appears to have any awareness that a problem even exists. They each reject, either outright or implicitly, the propriety of a disciplinary conversation about either the implications or the relative necessity of the values Gould questions. As a result, they ethically prohibit themselves from speaking about values from inside science because it is inappropriate as a subject of scientific discourse. They practically prohibit themselves from speaking about values from outside science because they are still scientists and/or because they fail to specify a location in which such a dialogue could occur legitimately.

Yet, each scientist wants to criticize Gould most directly on value-laden terms: Pert challenges his choice to attack abuses linked to differences;

Blinkhorn disapproves of his rhetoric, his interpretations, and his politics; and Samelson frowns upon his failure to appreciate social necessity as juggernaut. In order to freely critique Gould as they wish, however, each attempts to demonstrate that their criticisms are located in Gould's scientific errancy, a proper topic. Since none of them, however, demonstrate that Gould's alleged errancy is sufficient to undermine his larger claims, they are unable to avoid quarreling with his values, not the significance of his facts. As a result, they can neither defend nor attack Gould from inside science without stepping outside into a location they have not yet admitted is possible to occupy.

Pert, Blinkhorn, and Samelson are all unable to tap self-consciousness as an adjunct to discourse. As scientists who subscribe publicly to fairly straightforward discourse values in an incredibly, increasingly complex cultural context, they are simply baffled by the requirements of that context. Rather than accede to the necessity of maintaining a self-consciousness about the relationship of their values to their disciplinary commitments, thereby avoiding the dangers of hidden bias, they resist. They are not alone, however, and represent only one possible set of problems resulting from such resistance.

3.2 Two Troublesome Critics

A more detailed review of two other Gould critics reveals the deepest disciplinary fears that Gould's writings tap and models the way in which scientists themselves may use the authority of science to preserve their own value-laden judgments. Probably the most openly acerbic critic of Gould's work is Harvard colleague Bernard Davis. His article-length review of The Mismeasure of Man, "Neo-Lysenkoism, IQ, and the Press," is particularly useful for understanding arguably the most extreme, negative responses to Gould. His review draws heavily on those of Pert, Blinkhorn, and Samelson and in doing so appropriates their values in support of his own. L.B. Slobodkin's review of An Urchin in the Storm, is much narrower in its scope than Davis' and superficially much more congenial, but it also relies more profoundly on linguistic sleight of hand in order to resolve the issues it raises.

Davis and Slobodkin make the problem of language in scientific discourse a significant focus of their criticisms. They argue as if there are only two possible discourse options: an egoless transparent speech or an egotistical opacity. They endorse the former, a rhetoric in which authorial presence allegedly is submerged fully (the legacy of logical positivism), and call Gould to account for having

failed in its goals. They reject his rhetoric as inappropriately self-conscious, identifying and criticizing as it does its own interests. At one level, these charges appear to be relatively innocuous. Davis says, for example, "He personalizes his expository writing in a breezy self-deprecating manner . . . " (41). Later, in reference to the construction of a specific argument, Davis complains, "Unfortunately, the advocacy and the emotional appeal betray the scholar-ship" (45-46). Slobodkin's evaluation of what he characterizes as Gould's "truisms," however, is more detailed:

> The interest is in how they are expressed and discussed. Roughly translating part of the preceding paragraph into Gould's language (of which I claim a modest knowledge), it might read: "Panglossian adaptationists fail to understand holistic materialistic constraints. Punctuated equilibrium belies the simplistic claims of gradualists. Elitism and class structure color the role of IQ tests. . . ." These are essentially similar statements but now have a special flavor. (503)

The apparent innocuousness of these claims belies Davis' and Slobodkin's strategy. They are establishing

the grounds for arguing that Gould regularly transgresses what they perceive as fundamental, inviolable rules of engagement in scientific discourse.

Davis characterizes the requirements of scientific discourse this way:

> The strength of science in analyzing reality comes from its strict separation of facts from values, of observations from expectations. Measurements of intelligence, and of its hereditary and environmental origins, are part of natural science--even though one must go beyond science, bringing in judgments of value, in order to probe the social implications of the results. Hence any purported scientific exposition of these topics must be as dispassionate and objective as possible about the facts, whatever the social views the favors. These are precious standards, whose corruption we must resist. Unfortunately, throughout Gould's book they are not met. (45)

Slobodkin's view is almost identical, although deceptive in its avuncularity, when he says that Gould

> . . . violates certain rules of etiquette or perhaps only of style. One of the most difficult aspects of scientific work is

> reporting what one sees without having it colored by one's own preoccupations. It may be ultimately impossible. Nevertheless, in most scientific prose the author strives for clarity in the dual sense of expository simplicity and in making oneself transparent so that the empirical world is visible through the text but the peculiarities of the author are invisible. Even in popularizations of science or in reviews of science books, most authors strive for this transparency. The uniqueness of Gould is that he dances between us and his subject. (503)

Davis judges Gould's prose as dangerous and corrupting, while Slobodkin dismisses it as merely distracting, even occasionally charming. Both, however, criticize it on the same grounds: while we can agree that it is literally impossible, or nearly so, to be objective in writing about science, it is necessary to preserve the superficial appearance of an attempt to do so because what is at stake is the special nature of scientific discourse itself.

Neither Davis nor Slobodkin, however, is able to account satisfactorily for Gould's own preference for a self-conscious rhetoric that permits critique of scientific discourse as it is currently practiced. Nor

does either speculate about the validity of Gould's critique of the limitations of a one-dimensional discourse of science that claims transparency but which, admittedly, may be opaque instead. They are unable to read Gould's argument as a caution that we need simply go beyond one-dimensional discourse, not completely reject positivism. They may be unable to do so because they do not separate Gould's rhetorical strategy of disciplinary self-criticism from his practice of science. Yet, Gould himself makes this distinction, advocating pluralism in interpretations of data (at least for as long as there is any question that more than one interpretation might apply), while embracing fully the idea of an objective, empirical world.

At one level, then, Davis and Slobodkin object to Gould's diction because they are certain it betrays weak scholarship, a quaint (if useless) literary prolixity, or the personality of its author. At another level, however, they object to Gould's choice of content and method of argument because they all too clearly reveal what Davis and Slobodkin see as base self-interest. Davis begins by claiming that "Gould's own degree of bias is unusual in a work by a scientist" (47). One piece of evidence he points to for support of his contention is that Gould is a member of a "left-wing group called 'Science for the People'." Another is

a paper in which Gould comments on his Marxist background and "has spelled out explicitly his ideological commitment, and also its influence on his science" (56). Davis' assessment of Gould's having openly identified the source of his commitments is this:

> To most scientists (other than those tethered to a party line) such a claim of support from (or for) Hegel is silly, and such an insertion of an ideological preference, whether from the left or the right, is a corruption of science. (57)

Davis' premise appears to be that the existence of these facts about Gould is not the real issue. Rather the problem is that Gould has 'inserted' them consciously in his writing about science, thereby tainting his objectivity. Given the structure of Davis' argument, it appears that Gould could have escaped his censure by simply not revealing overtly his ideological preferences. Gould's response, however, is that the taint occurs in <u>failing</u> to identify clearly the self-interest driving his claims, because it misleads the audience into thinking that scientific discourse is exempt somehow from the influences of self-interest.

