ONE Science as a Culture and as a "Side"

Many years ago, in the late 1980s, as a postdoc in genetics at the University of California at Davis, I was interviewed by National Public Radio on the subject of the Human Genome Project, then beseeching Congress and the American public for a few billion dollars.

Sure, it would keep molecular biologists employed into the foreseeable future, but was it science?

Of course not, I told NPR, with the assuredness that comes with having recently earned a doctorate and of working in a laboratory with radioactive isotopes, toxic chemicals, and expensive machines with flashing multicolored lights. Science involves *testing hypotheses;* we all know that. We teach our students that. The Human Genome Project wasn't testing any hypotheses—it was merely collecting a large mass of data because we now could. We had the capability to carry out a big molecular genetics project, but it was disconnected from science; it was not the way we were taught that science was supposed to be.

I was also speaking with the scientific reasonableness that came from knowing we had recently beaten down a challenge to science by creationists, who had been working to get their ideas accorded equal time alongside evolution in science classes. A few years before the Human Genome Project discussion, a federal judge named William R. Overton had ruled that creationism wasn't science and therefore should not get equal time—or any time at all—alongside Darwinism. Why? He had

been told by respected scientific authorities that science tests hypotheses and creationism does not.

A few years later, I was recruited to help review a few hundred grant proposals by the scientific society Sigma Xi, which gives small sums to graduate students starting their thesis research. I was instructed to divide the proposals into two piles: those that tested hypotheses, and those that did not. The ones in the first pile would get about five hundred dollars each, and the ones in the second pile would not.

So, if you did not test a hypothesis, you could be denied five hundred dollars, but you could get three billion. And a creationist who did not test a hypothesis was *not* doing science, but a molecular biologist who did not test a hypothesis was *indeed* doing science.

This made little sense to me at the time, and it makes little sense to me now. I have no doubt that the Human Genome Project is (or was) science, and little doubt that creationism is not science. But testing hypotheses does not seem to have much to do with it. So what does? What makes something science? What makes something nonscience, like humanities—which are respectable and scholarly but nevertheless differentiated from science? And what makes something pseudoscience—that is, something disreputable?

A DEFINITION OF SCIENCE

Let us begin with a definition: *Science is the production of convincing knowledge in modern society.*

This is what I mean when I use the word *science,* and all of the polysyllabic words in that definition merit some discussion. By using *production* we acknowledge that science is not a passive experience. Scientific knowledge is a product—and as a product it is the result of some process. That process is science, and it is what we mean to analyze. There is a subtler and more threatening point embedded in this recognition, however. If science is the active production of something—say, reliable information about the universe—then it is more than, or at least different from, mere discovery. Discovery is a passive operation: to a suitably primed observer, the fact merely reveals itself.

But of course, "facts" of nature, of the universe, cannot reveal themselves, for they cannot act. The act of discovery hinges on what is constituted by a scientist's being "suitably primed." Being ready for a discovery implies a context of the right social environment, the means, and the intellectual precursors that allow the discovery to be rendered sensible. It is unlikely that natural selection could have been discoverable outside the context of competitive, industrial Victorian England. At any rate, it had never been discovered before and was recognized separately by Charles Darwin, Alfred Russel Wallace, and Herbert Spencer at about the same time. The fertilizing union of egg and sperm could not be discovered until the invention of microscopes. In the absence of the germ theory of disease, the initial serendipitous discovery of antibiotics would have passed silently.

Such examples illustrate that the production of scientific knowledge is highly context-specific, and that it is the context, more than the particulars of the discovery, that are critical. The individual discovery (or discoverer) is not terribly important, for if Darwin had never lived we would still have natural selection; if Watson and Crick had never lived we would still have the structure of DNA (the great chemist Linus Pauling was only weeks away from figuring it out himself).

If you can discover something only when you are ideologically, technologically, and intellectually prepared for it, then it seems to follow that the interesting question for understanding science is not "How was the fact discovered?" but rather, "What was needed in order to recognize and identify the fact?" Since facts are now seen to be actively produced rather than passively revealed, the production of facts becomes something that we can study, as one would study any other social or cultural process.

By *convincing,* we mean that there is a social process beyond mere discovery or fact production. Somehow the fact has to be accepted, in order for other scientists to incorporate it and build upon it. While it is certainly true that the growth and progress of science are due in part to the community at large recognizing that somebody's work is "correct," it is also true that some ideas we now know to be correct have sometimes been slow to be accepted (such as continental drift), and ideas that we now know to be wrong have sometimes been rapidly and widely accepted (such as Piltdown man).

These mistakes would not exist if science proceeded simply by the rejection of wrong ideas and their supplanting by right ones. Moreover, the mistakes can be rendered invisible by the pretense that science actually works that way—that is, merely figuring out what's true—which serves to conceal the networks of communication, authority, and power that retard or augment the spread of knowledge. By focusing on science as specifically convincing knowledge, we call attention to the processes that render its facts visible and credible to others. Revelation, for example, is a real source of knowledge, but not of convincing knowledge, for the knowledge can be shared only by someone who has had a similar revelatory experience. Science is different in that its work is directed toward the goal of successfully convincing an open-minded outsider of its propositions.

