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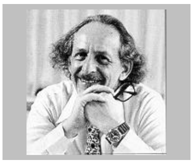

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How Science Works: David L. Hull Reviews *The Scientific Attitude* by Fred Grinnell

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Introduction

Book reviews are not usually featured in *Current Contents*® (*CC*®). The last was I.B. Cohen's review of Bernard Barber's *Social Studies of Science*.[1,2] In the following essay, David L. Hull, an eminent science philosopher at Northwestern University, Evanston, Illinois, reviews the second edition of *The Scientific Attitude* by Frederick Grinnell, University of Texas Southwestern Medical Center (UT Southwestern), Dallas.[3]

The book is an excellent introduction for students to the *practice* of science. As John Ziman's review put it, it is "nothing less than a systematic effort to describe the social institutions of contemporary science—graduate schools, research groups, specialist journals, review panels, and all the rest...."[4] It is also a refreshing reminder to veteran researchers of the professional habits and pitfalls of their careers.

I first became aware of Grinnell's book when he sent me a manuscript in 1985. He explained it was based on a course in the philosophy and sociology of science he taught for graduate biomedical students. It was interesting because it addressed topics previously commented on in these pages—the emergence of research fronts or "thought collectives,"[5] science ethics,[6] science and religion,[7] science education,[8] and so on.

The book was published in 1988 by Westview and received very favorable reviews in the *Times Higher Education Supplement*,[4] *The Scientist*,[9] and *Cell*.[10] The second revised edition, which now includes a chapter on scientific misconduct, was recently published by Guilford.[11] Hull reviews this edition in the following essay.

Frederick Grinnell

The Pros and Cons of Citation Analysis

As stated earlier, Hull is a philosophy professor at Northwestern. By citation criteria, his classic publication is a 1976 paper in *Systematic Zoology* entitled "Are species really individuals?"[12] It has been cited over 100 times through 1991 in the *Science Citation Index*® (*SCI*®) and *Social Sciences Citation Index*® (*SSCI*®).

Grinnell is a professor of cell biology who is also active in the philosophy and sociology of science. His most-cited work, "Cellular adhesiveness and extracellular substrata," was published in 1978 in the *International Review of Cytology*.[13] It has been cited about 520 times through 1991 in the *SCI*. His *Citation Classic*® commentary on the paper appeared in *CC* in 1990.[14]

Grinnell and Hull would appreciate the references to their impact since both are proponents of citation analysis. For example, in the reviewed book Grinnell suggests several interesting applications for evaluating research papers, "the singular measure for success" in science.[11] (p. 72) These include studying why a large portion of published papers are never cited, identifying cases of delayed recognition or premature discovery, mapping the emergence of "thought styles," and defining the journals serving thought "collectives."[11] (p. 83-4)

Hull takes a more provocative approach on citation analysis. In *Science as a Process*, he considers well-known bibliometric laws of concentration and asks, "If 95 percent of all citations are to works...by 5 percent of the practicing scientists, why waste so much money supporting all those third-raters?"[15] (p. 158) Based on similar data he states, "In the face of [these] figures...it is difficult not to conclude that publishing a paper is roughly equivalent to throwing it away."[15] (p. 360) Hull's motive in drawing such extreme conclusions is to provoke scientists to think about, understand, and explain citation patterns among their colleagues.

About the Authors

Fred Grinnell has been professor of cell biology at UT Southwestern since 1981, where he teaches four graduate courses. He earned a PhD in biochemistry from Tufts University Medical School in 1970, and has published over 100 papers on cell adhesion and wound healing. He serves on the editorial boards of *Experimental Cell Research* and *Wound Repair and Regeneration*, and is a member of the National Institutes of Health Cell Biology and Physiology (II) Study Section.

David Hull is the Dressler Professor in Humanities at Northwestern and past president of the Philosophy of Science Association, International Society for the History, Philosophy and Social Studies of Biology, and Society of Systematic Zoology. His 1964 PhD from Indiana University was in the history and philosophy of science. The author of about 100 papers and books on taxonomic philosophy, Darwinism, and topics in the philosophy of biology, he currently serves on the editorial boards of *Acta Biotheoretica, Biology & Philosophy, Evolutionary Theory, Philosophy of Science*, and other journals.