Although Davis is entirely correct that Gould's commitments do exist and that they do influence his

objectivity as a scientist, he fails entirely to demonstrate his claim that Gould's "degree of bias is unusual in a work by a scientist." He does, however, make a sound case that Gould's conscious revelation of his bias is unusual, and that appears to be for Davis the real threat in Gould's writing. His most severe criticism is the concluding section of his review, a coup de grace entitled, "Neo-Lysenkoism," in which he claims that in Mismeasure of Man Gould weakens science by undermining its authority and makes "an effort to outlaw a field of science because it conflicts with a [his] political dogma" (58).

Given the story behind the scientist Lysenko's (mis)management of Russia's agricultural genetics program, Davis makes a very severe charge indeed against a biologist. Lysenko suppressed Mendelian genetic research on wheat because it contradicted prevailing Marxist and Hegelian views, thereby setting Russia on the road to agricultural catastrophe. (Russia is still not agriculturally self-sufficient today.) Moreover, the charge is particularly cunning in this case, as Gould himself has authored an article in which he refers to Lysenko's arguments as "falsified data and vicious polemics," "rhetoric and invective," and to Lysenko himself as "a charlatan and a cruel polemi-

cist"(HT 134-135, 141). Davis omits that detail and concludes,

> the crucial lesson to be drawn from the case of Stephen Jay Gould is the danger of propagating political views under the guise of science. . . . when a book speaks with scientific authority on a controversial social issue, the innocent lay reader particularly needs protection from propaganda. (59)

There is no apparent irony in that conclusion, an almost perfect recapitulation of Gould's thesis in Mismeasure. On the one hand Davis claims that ideology has no place in science and on the other, indicts Gould for writing a book that exposes Davis' position as accurate.

The maneuver Davis attempts to make here is an implicit appeal to the authority of science's empiricism as somehow capable of temporarily suspending the power of individual ideology when a scientist writes. He wants to claim finally that it is possible for a scientist to hold values that are meaningful to her but whose influence she can neutralize when she writes about science. For Davis, Gould fails as a scientist because he, unlike most of his colleagues, is unable to stand outside the self-interest of his

politics and speak on behalf of a "pure" version of science.

Slobodkin uses a very different strategy to attack Gould, addressing the question of politics only very indirectly, and focusing instead on what he essentially casts as Gould's careerism. His overall assessment of Urchin in the Storm is that the "reviews are pegs on which Gould hangs his favorite ideas in poster form" (503). As Slobodkin's analysis unfolds, it appears that he sees several of the Urchin reviews as motivated by opportunism, since in his view one of Gould's "favorite ideas," punctuated equilibrium, is prominently featured to the detriment of other early proponents of the same theory:

> . . . Gould's emphatic distinction between "gradualists" and "punctuated equilibriasts" hinges on Gould having invented the word "gradualism" and having suggested that the repeated early assertions that evolution does not occur at a constant rate but involves long periods of stasis were not really seriously intended. (I am among the several dozen people at least that thought we had made this point in a very serious way. . . .) (503)

There is more than a suggestion here that Gould has made his reputation off the work of other, more

seminal, thinkers. There is no concession here that Gould has repeatedly acknowledged the intellectual debts he owes to others.

More damning perhaps, is Slobodkin's specific assertion about the source of Gould's reputation:

> Gould is a scientist who became a celebrity on the basis of charm and style and a strong ego . . . . This book is a fair entree into the phenomenon of Gould, just as essentially any book by Elmore Leonard or Dick Francis will do as a starter into their work. How celebrities fare in the light of history is never obvious, but, in the meanwhile, Gould is leading a much more exciting life than one would anticipate of a snail paleontologist, winking at his supporters and thumbing his nose at his opponents. (504)

In other words, any book by Gould is as good as any other, since they are all the lightweight efforts (dependent as they are on the work of others) of a lightweight intellect (whose chief claim to fame is charm and style). There is no admission here of originality or genuine merit in either Gould's practice of science or in his analysis of the history of science.

The other point on which Davis and Slobodkin take

issue with Gould is that of the social context in which science is practiced. On this topic they appear to hold approximately the same view. Davis summarizes it when he says that "The strength of science . . . comes from its strict separation of fact from values . . . " (45). More specifically, what he wants for science is a role outside the social culture, where it is contaminated by neither pride nor prejudice. As he says, "Science can make a great contribution toward solving our social problems by helping us to base our policies and judgments upon reality, rather than upon wish or conjecture" (59). Again, while this is a virtually complete recapitulation of the position Gould himself takes, Davis sees himself holding the higher ground. His contention is that the mechanism by which his position can be implemented in practice is very different from that which Gould advocates. Science can make its contribution only if its <u>claims</u> to objectivity are scrupulously guarded (Davis has already acknowledged the possibility that actual attainment of objectivity itself is problematic):

> In fact, the key to the success of the scientific enterprise is its passionate dedication to objectivity: Its advance depends on accepting the conclusions dictated by verifiable observations and by logic,

> even when they conflict with common sense or with treasured preconceptions. . . . Its observations and conclusions . . . correspond to reality more objectively and reliably than any other form of knowledge achieved by man. (55)

Only science, then, can judge the merit of its own activity, because only scientists have access to scientific method. Although Davis does not specify for us the mechanics, scientists apparently can discern between "treasured preconceptions" based on simple preferences or biases that must eventually yield to evidence and those that are based on scientific principle and should be held onto at all costs.

By extension, laypersons ("the innocent lay reader") evaluating Gould's work are bound to misunderstand the import of his argument. It is only other scientists who can see the potential havoc to be wreaked by cultural acceptance of his claims for the limits of objectivity in science: "Because this influence is so powerful it is essential for such contributions to be judged critically, by the standards of science" (59). Real criticism of science is to be reserved for real scientists, and it seems clear, to be kept within the scientific community. Where lies Gould's argument and the public critique of scientific

authority lies darkness and the madness of uninformed cultural opinion.

Davis' argument on the question of objectivity, however, is thoroughly undermined by his own use of the reviews of Pert, Blinkhorn, and Samelson in critiquing Gould. Davis' strategy is virtually identical to theirs: he claims that Gould's science is in error, and therefore, severe criticism of Gould's biases is totally warranted. In order to justify a charge of errancy, however, he relies heavily on the arguments of the other three whose positions we have already seen are suspect. For example, Davis includes Pert's review in a group that he describes as "highly critical" of Gould (43), although Pert herself qualifies her criticism of Gould and agrees with his assessments on some crucial points. Since her argument is not a refutation of Gould's position but an assertion of what his position should have been, Pert's position claims for itself considerably less force than does Davis' use of it.