By *knowledge,* we mean reliable information about the universe. It is something you can bank on. Of course, it could be wrong. But if it were wrong too frequently or too egregiously, it wouldn't be very reliable. So science is information about the universe that comes with some source of authority behind it. The authority is different from a shaman's, or from the pope's, or from a policeman's , and consequently its source merits some reflection.

Finally, by *modern society,* we mean the ideas, values, and social practices that arose in Europe and its satellites and colonies at a time in the eighteenth century often referred to as the Enlightenment. As the name suggests, we look back on this time as an era of illumination, when formerly obscure things finally "came to light." There are, of course, reasons why such conceptual changes occurred at that time—it is not as if there were simply more geniuses being born—but, more significant, it was there and then that science as we recognize it today began to take form. This is not to denigrate the thought and work of people from other times and places but simply to note that what we now call science is not directly descended from their thought and work but rather from the work and thought of those proverbial "dead white males."

We may certainly admire the metallurgy of ancient West Africans, the astronomy of ancient Mesoamericans, or the architecture and philosophy of the ancient Greeks or Chinese, but none of these achievements

represented science in any easily recognizable form. The cultural differences among these peoples, especially in relation to the Euro-Americans of the Enlightenment, are far larger than the superficial similarities that emerge from the fact that they all thought deeply about the natural world and applied the results of that effort successfully in diverse ways.

Science is different, and began to emerge only with a strange idea of the Enlightenment: that the physical world—the world of perceptions and sensations and measurability—was somehow different and separate from the spiritual and moral worlds. Nature was amenable to certain forms of knowledge production of a different order than the kinds of knowledge one could obtain from the spiritual realm. This was not to say that God or heaven did not exist, only that they were separate and distinct from the physical world. This bracketing off of nature from supernature became the signature of science. One was subject to measurable forces and deep regularities; the other was capricious, miraculous, and unknowable. Or, at least, knowable in a very different way.

One example can serve here: the ancient Greeks, for all of their contributions to our knowledge, had no word for religion. It is not that they were not religious, or that they lacked confidence in divine spirits and beings, but rather that these forces permeated their lives so inextricably that it made no sense to bracket them off from the mundane, earthly aspects of their lives and worlds.

In fact, the division of nature from supernature, of the physical universe from the metaphysical one, has been unfamiliar to most people over most of the course of human history.

Biblical Hebrew had a word, רוח *(ruach),* translated into Greek as πνευμα *(pneuma)* and subsequently into English as *breath* or *wind.* (You can easily see the association between *breath* and *pneumonia.*) The same word is, however, also translated into English as *spirit* from both the ancient Hebrew and ancient Greek. Of course, *breath* and *spirit* are associated as well, but what mostly seems to divide them is the invisible barrier between the physical world of breath and the metaphysical world of spirit. Without such a barrier as a part of one's conception of the universe, spirit and breath might well be the same thing.

It is specifically the construction of that invisible barrier which dif-

ferentiates what we recognize as science from other kinds of thinking about the world and manipulating it, even those of our own more remote cultural ancestors. Thus, we restrict *science* to mean specifically the kind of thinking that arose in Europe in the seventeenth and eighteenth centuries, when the respective domains of nature and supernature began to be circumscribed, in contrast to the more widespread view of seeing them as mutually interpenetrating and porous—indeed, as not really different from one another.[1]

THINKING ABOUT SCIENCE

People are always up in arms about science education and science literacy in ways that they don't seem to be up in arms about humanities education and humanities literacy.[2] C. P. Snow was a distinguished physicist at Cambridge as well as a successful novelist (his wife, Pamela Hansford Johnson, was an even better novelist). Ever since his essay "The Two Cultures" appeared in the 1950s,[3] the academy has been forced to acknowledge that the price we pay for knowing more and more about the universe is that knowledge becomes so specialized that a scientist often knows nothing *but* science.

Finding himself astride two distinctly different, although both highly intellectual, social circles, Snow set out the proposition that academic life was increasingly becoming bifurcated. On the one hand, the humanists on campus were becoming increasingly distant from, and uninterested in, the latest developments in science (which included the new areas of computers, space flight, and molecular biology). On the other, scientists were becoming increasingly removed from art, literature, and aesthetics, the very things that make us "human."

In short, Snow said, these two groups of scholars were at the point of becoming distinct campus cultures, a term he self-consciously chose to make the analogy to anthropology clear. They think about the world differently, have different interests, languages, value systems, and can hardly communicate meaningfully with one another. What Snow saw at Cambridge University was a microcosm of what he believed was going on

in society generally. There were otherwise smart people who knew about quantum physics but had once rather pathetically "tried a bit of Dickens"—as if Dickens were particularly deep—and there were also smart people who could not even articulate the Second Law of Thermodynamics (that physical systems tend toward entropy or disorder, or leak energy, which is why a perpetual motion machine is impossible), a situation Snow equated with having never read anything by Shakespeare.