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A Review of *The Scientific Attitude, Second Edition.* New York: Guilford Press, 1992. 180 p. $16.95. by Frederick Grinnell

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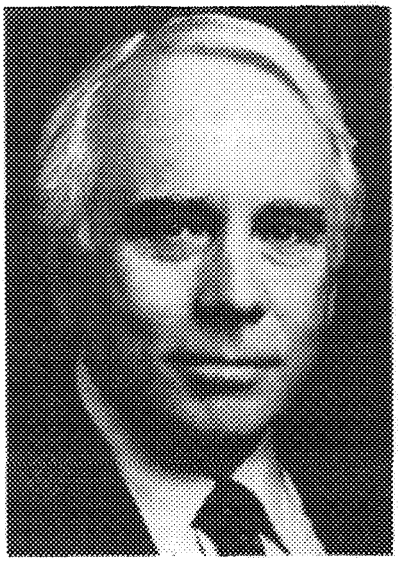
Once a critical mass of scientists was reached in the 17th century, science increasingly became an apprentice system. Young scientists came to learn what a particular area of science is all about by working closely with an established scientist for a number of years. Some of what young scientists acquire is taught to them explicitly, but much of what they learn is not. Instead they internalize the behavior and attitudes of their mentors. If they see more senior scientists check all their figures with great care before returning page proofs, they are likely to do the same. If to the contrary their mentors show a cavalier attitude toward accuracy, they may pick up that same attitude.

But these were the good old days. Since at least World War II, science has become increasingly a matter of large research teams and institutes. Instead of senior scientists working with two or three more junior scientists, they may well head a lab of two or three dozen workers, including graduate students, postdocs, junior scientists, lab technicians, and secretaries. Senior scientists are likely to spend more time writing research proposals and dealing with the administration and maintenance people of their universities than interacting with their younger colleagues. Just as there is no action at a distance in physics, the tacit knowledge imparted in the apprentice system requires extensive personal contact.

Currently science is receiving considerable attention from government committees, the news media, and students of science. The claim is that the old norms that used to govern the conduct of scientists are not very effective any more. Or possibly



David Hull

they never were very effective. If it is true that scientists are misbehaving more frequently now than in the past, one explanation may well turn on the rapid growth of science. Too few senior scientists are available to serve as mentors for larger and larger numbers of young scientists. Because of this imbalance the sort of tacit knowledge necessary for scientists to behave properly is not being imparted.

One response to this situation is to make the largely implicit norms of particular areas of science more available by making them explicit. Such codifications can never totally replace learning firsthand by example, but they can help. A second reason for attempting to explain how science works is that to survive young scientists need this knowledge. In easier times, when research funds were more plentiful, young scientists could afford to bumble their way through the early years of their careers, picking up what they needed to know catch-as-catchcan, but in the hard times that currently prevail, such informal methods are not good enough. Like it or not, young scientists must pay greater attention to the ways in which science actually works. Grinnell's *The Scientific Attitude* is designed to do just that.[1]

Grinnell has written a user-friendly book. He has read extensively in the history, philosophy, and social studies of science and gives his readers the benefit of his labors minus the tedium. He pays special attention to the social structure of science without committing himself to the relativist epistemology that usually accompanies it. Science is a social process. What else could it be? But it does not follow that science is nothing but a social process. Consensus is important in science, but it matters how this consensus is reached. Consensus can be reached and maintained in many ways—by propaganda, brainwashing, authority, and thumbscrews. All of these methods have been used successfully at one time or another, but it is the prevalence of other methods in science that has resulted in scientists producing dependable, credible knowledge.

Grinnell shows how these methods depend on the existence of what he terms "thought collectives"—groups of scientists who share the same fundamental thought style, who are aware of each other's work, who scrutinize it, using some results, questioning others. As individuals, scientists may be more objective than run-of-the-mill people, but the sort of objectivity that makes science work the way that it does is a group characteristic.

Grinnell begins his discussion of observation and discovery from the perspective of individual scientists. From my experience, nearly all science majors enter science, as Grinnell himself did, convinced inductivists. They think that good scientists should begin their investigations in a totally theory-free way, sticking with the facts and nothing but the facts. Once all the facts are in, then scientists can venture theoretical explanations of these facts, but theories remain suspect. They come and go. Facts, like true love, are forever. Grinnell shows how distorted this view of science is. Few scientists have actually conducted their research inductively, and the ones that approach this "ideal" tend to have minimal impact on science. Like it or not, our general understanding of the world influences even our most empirical investigations. Even observation is theory-laden.