Further, Davis apparently sees no difficulty with Blinkhorn's status as a psychologist (44). The question never arises of whether Blinkhorn's position could be influenced by his professional self-interest, and Davis repeats all of Blinkhorn's charges without caution. He uses Samelson in virtually the same way (43-44). In

each instance in which Davis uses the authority of the other three scientists, he fails to see the possible relationship between the values located in their commitments, both personal and disciplinary, and the assessments they make of Gould's work.

Davis, however, would claim that if his use of Pert, Blinkhorn, and Samelson is in any way prejudicial, it is just one more stunning proof that Gould is completely in error. Davis would reason his own mistake this way: he made an attempt to be as objective as possible, and in doing so, he relied on the writings of three other scientists who ostensibly also were making an effort to be objective. Yet they all still were tripped up by subjectivity. Assuming that Davis would concede the latter point, we can imagine his conclusion as being something like this: "See what can happen when everyone is working at being objective. Imagine the outcome if we just let our biases go!" Gould would, of course, concur, "Yes, just imagine! Had everyone involved in the conversation identified clearly their self-interests at the outset, Davis would never have been tempted to mistake values for facts."

Slobodkin is less interested than Davis in questions of Gould's errancy and so does not open himself so easily to a charge of having confused facts and values. He is more interested in claiming that

Gould has transgressed the border between the domains of science and those of culture. Slobodkin's use of the term "popularization" indicates that he recognizes that some scientific discourse goes on outside of science proper but that such discourses are essentially different than those of science proper. He makes this distinction clear by his exposition about what characterizes scientific work and prose as distinctive. More conclusive, however, is his characterization of the "typical" scientist's private world, as compared to that of the celebrity scientist Gould:

> Most scientists are plodders, relatively immune to boredom, not at all chic. Many have found a haven in the museum or laboratory where their frail, if not modest, egos will be unbruised, and where they may even swagger a bit among their students. Confronted with Gould's style such people become groupies or opponents, live vicariously in his celebrity, or jealously bemoan it. (504)

This is the the practice of science in its most romantic incarnation--the lonely, dull scientist as grind, chained to his bench by commitments to "the truth" and a pursuit of knowledge for its own sake, uninvolved in the petty day to day pursuits of the average employee, for he is not an employee at all, but

something more like an Essene-in-training. In this view of science, it is an endeavor completely outside the mainstream of culture and practiced only by a certain type of individual who can dedicate himself to a particularly narrow pursuit. In Slobodkin's view, Gould is distinctly aberrant, and there is a limit as to how seriously we should take him.

3.3 Summary

The question of Gould's errancy or accuracy in respect to the specific details of scientific fact, as discussed in these reviews, must finally be resolved by those who are qualified to do so--the larger scientific community. What can, and should, be dealt with here are the issues that Gould's critics raise about scientific discourse and whether they succeed or fail. For the moment, we can say that Gould's critics have failed to be persuasive in their claims that his discourse is tainted somehow by personal bias in a way that other examples of scientific discourse clearly are not.

Bernard Davis wants to cling to the positivist version of a science discourse of egoless transparency as the model for all writing about science and to criticize Gould for not living up to that model. In order to do so, however, he has to demonstrate that Gould's science is faulty. If it were not, the claims

about his rhetorical improprieties would be irrelevant, since Davis would have failed to demonstrate the necessary one-to-one relationship between the failure of Gould's science and the failure of the language he uses to describe it. To authorize his position, however, Davis has to call on the criticisms of other scientists, equating their claims about Gould's science with value-neutrality and using the details of their positions without regard for the shape of the arguments those details make. Unfortunately, the critics that he uses are in the same bind as he, all wanting to take issue with Gould, but finding far less to criticize in his scientific practices than in his rhetorical strategies.

Similarly, Slobodkin's critique of Gould's celebrity and transgression of the popular image of scientist, his contention that punk eke is an unoriginal theory, and his claim that Gould fails to submerge his own personality in his writing, put him in the position of making an ad hominem attack on Gould without ever discrediting his science. Further, in the process, he has to expose his own values as driving his critique.

Gould's critics, then, are left in the position of claiming that he provokes an intolerable and unneces-sary crisis for science by repeated public acknowledg-

menting of the vulnerability of scientific discourse to individual desire and cultural pressure. In order to make their claims, however, they end up modeling the very problems that Gould exposes. Davis, given an opportunity to retreat from the more indefensible of his positions in an answer to Gould's response to his review, is unable to extricate himself from the trap of ad hominem argument:

> . . . a number of letters about my article, from members of the National Academy of Sciences in Gould's field, have emphasized that such a sharp criticism of his science and its political bias was long overdue. It is clear that Gould, however effective as a teacher and a popularizer, is not a scientist's scientist. (1984, 154)

So there.

3.4 Gould's Advocates

The positive critique of Gould takes place at a very different level than does the negative critique. Whereas Gould's detractors prefer not to confront directly his claims that an infallible and value-neutral view of science is no longer possible, much less desirable, it is precisely at that level that the positive critique of Gould occurs. His supporters are

those who do not equate the discourse of science with the real and who are suspicious indeed of any claims to a discourse without opacity. They believe that Gould offers the possibility of a new scientific discourse that overcomes the problems of its traditions.

Gould, as it happens, is not without substantial support in the scientific community. Frank Sulloway, a psychologist and another recipient of the McArthur Foundation genius award, in his review of Time's Arrow, Time's Cycle notes that "Gould's monthly column in Natural History . . . has led to a series of highly praised volumes of essays . . . " (37). Further, he appears to be willing to engage the specific arguments Gould makes, not the ones he wishes Gould would make. His assessment of the book under review is this:

> If time's arrow and time's cycle are "great" and "eternal" metaphors for others, they have also given direction to Gould's work and theoretical interests. It hardly seems coincidental that his first published essay (1965) was an article on the confusion of meanings in Lyell's concept of uniformitarianism (a confusion about uniformity as immanent laws--or time's cycle--and uniformity as states--or time's arrow). . . . The more one reviews his writing over the years, the more

> one sees just how central this and another thematic pair of ideas--continuity and discontinuity--are in his thinking. To elucidate the pervasiveness of these metaphors in Gould's writings we must first identify, following Gould's lead, some of the other intellectual guises in which they appear. . . . Gould is one of those rare scientists who fully appreciates the past is not always "just history" and that many problems in science cannot be conceptualized correctly unless one escapes the intellectual straitjacket of prevailing scientific mythologies. (39)

Sulloway does not, however, claim that Gould makes a perfect case or that there is no room for criticism of his work. Instead, what Sulloway makes a specific effort to do, is to respond to Gould in terms of the particular arguments that Gould himself establishes. Whatever quarrels Sulloway may have with specific details, he still responds to the shape of the argument and at the level of the overall thesis, and more often than not, he finds them persuasive.