Like other classic texts, Snow's essay has been read differently by different audiences. In one self-interested reading, Snow is seen to be railing against the ignorance of science by humanists.[4] Perhaps the oddest thing about that particular reading is that it undermines Snow's title and very theme—the two cultures—by which he clearly intended to convey the relativistic notion of difference without hierarchical ranking; this reading replaces Snow's insight with its conceited, ethnocentric opposite.

A leading British literary critic lashed out at Snow for presuming that any sort of equivalence was appropriate between a mere equation and the works of Shakespeare, which express the grandest and basest of human motivations and articulate the deepest and most resonant feelings we all share. And, he added for good measure, Snow wasn't even all that good a novelist.[5]

In recent years, discussions about the science-humanities divide in academics and in life generally have built on Snow's essay, and it is difficult to find anyone defending the idea that the situation is improving. On campus, humanists publish books, scientists publish articles. As a result, humanists tend to read books, and scientists tend to read articles. Sure, there is the occasional scientist who has read a novel in the past year (science fiction, of course, doesn't count) and the occasional humanist who happily slogs through the latest issue of *Scientific American* to keep abreast of superstring theory and what's new in short interspersed elements of the genome—but they are, relatively and absolutely, a small minority. The rest of us find it more than a full-time activity to keep up to date on our subspecialty (say, molecular anthropology), much less on our specialty (biological anthropology), much less on our general field (anthropology, or whatever it actually says on the diploma on the wall), still less on other sciences—and still have the time and mental energy to read novels.

This "cultural" difference is manifested in other ways as well. In the late 1950s, the National Science Foundation was still relatively new, and cold war anxieties, aggravated by the Soviet orbiting satellite, Sputnik, were promoting both the expansion of science and the massive transfer of financial support for it from the shoulders of private foundations to the government bureaucracy. Not surprisingly, the academy began to change, as did its priorities. Major universities that had long been known as centers for training and research began to rely on research grants as a major source of their operating budget. This in turn placed a new emphasis on the ability of new faculty to apply successfully for grants to fund their research. Indeed, anything else they did began to be downplayed; a science department's significance became measurable in terms of the amount of grant money it brought in. And if the quality of teaching, or even the structure of the curriculum, suffered thereby, it was just an unfortunate consequence of the ruthlessly competitive marketplace, in which scientific grants dictated both academic stature and clout within the university.

I didn't realize it at the time, but that was why, as an undergraduate at a prestigious research university, I had a professor who could barely make himself understood in English trying to teach me integral calculus; a professor who couldn't explain anything sensibly trying to teach me introductory biology; and a professor who never looked at the class trying to teach me cell biology.

It wasn't at all the same in the humanities. In the humanities there was no money—or, at least, very little. Consequently, your advancement wasn't predicated quite so much on getting grant moneys. If you did, that was a feather in your cap, but it wasn't like biochemistry, where you were simply expected to. In both the sciences and the humanities you were expected to publish, but publication in the sciences required significantly more investment of capital. Consequently, faculty in the sciences were being recruited more and more one-dimensionally, as departments loaded up on faculty in hot (i.e., fundable) areas of research. If the curriculum suffered, so be it.

By the time I began teaching at Yale in the 1980s, the biology department had nobody who could teach undergraduate anatomy. Anatomy had to be taught in the anthropology department. Weirder still, stu-

dents who took, say, Comparative Primate Anatomy did not get science credit for it. They got social science credit, since the home department, anthropology, was officially a social science department. It didn't matter that the course was, by anyone's definition, science; that it was a laboratory class; that it was competently taught; and that it was filling a significant gap in the curriculum. What mattered was that *we needed to patrol the boundaries of science—particularly in its bureaucratized form—and aggressively regulate it.* Even if the regulation was arbitrary and produced bizarre results, the ability to decide what counted as science was a form of social power that was not to be surrendered lightly.

I was even more confused when a popular course I taught on the evolution of human behavior was not considered science, although human evolution was my specialty, while a similar course offered in the biology department, by someone whose specialty was *not* human evolution, *was* considered science. What did that imply about science? That it was anything an officially designated scientist said, regardless of their expertise?

The answer was made a bit clearer in 1995, when a mathematician and a biologist published *Higher Superstition: The Academic Left and Its Quarrels with Science.* This book represented one front of what came to be known as the "science wars" in the mid-1990s and suggested that there was some sort of anti-science conspiracy on the part of creationists, animal rights activists, philosophers and historians of science, and literary critics—in short, on the part of seemingly anyone who had anything remotely critical to say about any aspect of science. Although by no means a prominent member of it, I considered myself a habitué of the "Academic Left," and yet I was unaware of any such conspiracy. The book's tone was odd, rather more like something you might expect from the Inquisition than from a product of more recent times. But what struck me most was a thought experiment the authors suggested. If the humanities faculty of a university

> were to walk out in a huff, the scientific faculty could, at need and with enough released time, patch together a humanities curriculum, to be taught by scientists themselves. It would have obvious gaps and rough spots to be sure, and it might with some regularity prove inane; but on the whole it would be, we imagine, no worse than operative. What the

> opposite situation—a walkout by the scientists—would produce, as the humanities department tried to cope with the demand for science education, we leave to the reader's imagination.