Grinnell substantiates his position on observation and discovery by reference to both traditional philosophical arguments and psychological data. In general, Grinnell makes greater use of the sort of data about science currently being generated than do most students of science. By necessity scientists conceptualize the world in terms of categories such as mass, velocity, and photosynthesis, and these concepts influence to some extent what they see. Usually the constraints imposed by their thought style facilitate their research. They have some reason to run one experiment rather than indefinitely many others. On occasion, however, the constraints imposed by a particular thought style can hurt by closing off what turns out to be a promising avenue of research.

Grinnell illustrates his position by reference to the cell concept. Scientists never perceive an "ideal" cell, but through the observation of numerous different sorts of cells, they do develop an idealized concept of the cell, the sort of conception that is illustrated schematically in textbooks. Once they have internalized this idealized concept of the cell, it will unavoidably influence later observations, usually in ways that facilitate our understanding of cells, occasionally in ways that impede it. Because Grinnell himself is a cellular biologist, his discussion of this example is livelier than comparable discussions in other introductory texts.

A scientist must begin his/her investigations somewhere, and among the factors that influence such decisions are those peculiar to the individual scientist, but scientists also work in consort with other scientists in concentric and overlapping circles of professional relationships. Most narrowly, scientists form small research groups in which they pool their conceptual resources. One scientist might have a well-appointed lab and access to research funds, another might have opened up what appears to be a very promising line of research, another is mathematically sophisticated, another has good hands, and so on. By working together scientists can make greater joint contributions to the growth of scientific knowledge than any one of them could make working alone.

One fact about such research groups that is sure to put off budding young scientists is that each "member of the laboratory is an employee of the senior investigator."[1] (p. 63) Research scientists in a university do not like to think of themselves as employees, but until they attain the status of senior investigator, that is what they are.

The social structure of science is more extensive than just isolated research groups. These groups in turn are organized into more extensive communities, for example all the research groups working on cell adhesion. At this level the relationships get more complicated because the problems on which groups work are related in more than one way. One group working on cellular adhesion might be interested in the molecular structure of the cell membrane, which automatically situates them in another matrix of relationships. From the outside such relationships seem dizzyingly complicated, but the people working in them know their way around.

One of Grinnell's most important theses is that "in the final analysis, what makes the observations and experiments of one investigator scientific is their acceptance by others."[1] (p. 45) The surest sign of objectivity is intersubjectivity. If other scientists can replicate your results by the methods you used, then these results are likely to be dependable. Students of science have been surprised to discover how rarely scientists actually set about replicating the work done by other scientists. Most work is ignored. Of the work that is noticed, most is merely accepted without testing. Only when things go wrong or the results challenge other widely accepted beliefs do scientists feel called upon to replicate previous work. In science a little bit of replication seems to go a very long way.

One of Grinnell's major strengths is that he emphasizes the social nature of science without succumbing to relativism, but just as importantly he walks students through the stages of a scientific career. Those of us in academia already know how one goes from being an assistant professor, to an associate professor with tenure, to a full professor. Of course everyone knows what tenure is. How could they not know? But lots of people, including science majors, do not know. I once sat on a committee hearing an appeal of a tenure decision in which the court reporter throughout her transcript of the proceedings transcribed "tenure" as "ten year." Apparently she had never heard of it before.

Grinnell begins his discussion of how students become successful scientists with graduate school. The choice of a graduate program is crucial. I happened to go to a graduate program that turned out to be very influential, but my choice was sheer luck. Grinnell details the sorts of considerations that should go into the choice of a program: the number of graduate students, the ratio of faculty to students, the reputation of the department as well as of individual professors, the availability of research support, publication record, etc. Not included on this list are such things as the amount of ivy on the walls or the proximity of the university to theaters, opera, or even good beaches.

Grinnell also emphasizes the importance of scientific lineages. Most successful scientists were trained by successful scientists. Part of the influence of lineages is intrinsic: Good scientists working on the frontiers of science are likely to have access to the best students and provide the best training for the next generation of scientists. But part of the influence of lineages is that well-placed scientists are in a position to get good jobs for their students even if they do not happen to be all that good.