Similarly, other critics of Gould's writing, particularly of Mismeasure of Man, who find him convincing consistently do so within the context of

responding to his broader arguments. Philip Morrison, for example (in a review in Scientific American that Bernard Davis found objectionable because of SA's editorial policy), calls Mismeasure of Man

> A persuasive chronicle of prejudice in science, founded on scrupulous examination of the record, enlivened by the talents of a gifted writer. . . . (30)

Morrison's review is in such contrast to those of Gould's advocates that we have to read closely in order to see that Morrison has defined the problem differently. The "scrupulous examination" he refers to is Gould's reading of the historical record of specific instances of scientific ignominy. He finds the reading persuasive in its demonstration that science has been used repeatedly to defend reprehensible cultural and personal agendas. Gould's detractors, however, will admit that he has read the record correctly, only to deny that his reading carries force because he may have some detail partially wrong, left out, or incomplete. They generally make no attempt to demonstrate that the detail has sufficient importance to impeach the entire argument. Further, they see no reason to read the future in the past, whereas Morrison and others think that the past is a useful cautionary against the possible repetition of scientific error.

Similarly, Steven Rose's review of Urchin for the British publication, New Science, is favorable for almost exactly the reasons that L. B. Slobodkin took it to task. Rose makes clear at the outset, in a very Gouldian gesture, that he has admired some of Gould's other work:

> I have described him as the greatest populariser of science since J.B.S. Haldane. . . . A new collection of Gould's essays should . . . find a ready welcome, especially on the British side of the Atlantic, where, as a distinguished sociologist was lamenting to me the other day, the essay-form, once the pride of English literature, expired some time in the 1930's. (63)

Rose also has objections to the book under review, spending half his space raising questions about the wisdom of publishing a collection of book reviews. Only then does he finally conclude:

> An Urchin in the Storm works, at least in part, because the reviews form a coherent collection, rehearsing themes which are, by now, familiar to Gould aficionados. The essays expound evolutionary theory and the history of geology; they castigate sociobiology, IQ theory and biological determinism;

> they rescue from the shadows some major historical figures; they defend scientific rationality against creationism on the one hand, and the further reaches of crankiness (Fritjof Capra, Jeremy Rifkin) on the other. (63)

Rose displays a keen awareness of, and respect for, the audience for Gould's work, the values that audience may hold in common with Gould, and the particular rhetorical enterprise Gould is involved in. He also points out (as Sulloway does about Time's Arrow) that "To understand Gould one should read his new book," because:

> his latest volume offers his readers a valuable insight into his wider intellectual vision, providing them with a literary blueprint for a number of the basic concerns that unite his many essays and books. (37)

In other words, Gould's concerns are more coherent and complex than a mere objection to some scientific projects and some scientists, and there is in his concerns something that needs pondering.

Both Rose and Sulloway suggest that what Gould's writings about science actually permit may be much more significant than what Davis and Slobodkin imagine. While one outcome may be popularization of science,

given the accessibility of his writing, the more significant project is the way in which it offers intellectual honesty and candor as an antidote to the traditional, self-serving claims of scientific discourse, providing a viable alternative in a new discourse of science. Rose's and Sulloway's open-arms acceptance of Gould's version of scientific discourse, in spite of their reservations about some details, signals the possibility that a new rhetoric of science may be in the making.

CHAPTER 4

TOWARDS A NEW RHETORIC OF SCIENCE

We are left then with three possible positions science might take in response to challenges to its authority: it can play ostrich and put its head in the sand and simply deny that the problems under discussion can be talked about productively; it can invoke a Nietzschean claim that such challenges are nothing more than a will to power; or it can accept a liberal compromise in which such claims are seen as fair acknowledgement of real problems in the discourse of science.

4.1 Three Possible Positions

The first position, that of the ostrich, is the fallacy of Slobodkin's argument in particular. Science argues that if we do not address power and desire as human motives in the social construction of knowledge, then we can repress them. When Slobodkin describes "certain rules of etiquette" in science discourse, he is referring to the disciplinary expectation that scientists will submerge personality and opinion in their writing, thereby making themselves faceless. The implication is that that strategy will result in value-neutral, or at least, conflict-free discourse. He

contends that "the passion with which [current evolutionists] support or oppose Gould. . . . is odd given that all of this is fairly innocent stuff, and often not wrong. Why the passion?" Slobodkin's use of "fairly innocent" as a descriptor is characteristic of his rhetorical strategy in general. He consistently attempts to defuse the energy of the Gould-inspired debate with a collegial avuncularity meant to return scientific discourse to a more seemly discussion of facts and not passions.

Pert's strategy of focusing attention ostensibly only on facts is even more subtle than Slobodkin's attempt to smooth over conflict. Close examination of her the procedures she advocates for dealing with differences reveals the problems she creates for herself:

> . . . attacking the historic abuses of those [biochemical] differences or ignoring differences simply because they could become abuses will not make them go away (100).

First, while she asserts in a preface to this phrase that "if modern neuroscience reveals biochemical differences . . . we must deal with this important knowledge," the sentence proceeds as if the differences are an established fact of some importance. Further, there are at least three different readings of this

construction, two of which are quite logical contextually. The most likely is that we read "attacking the historic abuses" or "ignoring differences" merely as alternative treatments of differences, in which case Pert is claiming that there are no circumstances under which differences will disappear.

A second option, however, is to read "attacking . . . abuses . . . or ignoring differences" as a compound subject, with "them" referring both to historic abuses and to differences, in which case Pert is claiming that abuses will not disappear because of talk and that differences will not go away because we pretend they are not there. (The third option is to read "them" as referring to abuses only, in which case Pert is claiming that either attacking abuses or ignoring differences will not cause abuses to disappear. This seems unlikely given the logic of the passage.)

The most likely reading is that Pert is arguing a strong case for the the necessity of differences. Because Gould is arguing an issue of value (an interpretation of abuses), however, this reading forces Pert to shift the grounds of the debate in order to argue about an issue of fact (an explanation of biochemical differences). In that case, she implies that since the differences are material and that since ignoring the differences will not make them go away, they must be

significant somehow. According to this reasoning, Gould is simply talking about the wrong thing: the differences are there, they are significant, ignoring them won't make them go away, and since talking about abuses of them is useless, Gould should be talking about differences. Pert cannot talk to Gould because she wants to argue on a different level of reasoning (see section 4.3).