To my imagination, at least, the result would probably be a science curriculum with "gaps and rough spots" as well, but one on the whole far better *taught* than previously. One of the consequences of basing decisions about hiring and promotions for science faculty strictly on funding and research, after all, as is customary at major universities, is the widespread devaluation of the quality of teaching in the sciences. Scientists generally receive far less experience teaching while in graduate school, and fewer teaching responsibilities while on the faculty, than do their counterparts in the humanities. If practice makes perfect, then an average scientist might be expected to develop into a less perfect educator than an average humanist. Moreover, if academic scientists are at all as smart as they're cracked up to be, then they certainly realize that their professional fate rests with funding and research, and consequently any time they spend improving themselves pedagogically would act against their own professional interests.

So, on what basis could one realistically expect that scientists would teach a humanities curriculum more competently than humanists would teach a science curriculum? There is only one basis on which to expect that, namely, if sciences are simply hard and humanities are easy. Scientists would necessarily then be smarter than people in other fields and could be expected to pick up those fields more readily than a nonscientist could pick up science.

Maybe that's true. I don't know. But it's not the kind of thing I'd publicly crow about, because it sounds kind of egotistical, arrogant, and boorish. And like a popular unflattering stereotype of scientists.

THE SOKAL HOAX

Okay, this is hilarious. I hope you're sitting down.

There's this journal called *Social Text*, which publishes a lot of this left-wing humanistic stuff. It's a non-peer-reviewed opinion journal. This

scientist at New York University, a physicist named Alan Sokal, decides to show what dopes these humanists are, so he sends them a manuscript called "Transgressing the boundaries: Toward a hermeneutics of quantum gravity," which is full of double-talk and bullshit. The editors are delighted that a physicist apparently wishes to contribute something to the dialogue about science that they are trying to create.

So anyway, they suggest some revisions but figure that since the point of the journal is not the dissemination of new science and since the author is a physicist at a reputable university, he probably knows what he's talking about when it comes to physics, and so they publish it.[6] And as soon as the article comes out, he goes public with the story that he got a totally bullshit article published in their journal.[7] Ha, ha, ha!

Man, that was great! Did those guys look stupid! Score one for our side!

The "Sokal hoax," as it came to be known during the summer of 1996, showed science in a very strange light, scoring points at the expense of its university colleagues. Not only were there two cultures, but one had seemingly declared war on the other. Journalists didn't have to look hard to find scientists who could scarcely contain their glee about a paper that made humanists look so foolish.[8]

But it actually sounded rather more like the final revenge of that antisocial geek with the plastic shirt-pocket protector (another popular unflattering stereotype of scientists). After all, what kind of person goes out of his way to show how smart he is by humiliating others? And then gloats over it? It was that combination of malice and arrogance that left a bad taste in people's mouths.

The paper, obviously, had been submitted under false pretenses. It is an assumption of the scholarly process that one is dealing with a scholarly submission in good faith. Once the good faith agreement is violated, history shows quite clearly that it's not all that hard to get a bogus paper published in the scientific literature. So the Sokal hoax shows nothing about whether the standards are lower in sciences or humanities, or whether one or the other is easier to fool.

But who ever heard of an art historian trying to make biochemists look foolish? Why would they bother?

And yet the distinguished physicist Steven Weinberg could write, "Like many other scientists, I was amused when I heard about the prank."[9] Apparently even mature and subdued scientists were amused before, or instead of, being appalled. Thus Sokal's act, which would ordinarily be regarded as sociopathic, was actually resonant with (at least major parts of) the scientific community.

How had we gotten to such a point, from merely the wry observations of C. P. Snow a few decades earlier? What was the source of such open hostility between the two cultures?

Sociologist Dorothy Nelkin found several factors at work, all related to the erosion of an informal contract between science and modern society.[10] One was a widespread public call for greater accountability on the part of scientists, in place of the honor system that had long been the norm. A second was the large infusion of financial support from "private sector" interests, with attendant claims upon a scientist's loyalty, in turn affecting the public's perception of scientists as a source of unbiased knowledge. Another was the overall relativizing of scientists in society. At least in the last of these, the role of humanists can be discerned, with ethnographic techniques broadly adopted to study science and scientists, just as one would study the origin and production of knowledge in any other culture.

Perhaps this rise of the "anthropology of science" entails a bit of iconoclasm, if we try to study scientists as we study the Yanomamo of Brazil or the Hopi of Arizona. Some scientists find it insulting or degrading (which should make you wonder how the Hopi feel about it, at the very least). Some find it valueless, as if there were something an outsider might see about human behavior that would be invisible to an insider (which, obviously, is a major rationale for more than a century of serious ethnographic research on anybody). Some find what humanists write about science to be impenetrable and jargon-laden. Alan Sokal himself complained that their "incomprehensibility becomes a virtue; allusions, metaphors, and puns substitute for evidence and logic." And, finally, there seems to be a widespread insecurity that humanists have proved, or have convinced themselves, that there is no external reality—that all is perception, or text, or politics.