How to choose a dissertation topic? One common answer is to choose a problem that you personally find interesting. Certainly graduate students are well advised to choose a topic that they find or think that they will come to find interesting. They are going to spend a large chunk of their lives on it. But they need to take other considerations into account as well. For example, they might pay some attention to the issues that their most prestigious professor finds interesting. Big names are very busy. They need reasons for giving students time, and the best reason is that a student is working on a problem that bears on their own research.

While working on a PhD, students must also publish, usually as junior investigators on some of their advisor's papers. Grinnell explains how papers are written, submitted to journals, refereed, modified, and eventually published. Most importantly he distinguishes between two sorts of experiments—heuristic and demonstrative. Heuristic experiments are those that are performed to discover new information. They are frequently incomplete, inconclusive, and sometimes outright failures. They help researchers to learn the lay of the land so that they can finally design a demonstrative experiment, one that will not only work but also convince other scientists.

After graduate school, most young scientists sign on as postdocs for a couple of years. The chief benefit of a postdoctoral fellowship is that the young researcher can work full-time on research without having to teach. The danger is that the postdoc becomes nothing more than a glorified lab assistant. When young scientists finally get their first tenure-track job, the need to teach is joined by the need to obtain funding. Young scientists find the funding process among the most mysterious of all the rituals that they must learn to master. Grinnell does an excellent job of explaining how at least one funding agency, the National Institutes of Health (NIH), has worked in the recent past. However, if Healy (1992)[2] has her way, he may have to rewrite this discussion.

All of the preceding concerns how science works when it is working well. But every once in a while, no one knows how often, scientists misbehave. The sort of misbehavior that receives the most attention outside of science is fraud, instances in which a scientist claims to have run experiments, made observations, or obtained results that are purely fictitious. These results need not be mistaken, but usually they are. The important point is that the deception was intentional. Scientists publish lots of results that turn out to be mistaken, but they do so unintentionally. In this connection scientists distinguish between honest error and inexcusable sloppiness.

One reason that scientists are made uneasy by all the attention that they are receiving from congressional committees and talk about "science courts" is that error is at the heart of science. Certainly scientists want to eliminate errors when they find them, but if too much emphasis is placed on not letting any errors at all sneak into the scientific literature, science is likely to grind to a halt. Pedestrian research can be made all but error free. Highly innovative work is just as likely to include some mistakes. Scientists fear that outsiders do not appreciate this fact about science.

As shocking as cases of outright fraud in science are, most scientists are convinced that the effects of sloppiness are much more serious than the effects of fraud because sloppiness is so much more prevalent than fraud. Grinnell agrees that, from the perspective of the progress of science, sloppiness is more serious than fraud, but he finds this perspective too limited. "For those most concerned with protecting patients and guarding the public trust, these other consequences may overshadow scientific progress *per se*."[1] (p. 116)

As Grinnell notes, the legal definition of fraud requires both intention and actual damages, while the regulations established by the NIH and National Science Foundation depend solely on the intentions of the investigator. I happen to think that too often we overemphasize the importance of the distinction between intentional and unintentional behavior and underemphasize the effects of the behavior. For example, in most legal systems, if one person intentionally kills another, that is very serious and is often punished severely. However, if a person intentionally gets drunk, gets in a car, and unintentionally kills someone else, that is not nearly so serious. In the US at least, first-time offenders are usually let off with a reprimand or suspended sentence, especially if they are solid citizens. Repeat offenders may spend a couple of years in prison. The victims in both cases are, however, equally dead. Intentions matter but so should effects, and mistakes that find their way into the literature have the same effects on the work of anyone who uses them regardless of whether or not they were introduced intentionally.

Another problem with the central role that intentions play in deciding fraud is that they are so difficult to ascertain, and scientists themselves are really not that well equipped to make such decisions. It is difficult enough to decide whether or not a particular finding is mistaken without having to decide whether the author presented the erroneous result intentionally. For example, other scientists are in a position to discover if the results claimed by Baltimore and Imanishi-Kari can be replicated. It took the FBI to raise serious questions of outright fraud. Grinnell tells of a series of experiments that he performed in 1967 that he could not replicate in 1968. Because he has access to his own intentions, he knows that he did not commit fraud. He really ran the experiments. Other scientists would have a much harder time deciding.