While it seems unlikely that Pert intends the second reading, it is present nevertheless, as a paradoxically perfect expression of the dilemma she and her colleagues are in. On the one hand, she wants to argue that talk is cheap: talking about abuses does no good. On the other hand, she wants to argue that silence does no good either: ignoring differences won't work. Since she obviously cannot have both, she allows us to infer her preference--talking about differences. The only logic to be found in this reading is that of disciplinary bias.

The effect of the confusion in that crucial sentence is an evasion. Pert is sufficiently ambiguous about what should or should not be discussed that she remains uncommitted to a crystal-clear position. She attempts first to define the problem as a true-false question about genetic differences in which she implies the discovery of their existence is only a matter of

time. Then she attempts to limit the issues to simple procedural disagreements. Finally, she fails to confront directly Gould's claim that the issue of what to investigate next about abuses is the significant problem, not the existence of some differences which he has already described as trivial.

Davis shares, in particular, Slobodkin's convictions about the proprieties of scientific discourse. His concern is not that power and desire may motivate certain scientific errors, but that talking about the existence of such problems will distract science from his version of objective inquiry, thus undermining public confidence in it. Such exposure, then, is dangerous, unnecessary, inappropriate, etc. Similarly, Samelson appears vaguely to think that there is somewhere that such issues can be discussed, but not apparently in public.

Adherence to the position that only problems of fact can be talked about productively in scientific discourse immediately creates difficulties for its proponents. If we cannot talk about Gould's issues in scientific discourse itself, then where? If not now, when? Strict regard for this stance entails a serious moral dilemma, as it requires its adherents to endorse publicly what they may privately regard as execrable science or to remain silent in print about disciplinary

errors that may have significant implications for the wider culture. Moreover, each of the scientists who has criticized Gould has done so by foregrounding Gould's personality, ego, and/or style, at the expense of substantial empirical claims about his scientific errancy. Not only does their strategy cast doubt on the legitimacy of the position itself, but we still have no idea what this position would look like in implementation or any evidence, for that matter, that it actually can be done. To the extent that scientists have raised the issues of personality and ego themselves, they have made themselves responsible for providing guidelines or criteria for discriminating between egotistical and non-egotistical discourses.

The second position invokes the post-structuralist notion that knowledge is a will to power. In his eagerness to demonstrate the error of Gould's resistance to transparency in discourse, and under the pretense of a scientific dispassion, Slobodkin charges him with an extreme version of a Nietzschean relativism which permits every position to be construed as a strategic misstatement in which knowledge equals power and power equals knowledge. In this view, Gould endorses "mere rhetoric." According to Slobodkin, Gould believes that language is merely an instrument for asserting personality and gaining power. This position

is corollary to the first in that it fears that if we acknowledge scientific discourse as vulnerable to the same misprisions that it has long accused culture of, we inevitably will reduce truth to Nietzsche's description of it as "a mobile army of metaphors" (46).

Slobodkin attempts to claim that Gould's public credibility resides in his ability to manipulate information and language to his personal benefit and for his own aggrandizement. The implication is that the various critiques that Gould makes of science are just a collection of personal interests and opinions, without merit of their own, gaining a kind of public currency because they sound so good. Implicit is the hint that Gould is a relativist because it suits him. In critiquing Gould, Slobodkin reduces scientific discourse to a kind of ideological smoke screen that disguises lust for control as scientific discourse. The difficulty he creates for himself is that he subscribes to a version of transparent language that denies such a possibility. In spite of Slobodkin's disclaimer that preconceptions "may be ultimately impossible" to avoid, he chooses to critique Gould's rhetorical opacity as if it were possible. In doing so, Slobodkin uses language that does not appear to live up to the standards by which he judges Gould, thus failing himself to meet

scientific criteria for transparency. He thereby denies himself a place to stand.

Slobodkin's leap from a scientific discourse of transparency to one of culturally value-laden assumptions, that is, to a Nietzschean will to power, leaves out the middle-ground position that Gould himself actually occupies--a liberal compromise. This position acknowledges scientific discourse's own vulnerability in the interest of making it stronger. It may not be able to claim its authority in the same way that it has in the past, but it nonetheless still has a claim to make. Moreover, this position allows science explicitly to reject relativism as inappropriate to the descriptions of the natural world and the real.

Gould's response to his critics would have to be that it is not true that personal motives are not indistinguishable from scientific knowledge. He would argue that they cannot reduce his argument to his self-admitted egotism because self-appraisal is not the issue. Irrespective of ego, and even in the presence of ideology and politics, there is still a viable discourse here. What Gould says has a legitimate place in discourse irrespective of motive. His position is that we cannnot reduce knowledge to power and desire, but neither can we easily separate knowledge from its psychology (desire) nor from its political (power)

content. Only vigilance and questioning of scientists' motives can protect us from the very abuses that Slobodkin is concerned about and to which he commits himself when he pretends a disinterested stance in regard to Gould.

Gould offers two ways to avoid the Nietzschean reduction in the discourse of science. The first is to talk about and make distinct the way in which unselfconsciousness about power and desire can lead to scientific mistakes. That is precisely the strategy that led him to punk eke as a theory and that can help us to understand why certain mistakes invite us to repeat them. The second is to notice precisely the way in which scientific results are interpreted, mythologized, and incorporated into social practice. The lesson that meritocratic ventures such as those described in Mismeasure teach us is that culture inevitably will appropriate science for its own purposes.

The way in which scientific discourse is embedded in cultural discourse affects both of these ways that science must act in order to avoid such a reduction. The problem for science is whether it wants to continue to claim a disinterested position when it is absolutely clear what the results will be of doing so. Science once could claim that its disinterest protected itself.

To continue, however, to deny the way in which culture misuses scientific authority puts science in the position of playing the emperor in new clothes, subjecting science to public ridicule when it says that it only made the bomb, but someone else lit the fuse.

4.2 A New Rhetoric of Scientific Discourse

What Gould's supporters see as hopeful in this debate is that his writings offer the possibility of a new rhetoric of scientific discourse, that is a new set of strategies more responsive to social, political and psychological demands, and a new set of conventions appropriate for representing science in writing and conversation. This new rhetoric accepts the potentials and limits of language: it recognizes the consensual, community-building potential of language, as well as the limits that inhere in the fragility of cultural consensus. This rhetoric offers the availability of alternative vocabularies that science may need to use as it acknowledges the existence of multiple purposes in its discourse. It also accounts for the socially constructed nature of knowledge, recognizing that culture will find uses for science and ways of talking about it that may assign value to phenomena that science prefers to regard as value-neutral, including its own motives.

The distinguishing characteristic of this new rhetoric of science is that it demands a re-definition of traditional views about science writing in general. As Bob Coleman has pointed out, while the early traditions of science writing were unblinkingly linked to candor and frank discussions of the complexities of the practice of science, that tradition began degenerating in the 19th century into something resembling the scholastics' practice of recycling of "received knowledge" (45-46). Scientific discourse has changed from scientists writing for whoever wanted to read their work to a carefully staged public display by "Olympians," usually written in such a way that only other scientists can understand it (1, 47).