Sokal, for example, invites "anyone who believes that the laws of physics are mere social conventions" to step out of the window of his twenty-first-floor apartment. The like-minded Oxford biologist Richard Dawkins likewise baits his self-designated academic antagonists to go their scholarly meetings on flying carpets rather than on airplanes.

While it is widely appreciated that there is a social or cultural construction of reality, that does not mean that there are no laws of nature, or that there is "nothing out there." It *does* mean that it may be difficult to distinguish facts from meanings, and that facts are, at the very least, expressed through the medium of language, which is how humans most fundamentally impart meanings to things.

Notice that Sokal did not even say "social construction" but "mere social convention"—which clearly implies something very different—an entirely arbitrary agreement to say "God bless you" after someone sneezes, for example. Once again, that is not at all what we mean by "culturally constructed." We mean that scientific facts are produced, and exist, within a historical and social matrix of meaning. Thus, while no humanist would deny that falling twenty-one stories out of a building is likely to be fatal, our understanding of falling is a cultural contingency. When the earth was thought to be the center of the universe, falling down meant being drawn to the center of the universe. In the seventeenth century, after Jupiter was shown by Galileo to have its own moons, a question become visible that had previously been concealed: if you fell down on Jupiter, would you fall back toward earth or back toward Jupiter?

The point is that nobody had to "discover" falling; it was always there. But what we *think* about falling, that is to say, the science of falling, is derived in large measure from Isaac Newton's construction of it. And Newton's work could only succeed the work of Copernicus and Galileo, for their own ideas made Newton's possible and indeed continue to render Newton's work meaningful. To the extent that more recent work has superseded or generalized Newton's, it is nevertheless contingent upon Newton's. (It is hard to imagine the history of physics bypassing Newton, for he is so iconic, but sooner or later someone else discovers everything.)

THE CULTURAL CONSTRUCTION OF KNOWLEDGE: "WE ARE APES"

A nice example of the problem in confronting the constructed nature of scientific facts can be seen in an essay published in the *Journal of Molecular Evolution* in 2000. It is a minimally referenced essay, labeled "Opinion" and written by the editor-in-chief, the distinguished biochemist Emile Zuckerkandl.[11] The fact that it is an opinion piece in a journal that rarely publishes them, that it contains but a single reference, and was submitted by the editor-in-chief himself are all relevant. It suggests a very important issue—perhaps commenting upon a brilliant new discovery in molecular evolution?

Alas, no. The piece is about "social constructionism" and rails against an article published in a different journal by the paleontologist Stephen Jay Gould. Gould, of course, did not deny that there is a reality, but whatever he did say was sufficient to get Zuckerkandl inflamed to self-publish a response. Zuckerkandl begins by superciliously drawing distinctions between "the process of discovery" and the early and late stages of maturity and stability of scientific knowledge. "Society does intervene in some important ways in the acquisition of scientific knowledge," he concedes, "yet, at the end of the day, none of these ways affects the content of the scientific product." Since these claims are unreferenced, we cannot know whether he believes this is common knowledge or is simply making it up, oblivious to the difficulties in making such broad declarations.

As luck would have it, I was already engaged in studying a scientific fact that Zuckerkandl himself had discovered (the extraordinary genetic similarity of human and ape) and was busy writing a book on the cultural construction of that natural fact. Zuckerkandl had found that, when you compare the amino acid sequence that constitutes the protein part of hemoglobin—which transports gases in the blood—between human and gorilla, you find only two differences out of 287 possibilities. Thus, he wrote, "from the point of view of hemoglobin structure, it appears that gorilla is just an abnormal human, or man an abnormal gorilla, and the two species form actually one continuous population."[12]

The distinguished paleomammalogist G.G. Simpson responded bluntly. If any competent biologist can tell a human from a gorilla at thirty paces, does it not follow that the "standpoint of hemoglobin"—which seems to confuse the human and the gorilla—is a rather silly standpoint to take?[13] And yet, over the ensuing decades, the "standpoint of hemoglobin"—or relations as told from molecules, as opposed to the animals in which they are found—became so dominant that the biologist Jared Diamond could write a best-selling book calling us the "Third Chimpanzee," predicated on that very genetic comparison.

Actually, though, it is not entirely clear that the discovery of the genetic near-identity of human and ape is strange or paradoxical in the first place. In fact the genetic relationship basically replicates the anatomical relationship: in the great panoply of life's diversity, humans and apes are very, very similar, yet diagnosably different, throughout. The idea that this has a self-evident meaning, which is somehow counterintuitive, is simply the result of two cultural facts: our familiarity with the ape's body, and our unfamiliarity with genetic comparisons.