Plagiary and failure to give adequate credit is a second distinct area in which scientists, as well as all academics, can misbehave. Scientists are supposed to use each other's ideas. Only in that way can science be as cumulative as it is. But scientists are also supposed to give explicit acknowledgment of the work that they use. Plagiary is the unacknowledged use of not only other people's work but also their very words. As might be expected, people accused of plagiary plead that they may have quoted verbatim several paragraphs from another work without the benefit of quotation marks, but it was all unintentional. They are cursed with a photographic memory.

It is easy enough to decide whether the words in two publications are the same; much harder to decide whether a later author intended to pass off someone else's words as his/her own. Perhaps the quotation marks were omitted unintentionally. It does happen. I myself have never understood why anyone would bother to use another person's words when paraphrasing is so easy. Besides, I never read anything that could not use at least a bit of stylistic improvement.

But once again, failure to give adequate credit is not nearly as damaging to science as is sloppiness. The author whose work has not been given adequate credit may be unhappy, but if the view is correct, the fact that its author was not given due credit does not invalidate the work of those people who use it. Besides, as the controversy between Gallo and Montagnier shows, it is not always easy to decide who really deserves the credit. Grinnell is worried about the trust that the general public has in science and scientists, but ordinary people are more than puzzled by the amount of time and money that have been spent trying to decide who really discovered the AIDS virus when millions of people are threatened by death from the virus.

Self-interest drives scientists the way that it does all people. If in general scientists did not get credit for their contributions, science would be seriously damaged. Occasional inaccuracies damage the careers of individual scientists, but such departures from the norms of proper scientific behavior do much less damage to scientific progress than does sloppiness. In his own lab, Grinnell demands that his students write everything in a bound laboratory notebook, not on loose pieces of paper. Perhaps the finished research paper is to some extent a sanitized fiction, but Grinnell insists that research notebooks be accurate records of the research that was actually done—warts and all. In this respect he differs from Baltimore, who was willing to excuse Imanishi-Kari's admitted sloppiness.

Finally, Grinnell addresses the question of secrecy and conflict of interest in science. In the early stages of research, scientists tend to play their cards close to their chests as they ready their work for publication. That way they increase the likelihood that they will get credit for their contributions. In addition, if they have discovered a new procedure, reagent, virus, etc., they think that they deserve first shot at development but at the risk of being scooped if they wait too long to publish.

Publication is the primary way that scientists share their knowledge, but sometimes the sharing is a good deal more literal. According to the accepted standards in virology, Montagnier shared his viral samples with Gallo. Grinnell discusses two instances in which other investigators in his area refused to send him samples of their antibodies. In such matters some self-regulation is possible. The next time either of these two investigators request samples from Grinnell, he might well remind them of their previous miserliness, and certainly Montagnier will think twice before sharing any future isolates with Gallo.

Problems of secrecy in science are only exacerbated when money enters the picture. The dispute between Gallo and Montagnier was not just about credit but also about who was to profit financially from the antibody test for AIDS. Most discussions of the nature of science concern research occurring in a university setting. This is the thought style that constrains our discussions. Few research scientists are making discoveries that hold out much hope for monetary reward, but even in those cases in which money is to be made off a discovery, the researchers themselves don't see much of it. When scientists work for industry, profits are of primary concern. They are required to work on problems that hold out some hope of application. As in the case of university-based research, however, they are not the ones who will make the millions. But increasingly, scientists themselves are taking over the business end of science by forming their own companies. In such circumstances, the desire to make money is added to the other considerations that motivate scientists. Much of science can be explained in terms of credit for contributions. To that is now being added cash for contributions. Grinnell is understandably worried.

One of the main messages of Grinnell's book is that science has varied through time. Levels of experimental care that were considered acceptable at one time may no longer be tolerated a generation later. Science also varies from one area to another at any one time. I was amazed to discover that certain social science journals publish papers without bothering to have them refereed. Anyone who will pay page charges gets published. Such a practice is certainly unacceptable in those areas of science with which I am familiar. And this is one of the difficulties in attempting to explain to students what precisely the scientific attitude is. It is as variable as different types of cells. The conception of the ideal cell obscures all this variation. Most discussions of science also disguise how variable it is. Grinnell is exceptional in that he not only acknowledges this variability but also reflects much of it in his discussions. I wish that I had read a book like Grinnell's before I entered graduate school. It should be required reading for anyone who plans a life in science.

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