The only alternative has been "popular science," a term sometimes used in connection with Gould's writing but which he himself rejects. He has on several occasions deplored the fact that while Galileo wrote in the vulgar Italian instead of Latin, modern popular science writing, especially in America, has become synonomous with adulteration and cheapening (TI). Why this state of affairs should exist puzzles and troubles him, and his own solution has been to write (apparently) as Galileo did. He reasons this way:

> . . . if there is any key to my essays, it's that there is no dilution of intellectual

> content. That is, I write about the exact same things I would write about if I were writing for my most rarified professional colleagues. I just use a different language. I don't use jargon; I define terms. But I think there is no reason to dilute the intellectual content at all. The most abstract, conceptual materials have the highest interest. There's no reason why you shouldn't write about it for everybody. (Dowdey 458-59)

He challenges two assumptions central to the modern practice of writing about science: that it demands a specialized jargon and a specialized audience for it to have intellectual integrity. The inclusion of the chapter on factor analysis in Mismeasure demonstrates materially his commitment to avoid dilution of the content, and the commercial success of the book indicates that lay readers are not automatically dismayed by rigor.

It may very well be that the reason that popular science may be thought of as too easy, or as merely an ersatz vulgarization, is that it does resemble a modern, commercial version of scholasticism. If Coleman is right in his assessment that the important intradisciplinary debates and the evidence of scientific

failure and error have been essentially forced off the pages of scientific journals (47), popular science may simply reflect that reality. Professional scientists write for scholarly publication far more often than for public consumption. Popularizations of science are often done by "science writers" such as John Boslough (Stephen Hawking's Universe), K. C. Cole (Sympathetic Vibrations), and John Gribbin and Jeremy Cherfas (The Monkey Puzzle), all of whom have written respected books.

Nonscientists, however, who write about science are at the mercy of the scientists they learn from. If scientists' scholarly publications have been a primary source of modern cultural understanding of science, it is little wonder that Watson's insider's view of science in The Double Helix shocked both the public and the scientific community in 1968 (Johnson 39). Until that time, Isaac Asimov had been one of the very few scientists who wrote regularly for public consumption, and even Asimov was not a practicing scientist. With only carefully censored accounts of science to research from, it is not surprising that popular science writing has been inadequate to the realities of scientific life.

The recent reentry of more scientists into the public arena of scientific discourse may do much to

rehabilitate "popular science" writing if Gould's rhetoric influences their efforts. What his rhetoric proposes to do is return science writing to the Galilean tradition of writing for a public audience with science's processes and failures exposed as often as its results and successes are announced. He would make science popularization into a version of what Chaim Perelman and L. Olbrechts-Tyteca have called initiation, in contrast to popularization:

> Initiation into a given discipline consists of communicating its rules, techniques, specific ideas, and presuppositions, as well as the method of criticizing its results in terms of the discipline's own requirements. These characteristics distinguish initiation from popularization, which aims at the public at large for the purpose of acquainting it, in nontechnical language, with certain interesting results, without enabling it, however, to use the methods which made it possible to reach these results, or, a fortiori, to attempt to criticize them. The results are presented, so to speak, independently of the science that produced them: they have acquired the status of truths, of facts. The difference between enlightened

> knowledge, that of scholars, and accepted knowledge, which becomes that of the universal audience, is characteristic of the difference between initiation and popularization. (99-100)

Gould sees no significant advantage in passing science along to the public in the form of sanitized bundles of finished knowledge, ready for reconstitution as received wisdom. He does, however, advise of real policy difficulties if science continues to assert that such descriptions of the natural world and the real are somehow inherently better, either for itself or for culture, than the exposure of science as a thoroughly human enterprise.

4.3 An Invitation to Shift Stasis

Gould's rhetoric points to the way in which modern concerns necessitate a shifting between levels of scientific discourse and having the self-consciousness to recognize it when we do it. The rhetorical theory of stasis, which describes levels of reasoning and discourse, offers a useful schematic for understanding how Gould's rhetoric works and how we can obviate the problems caused by a scientific discourse that wants to operate with only a vocabulary of definitions. As Lawrence Green explains (625), stasis theory identifies

a hierarchy of levels of overt, conscious reasoning and questions raised at each:

Policy	What "should/must" we do?
Value	What is "good/bad" about a proposal?
Consequences	What "happens?" (trans. verb)
Definition/Fact	What "is"?

Policy statements necessarily involve the lower-level questions of value, consequence, and definition. The obverse, however, is not true. Definition does not demand necessarily that the questions of the upper level of reasoning follow, although they may.

Linda Bensel-Meyers, in her forthcoming Rhetoric for Academic Reasoning, notes that for each of the levels of reasoning, there is a corresponding, typical category of discourse:

Policy	"should/must"	Political science
Value	"good/bad"	Literature/ Philosophy
Consequence	trans. verb	History
Definition	"is"	Science

Science argues that its discourse is, and must be, located only at the level of definition or fact. Some scientists' quarrel with Gould is that he casually/ blindly transgresses the boundary between science and

other discourses, frequently arguing from the positions of political science, philosophy, or history, when he properly should limit himself to questions of fact. What Gould's texts argue, however, is that his transgression of the border between science and other cultural discourses is by no means unconscious.

Indeed, Gould argues that his transgression is at once inevitable and self-consciously calculated and most important, a caution to his profession. As he says in response to those who need only the appearance of definition, science is "no good if it's a lie" (TI). What he insists is that the stasis of scientific arguments may shift without the writer being aware of it, if she is not careful, which can lead to mistaken notions. In looking up the hierarchy, science sees nothing but trouble and rejects other discourses as inappropriate and irrelevant to its project. In doing so, it traditionally has attempted to flatten the rhetorical hierarchy, attempting to reduce all discourse about science to that of fact. In its insistence, however, that a conscious and theoretical rejection of other discourses is identical to an unconscious and practical rejection of them, science makes itself vulnerable both to fooling culture and being fooled by it.

In the case of Mismeasure, Gould is arguing that

in its insistence on flattening the reasoning hierarchy of stasis theory and arguing that all scientific discourse proceeds from definition, science has failed to notice that it has unconsciously mistaken certain cultural values for an objective basis for scientific investigation. The question of the value ("good/bad") of having a way to judge individual merit has been interchanged with the fact (the "is") of IQ's existence. Moreover, when confronted with the results of such investigations, culture has appropriated them for a variety of social policy purposes, which it has then justified as lodged in scientific fact.