We have, after all, been studying chimpanzees scientifically since 1699. When they were new and interesting, back in the eighteenth century, scholars ranging from the Swedish biologist Carl Linnaeus to the French social philosopher Jean-Jacques Rousseau and the Scottish jurist Lord Monboddo were overwhelmingly impressed by the striking physical similarity of ape and human. Linnaeus, Rousseau, and Monboddo were all quite satisfied to understand the ape as a variant kind of human—one lacking certain of the essential features of humanity, to be sure, but nearly human nonetheless.

A couple of centuries later, the physical differences between human and ape had become fairly well understood. Anyone who knows what to look for can easily distinguish the femur (thighbone) of an ape from that of a human, although they might look identical to a naïve viewer. The observer might *not,* however, be very inclined to present the relationships of the thighbone as a single number, a scalar quantity. How do you reduce a comparison of three-dimensional forms into a one-dimensional number?[14] Gene sequences, on the other hand, are long chains of simple subunits; their differences are easy to tally and quantify

because they are conceptualized in a single dimension, a line. There is charm in comparing linear quantities; everything is either higher than, lower than, or equal to everything else. Witness the popularity of the linearized IQ in the twentieth century, which could be easily compared and rank-ordered, as a stand-in for intelligence, which cannot be so easily compared. So it is with genetic sequences: the extent of their differences can easily be represented numerically, but it is a crude stand-in at best for the overall relationships of the species.

Moreover, it is not clear—as Simpson argued—whether the perceived genetic relationship is transcendent or just erroneous. The fact that biochemically or genetically you might not be able to tell human and ape apart does not necessarily mean that they are identical; rather, it might just mean that the differences between them have not yet been fully studied and evaluated. Indeed, the same pattern is actually present genetically and anatomically: each corresponding part is very similar, yet diagnosably different, in human and ape.

In other words, Zuckerkandl's discovery that human and ape are merely abnormal variants of one another, surprisingly similar from the standpoint of hemoglobin (or protein and DNA sequences more generally), was a highly constructed fact. It is true enough that humans and chimps are more than 98 percent genetically identical, but it is not necessarily true that this is (a) more than, say, the similarity of a human and chimp femur, (b) "realer" than any sort of comparable measurement of the femur, or (c) higher than we should have anticipated.[15]

Another fact can help contextualize the genetic similarity of human and ape: the structure of DNA constrains us to be more than 25 percent genetically identical, in a base-for-base comparison, to a carrot. But saying on that basis that we are genetically "very abnormal carrots" or "over one-quarter carrot" would properly be considered idiotic.

Where, then, is the logic for assuming that the extent of our DNA matching is a measurement of our "true," "deep," or "real" similarity? The DNA matching is an arbitrary measurement, not necessarily highly informative, not obviously highly profound, and rendered meaningful or significant only in a cultural context that privileges genetic information, mystifies genetic information, and privileges scalar comparisons.

Far from being a "lost cause," as Zuckerkandl condescendingly put it, constructionism is what allows us to make sense of his own work; his own un-self-consciousness about it is the shortcoming that prevented him from understanding it himself.

CHANGING TIMES

There does seem to be a time within memory when the science faculty were rather more introspective and less haughty. In 1954, a botany professor published the results of a small informal study: he asked fifteen biologists a set of questions at their Ph.D. orals: Can you identify (1) the Renaissance, (2) the Reformation, (3) the Monroe Doctrine, (4) Voltaire, (5) the Koran, (6) Plato, (7) the Medici family, (8) the Treaty of Versailles, (9) Bismarck, and (10) the Magna Carta. He considered barely one-third of the answers to be satisfactory.[16]

A year later, the geneticist Conway Zirkle went so far as to construct this mock diploma:

> THE JOHNS HOPKINS UNIVERSITY
> certifies that
>
> *John Wentworth Doe*
>
> does *not* know anything but
> Biochemistry
>
> Please pay no attention to any pronouncement he may make on any other subject, particularly when he joins with others of his kind to save the world from something or another.
> However, he has worked hard for this degree and is potentially a most valuable citizen. Please treat him kindly.[17]

Of course, we should be reluctant to generalize from these two examples, but two things are clear. First, they were both published in the journal *Science,* the leading general science periodical in America, which suggests that they had some broad resonance with the scientific community at large. And second, those very graduate students who

couldn't identify Plato or Voltaire and knew nothing but biochemistry in the mid-1950s had matured into the tenured gatekeepers of science by the mid-1980s.

What seems to have happened is that the ignorance or benign neglect of other areas of scholarship, noted by C. P. Snow and Conway Zirkle in the 1950s, had metastasized into the paranoid fear and loathing of the "science wars" a few decades later. To be sure, as the old saying goes, just because you're paranoid doesn't mean nobody is out to get you. And indeed there are forces working to undermine aspects of science education—most prominently, creationists; but also (with diverse motivations and credibilities) zealous animal rights activists, greedy corporations, ambitious politicians, sanctimonious anti-abortionists, not to mention just old-fashioned hucksters. But where is the wisdom in imagining that they are all colluding, when they are simply pursuing diverse agendas that happen occasionally to line them up against the perceived best interests of science?