Some scientists insist that specific instances of misuse of such research is aberrant and do not proceed directly from the investigation itself. What Gould argues first is that such aberrancy is totally predictable because the research proceeds from a value-laden premise about a cultural ideal of individual merit, but does not recognize its error, and second, that the problem is not one of specific, individual misuse of research. Rather, it is that the potential for such abuses are inherent in the investigation from the outset, representing a misprision of the basis for the investigation. Investigations into human intelligence have always had their origins in the idea that it is worthwhile to claim distinctive differences between

members of the species. In this way, having mistaken from the outset its investigation of a cultural ideal for a "pure" problem of factual research (the mere discovery of what "is"), science is deluded into thinking that the cultural value is identical to what exists in the realm of the real.

Similarly, science has been fooled by shifting stasis in another way. Geologists, at a loss to account for the real ("is"), the apparent nonexistence of a geologic record to support the theory of evolution, turned specifically to a cultural account--gradualism --to explain a problem of fact. Gould would not argue that such accounts are necessarily in error. He himself says that while Darwin was done in by gradualist assumptions, because "it cannot be any other way," he also would probably never have arrived at his theory of natural selection without Adam Smith's economic model of <u>laissez faire</u> to suggest how natural processes might be explained (TI). What Gould rejects as both suspect and problematic is an <u>insistence</u> on the cultural explanation, whether as a preference or to fill a gap that science has not yet explained. The fact that Darwin's theory of evolution did not require the cultural explanation in order to be valid was an important warning signal that Darwin should have heeded. Because he did not, science clung for nearly a

century to an explanation of physical phenomena that was totally inadequate.

Likewise, when Pert claims that if genetic differences are proven to exist within the human race, we must deal somehow with that "important" knowledge, she loses control of her own use of discourse. Unable to maintain stasis at the level of fact that she says science should remain at, she shifts from a discussion of fact to a discussion of value without so much as the blink of an eye. Further, she permits an easy cultural appropriation of her discourse that leaps to the level of policy without a cultural debate on value. Similarly, Slobodkin and Davis want very much to be arguing the facts of a case against Gould, but end up aruing whose values are the more appropriate.

Science, however, fools itself no more often than culture is fooled by it. Relying on science's insistence that its products both are the result of value-neutral decision-making and are themselves, therefore, inherently value-neutral, culture has busied itself with the commodification of technology. Science repeatedly develops solutions to problems, only to see culture appropriate them, often for purposes that science never intended and does not approve. Nuclear power is appropriated for military weapons development, in vitro fertilization for surrogate pregnancies, and

genetic research for creation of bacteriological weapons. While there are many advocates of good conscience who do not decry the alternative uses to which much scientific research has been put, virtually no one of good conscience argues that those uses are neither troubling nor fraught with ethical difficulty. Science's claims for a value-neutral status are undermined by its continuing naive participation in policy-centered projects whose applications are obviously dangerous and even apocalyptic.

Simply because culture may misappropriate its efforts is not sufficient cause to stop practicing science. Gould himself has seen creationists use his own writings to claim, erroneously, that Gould does not support evolution (J. Cole 38). What is necessary is the recognition of what stasis theory clearly predicts: when formulating public policy that needs the authority of scientific objectivity as a justification, society will appropriate, without hesitation, science's own claims to value-neutrality. The extent to which science has misled itself and culture about the neutrality of its efforts may define the extent to which culture will misuse its products. Further, the extent to which science may make error in its explanation may be a function of its inability to recognize easily the instances in which it has shifted stasis unconsciously.

CHAPTER 5

CONCLUSION

Stephen Jay Gould's writings about science invoke questions about the relationship between relativism, scientific truth, and social value. His writings are, in every sense of the phrase, what the two cultures debate should be about. He demonstrates successfully that implicit assumptions of science about the relationship between Darwin's theories and nature gave us a hundred years of mistaken notions about the history of geology. Gradualists failed to fight off the influence of culture, and the influence of culture prompted Darwinian biologists to misrepresent the facts. Because the gradualist model of social change has been so highly prized in Western culture, scientists have been unconsciously motivated to make biology conform to the social ideal. He demonstrates that this is not an isolated instance of culture's sway and that it has also influenced the investigation of human intelligence. We can expect to find other such instances in the history of science and, without changes in the present, in the future of science.

Gould's work invokes, at the extremes, the opposing claims of transparency vs. the will to power--a binary pair with tremendous appeal to his detractors.

The claims of a transparent scientific discourse offer the security of certainty and an affirmation of science's autonomy from the ambiguities of cultural values. The claims of a will to power in Gould's writings offer a way out of dealing with his assertion that science is not as objective as others have argued it is and an affirmation of science's need to remove itself from cultural debate. Both claims share the attraction of enabling science to maintain its claims to naivete. The claim of transparency demands only that science "rise above" personal bias and does not require that science confront charges made against it. The will to power argument offers science a straw man of an either/or position that is clearly appealing in its simplicity: scientific discourse either remains blind to its weaknesses or it must engage in a will to power in which its weaknesses are used for the lustful purposes of self-aggrandizement and careerism (a la Gould). A preference for this kind of double-bind is understandable because it keeps the world in order. By making available to scientific discourse only two options, science can stave off indefinitely entrance into the ambiguities of language and thereby preserve the illusion of language's finiteness.

What science does not notice, however, is that by casting Gould in a Machiavellian role, it puts itself

in a potentially untenable position. In reducing Gould's position to a Nietzschean will to power, science eliminates the possibility of a public arena for discussion of the politics or values located in science. It reduces truth to a kind of verbal warfare in which truth does indeed become a "mobile army of metaphors," because we have only two incredible, unsolvable choices: either follow me (science) or suffer a fall into darkness.

On the other hand, it is possible that there are only two legitimate descriptions of scientific discourse: one that is marked by self-interest and one that is marked by objectivity. However, those who wish to claim that such is the case are in a very delicate position. When they criticize Gould's rhetoric as representing only debased self-interest, scientists potentially implicate themselves in exactly the same dilemma. Practically and rhetorically, they raise suspicions about their own degree of self-interest. We fairly may suspect that these scientists are unaware that they have moved from a discourse about science to one about values when they depart from their professed objectivity to make ad hominem charges against another. They have, nevertheless, performed a maneuver that shifts the burden of proof. Specifically, they have placed the burden on themselves to demonstrate what a

non-self-interested public discourse on cultural values would look like. So far, none of them have done this.

5.1 Another Option

Despite the caricature that science as ostrich or as Nietzschean relativism leaves us with, Gould offers us a salvageable position in which we can stand. My own attitude towards, and critique of, community is probably not identical to Gould's, although we share certain political commitments. Specifically, I suspect that Gould would not ascribe to language the power that I do. Ultimately, however, I believe that Gould offers us a view of the centrality of community as something more than a distraction. What he offers us, perhaps for the first time in modern scientific discourse, is a positive view of culture and community in which there is adequation between the goals of science and the reality of its existence in a cultural mileu. In so doing, he saves us from the extremes of the ostrich and the anti-Christ and provides us with an option.

In this version of science and community, the truth of science is never, and never can be, completely, finally separate from the cultural, ideological, political power of community. There are, however, versions of truth available to us with which we can begin to know and to disentangle specific

scientific projects from their situation in society. They require, though, a redefinition of the boundaries between science and culture and as Richard Rorty has noted, a recognition that the fact-value quarrel is not about differences in methods of attaining the same goal but about different purposes (196-97). As Rorty puts it:

> This contrast is real enough. But it is not an issue to be resolved, only a difference to be lived with. The idea that explanation and understanding are opposed ways of doing social science is as misguided as the notion that microscopic and macroscopic descriptions of organisms are opposed ways of doing biology. There are lots of things you want to do with bacteria and cows for which it is very useful to have biochemical descriptions of them; there are lots of things you want to do with them for which such descriptions would be merely a nuisance. Similarly there are lots of things you want to do with human beings for which descriptions of them in nonevaluative, "inhuman" terms are very useful; there are others (e.g., thinking of them as your fellow-citizens) in which such descriptions are not. "Explanation" is merely

> the sort of understanding one looks for when one wants to predict and control. It does not contrast with something else called "understanding" as the abstract contrasts with the concrete, or the artificial with the natural, or the "repressive" with the "liberating." To say that something is better "understood" in one vocabulary than another is always an ellipsis for the claim that a description in the preferred vocabulary is more useful for a certain purpose. If the purpose is prediction, then one will want one sort of vocabulary. If it is evaluation, one may or may not want a different sort of vocabulary. (197)

In other words, we must be particularly clear about our purposes (motives) and the possible ways in which we can trip ourselves up in language.

The position demanded by such a version of discourse is an exceptionally difficult one to maintain, because it requires that we occupy two different rhetorical locations at once. On the the one hand, we are in the position of having to accept that science is being truthful when it claims (predicts) the probable existence of quarks in the universe and on the other, of having to challenge its claims that it is

being truthful when it claims that heritability between racial groups is predictive (understanding) of significant race differences. Such vigilance is tiring and demanding. As Gould has noted in the case of Immanuel Velikovsky, it is difficult for even another scientist to assess adequately the claims of science with which he is not familiar (ED 159). Responses to Gould himself from other scientists make that clear enough.

If we are to take advantage of how Gould helps us to define a version of community in which we can use science and not be victimized by it, laypersons as well as scientists must be prepared to question science. But how? A recent report from the National Science Foundation

> shows that fewer than 6 percent of American adults can be called scientifically literate. The rest think that DNA is a food additive, Chernobyl is a ski resort, and radioactive milk can be made safe by boiling. (Stone 102)

Jon Miller of the Public Opinion Laboratory at Northern Illinois University, claims that "'Only about one in twenty adults knows enough about science to function effectively as a citizen and consumer when asked to help formulate public policy about issues like nuclear

power or toxic waste'" (Stone 102). This lack seems to argue at least partially for an educational response as public policy.

In addition, however, and more important to both science and culture, saving ourselves from victimization requires that we commit ourselves to a simultaneous vigilance and self-consciousness about the social context in which science must exist. It requires a critique of science which constantly refers to the social context of science without reducing it to that social context. It requires a critique of personal contexts without reducing science to personal contexts. It requires a critique of boundaries without reducing science to just another version of cultural discourse. It commits us not to iron curtains but to shared borders between different communities with interests in common and the concomitant necessity for constantly patrolling the borders to keep the crossings clear, not closed.

Such vigilance is at cross-purposes with our inclinations. Gould cautions us in Time's Arrow that geological history is inevitably paralleled to human history and notions of human progress. We attempt at every opportunity to describe geology as narrative, and in our unconscious attempts to parallel human to geological history we reveal the desire that makes us

all, scientists and nonscientists alike, vulnerable to the misprision that the discourse of science is identical to the world it describes.

We must remind ourselves constantly that evolution, the search for our own origins, is the ultimate quest narrative. While narrative sometimes may aid our understanding of the physical world, the very fact of narrative is implicitly a challenge to, or necessarily a distortion of, scientific causality. As soon as we turn evolution into a narrative, we have distorted it. Being human, it is inevitable that we think in terms of narrative, but it always holds the potential for misleading scientists into their preferences. When that happens, science becomes just another version of Harold Bloom's Romantic narrative of internalized quest (3-24).

5.2 The Liberal Compromise

Rejecting science as the ultimate source of truth in our universe, and endorsing a liberal compromise in which we privilege neither the discourse of science nor that of culture but look to both for explanation and understanding, need not leave us without hope or mired in an indeterminate relativism. It offers us instead a version of community similar to that described by Rorty (203-208) in which humanity is neither compelled to

look outside itself to science (or to any other authority) for affirmation, nor to reject out of hand explanations of itself made possible by science's discoveries.

The solidarity that a liberal compromise offers us is to be found in our acceptance that "out there" tells us nothing more useful about ourselves than "in there" does and vice-versa. The hope that it offers us is to be found in the knowledge that such acceptance is liberation, not the source of despair. It offers us the opportunity to create an assertive new world out of realistic understanding of the options available to us and not out of dead desire. It is not the easy or facile choice that some ideologues claim it to be, committing us to everything and therefore to nothing. If anything, in the late 20th century, the liberal compromise forces Westerners to acknowledge finally that we are at the end of unlimited options, resources, and choices.

The liberal compromise insists on a new version of community that is based on the reality that science can no longer deliver on its promise to solve, by wiping them out of existence with technology, all the problems that civilization has created for itself. The science that offers us a technology to power the world simultaneously enables politics to exterminate us; a

technology to feed the world simultaneously enables industry to poison us; a technology to heal us simultaneously enables medicine to torture us with prolonged pain and suffering. Whatever else the knowledge of this reality offers us, it surely must remind us that we need both versions of nature to survive: it is indeed nonmoral, and it indeed is not nice to fool Mother Nature.

Stephen Jay Gould's new rhetoric of science is bound to frighten some of us because he removes certainties that we have come to depend upon. Nevertheless, his writings offer us alternatives and an important reminder: humanity's choices about what to do with the technology that it so naively, and perhaps even stupidly, invited into its presence is not located in the authority of science itself. They are located in our hopes for ourselves. We first must decide whether we wish to survive the 21st century. Then we must decide how to obtain a consensus for that goal and to develop a strategy for its implementation. While science will give us much-needed data and predictions for the success or failure of our enterprise, it cannot tell us what that enterprise should be. If we continue to look for our communal identity and our common future in the false hope that science can deliver us from the valley of the shadow of death, we effectively prevent

our emergence as a species into adulthood. Instead of becoming the mature custodians of our own destinies, we will remain, for as long as we can hold the fantasy in focus, as the adolescent wards of our unfulfilled yearnings and imaginings.

At the end of innocence is either wisdom or annihilation.

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VITA

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