RIVERRUN

You know you're in trouble when the novel's first word is recognizable but unfamiliar and is not even capitalized. It violates the most basic rules of English prose. You are going to have to work hard to get something out of it. Will it be worth the effort?

In this case, the novel is James Joyce's *Finnegans Wake,* and the general consensus is that it is indeed worth the effort.

But why should the onus be on me to have to work so hard to read a story? Stories are supposed to be easy to understand; they're supposed to be *stories.* Not like science, for example, where we take it for granted that years of study are required to master the vocabulary and concepts, where meanings will be hidden from all but the fully initiated.

On the other hand, why shouldn't specialists in things other than science require a specialized vocabulary and conceptual apparatus to communicate their ideas as well?

One of the weirder fronts on the "science wars" is the claim that sci-

ence strives for transparency while the humanities seem to be striving for opacity, with dense, self-important academic blather, often in the name of "deconstruction" or "postmodernism," rather than the lucid, comprehensible prose that characterizes science. Indeed (this position continues), the very goal of science is to be as widely understood as possible, while these postmodern humanists are terrible writers, merely using gobbledygook to cover up the fact that they have nothing to say.

I suppose that some humanists indeed have nothing to say but need to say something in order to keep their paychecks coming. But that situation is not much different in science.

And just how lucid is the prose in science, anyway? There is certainly very good science writing and very bad science writing. But what is the middle like?

Here's what I think: The writing, on average, is probably better in humanistic fields than in scientific fields. Why? For a simple reason: scholars in humanistic fields have been subject to a lot more intensive formal training in writing than scholars in scientific fields have. It's part of their curriculum.

In a book called *Fashionable Nonsense,* that mathematics wag Alan Sokal returns with a French physicist as coauthor to call attention to the incomprehensibility that characterizes the writing of some humanists and is taken seriously by others.[18] The fact that some of this work is difficult to comprehend (and, to make things worse, some of it is actually translated from French) is, however, a red herring. The real issue is whether there are some useful ideas behind the work.

Once again, some comparative perspective may be useful. Isaac Newton's 1687 *Principia,* which helped to frame modern science, was (and remains) incomprehensible to all but a very few readers. A famous story holds that a Cambridge student, passing Newton on campus, told a friend, "There goes the man that writt a book that neither he nor any body else understands."[19] Nor was that an accident. Newton later remarked that he had made his work unintelligible deliberately "to avoid being baited by little smatterers in mathematicks"[20]—that is to say, by those very people who now man the front lines of the "science wars."

None of this is intended either to demean Newton or to assert some

sort of equivalence between the seventeenth-century physicist and the twentieth-century literary critics. All I want to show is that obscure writing is by no means the exclusive domain of contemporary deconstructionists or postmodernists. Needless abstruseness has made a home in science from the beginning.

Nor is there any evidence that the situation is improving. While there are well-known public complaints about the poverty of academic writing in general, there are at least as many bemoaning the poverty of writing specifically in the sciences. As an essay in *Nature* explained, "Pleas for scientists to write readably have failed for at least 300 years." What reason do we have for thinking the future of scientific prose looks any brighter?

> Everyone can write, so it is assumed that writing is easy, or unimportant. Everyone can paint as well, but not everyone's paintings are worth hanging on walls. To expect scientists to produce readable work without any training, and without any reward for success or retribution for failure, is like expecting us to play violins without teachers or to observe speed limits without policemen. Some may do it, but most won't or can't.[21]

In his farewell editorial after a quarter-century of editing the leading science journal in the world, Sir John Maddox was hardly one to mince words, but he could only speculate on the cause of the problem he observed:

> It used to seem that Nature's contributors wrote clearly, but no longer. . . . The obscurity of the literature now is so marked that one can only believe it to be deliberate. Do people hide their meaning from insecurity, for fear of being found out or, in the belief that what they have to say is important, to hide the meaning from other people?[22]

The latter choice, of course, was Newton's—although the journal *Nature* was not yet in existence when Newton lived—but he obviously was a precedent setter. And it's hard to deny the editor of *Nature;* there are no doubt plenty of insecure contributors, and some contributors afraid to be found out, as well.

Sometimes you can even forget delving into the prose itself, for you can't even get past the title. From a random issue of the prestigious *Proceedings of the National Academy of Sciences,* you can easily find a title like "*In vitro* assembly of the undecaprenylpyrophosphate-linked heptasaccharide for prokaryotic N-linked glycosylation," as evocative of James Joyce as you could hope for, albeit certainly unintentionally.

I pulled that one out of the current issue. Another article in the same issue is titled "Giant-block twist grain boundary smectic phases." I know what most of the words mean (except "smectic," which sounds vaguely scatological but actually has something to do with the arrangement of molecules in a liquid crystal). Together, however, they sound like nonsense, with too damn many nouns in a row *(block, twist, grain, boundary).* In combination they sound almost like "Colorless green ideas sleep furiously"—the linguist Noam Chomsky's famous example of a sentence that is recognizably English in spite of being nonsensical.

From the same issue, we can find a stylistic device commonly used in the humanities as well: the two-part title, divided by a colon. In the humanities, this is generally structured as something cute, colon, then something explanatory. In the *PNAS,* however, we can find nothing either cute or explanatory in "Surface-mounted altitudinal molecular rotors in alternating electric field: Single-molecule parametric oscillator molecular dynamics." (I double-checked to see if there is an article missing before "alternating." There isn't.)

And finally, we encounter the newest trend in science titles, the declarative sentence in lieu of the topic: "C-type natriuretic peptide inhibits leukocyte recruitment and platelet-leukocyte interactions via suppression of P-selectin expression." Since you now know the conclusion, the authors seem to be saying, you don't even have to bother reading the article itself; we're sure you have better things to do with your time.

No, far from being a transparent, accessible, universal literary genre, the scientific literature is for the most part as dense and impenetrable as a Mayan codex, and certainly no less so than the humanities literature, postmodern or not. Moreover, it has its own stylistic rules and literary conventions, in some cases so at odds with actual practice that the

immunologist Peter Medawar once famously pronounced the scientific paper *as a genre* to be fraudulent (see chapter 4).

TOWARD AN ANTHROPOLOGY OF SCIENCE

Let us adopt the relativistic position that C.P. Snow suggested half a century ago: science is an anthropological "culture"[23] and, by implication, can therefore most profitably be understood using anthropological methods, conceptual frameworks, and analyses.

At the famous Scopes trial (see chapter 5), the attorney for science asked the attorney for religion a set of questions designed to show the latter's ignorance. One question was the number of people alive at the time of Christ. The attorney for religion had never given it any thought. When pressed, he finally said, "When you display my ignorance, could you not give me the facts so I would not be ignorant any longer?" The attorney for science answered him sharply, "You know, some of us might get the facts and still be ignorant."

This gets to the very heart of science. Science is a method, a way to knowledge, a path to enlightenment. Facts are great, but they don't constitute science; they are merely its many endpoints. Science is how we get facts, not the facts themselves. You can know a lot of them yet still be ignorant or unscientific.

This raises a fundamental question about science education. If science is a process of knowledge production, then is science education best expressed as teaching students the process or as teaching them the knowledge itself? If we focus on teaching students the accumulated knowledge, the facts of science, then we are not actually teaching them science. Rather, we are teaching them science's products, and indeed we are misleading them by substituting the teaching of scientific facts, as if it were the teaching of science itself.

Consequently, beware of people who complain about this generation's lack of "science literacy." The kids who don't know the difference between fluorine, chlorine, and schmorine are no worse off than the ones who think Rodin is a Japanese movie monster and that Plato's most

lasting contribution is the children's modeling clay which now bears his name. In fact there is probably a large overlap between the kids who are illiterate about science and those who are illiterate about anything else.

All right, there are a lot of people out there who don't know, or don't believe, what you want them to. Is it worse that they don't know whether the Axis won or lost World War II or that they don't know the difference between a muon and a gluon?

Different people know different things. If you don't like what they know, then it stands to reason that the solution is a massive campaign of indoctrination, or evangelization. By that token, though, science will have devolved into an ideology, or a set of beliefs, requiring something like a Nicene Creed to proclaim one's adherence to. The depth of one's knowledge would be a measure of the depth of one's immersion into the faith, and consequently the minutiae of the faith would begin to assume a disproportionate role—thus, the stereotypical science teacher obsessed with the minutiae that Robert Benchley satirized as "The Sex Life of the Polyp," back in the 1920s.

The alternative is not to worry about science literacy, except as an expression of general ignorance. As C.P. Snow originally observed, scientists know their stuff, and humanists know their stuff (to which we might add, the people that know *neither* may well be able to fix the scientist's and the humanist's broken transmission).

Instead, however, let us focus on science as a method of knowledge production. Then learning science is not principally about learning *what* scientists think but *how* scientists think. If science is method, then let us understand how the method works—how it is that science does come to tell us what the physical universe really is like, either because of, or in spite of, its practitioners—and why it is important for us to know what the universe really is like in the first place. There are, after all, other things worth knowing: good from evil, for instance. Legal from illegal. Sublime from vulgar. Gothic from Romanesque.

The point is not that scientists are stupid, which of course most are not; nor that there is not an external reality, which of course there is; nor that science is not the best way of finding it, in which it has achieved considerable success; nor that science is not important, which it manifestly

is. The intelligence of scientists, the existence of reality, the methods of assessing that reality, and the importance of doing so do not require defense or justification.

What an anthropology of science raises are more down-to-earth issues. How is scientific knowledge produced? How is science different from other cultural systems that produce knowledge? Can you believe everything a scientist says? If not, why not? How can you tell science from stuff that is not science? How can you tell good science from bad? What constitutes scientific practice—the activities of information gathering, social interaction, and ratiocination—that result in scientific knowledge? What counts as acceptable practice, and why? How does science impinge upon daily life, and how do people adapt to it? How is science absorbed, performed, utilized, and administered in particular political economic contexts? How and why would people resist science? And, from a practical standpoint, are there intellectual areas in which the training of today's scientists could stand some improvement?[24]