Opinion Page

Teaching research integrity – Using history and philosophy of science to introduce ideas about the ambiguity of research practice

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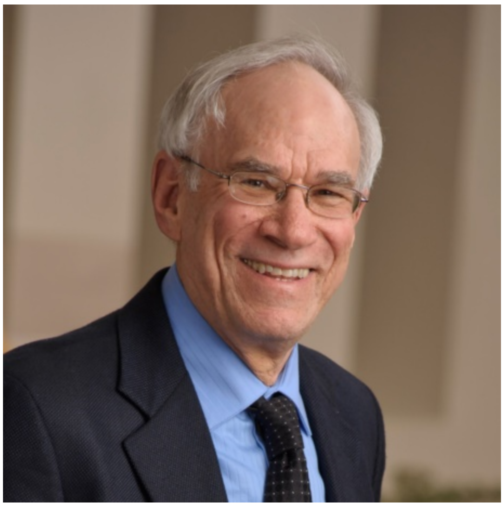

Having worked in science and philosophy of science for 50 years, I can say that in my experience these two disciplines fit perfectly the stereotype of two cultures. Mostly, they don't talk to each other. Trying to do interdisciplinary work has been an ongoing challenge. Ironically, the best opportunity that I have had to share ideas from history and philosophy of science (HPS) with science students (in graduate school and medical school) is when I teach them about research integrity. I use examples from HPS to emphasize the ambiguities inherent in everyday practice of science. Achieving research integrity requires learning to recognize and manage those ambiguities.

How did I get this opportunity?

My introduction to research integrity in science began in 1966 shortly after entering graduate school in the biochemistry department at Tufts University School of Medicine. We were told that one of the members of the external review group evaluating the department's graduate program had a former graduate student who had fabricated a series of experiments first as a graduate student and then as a postdoctoral fellow. In one series of experiments, the student claimed to have discovered a ribonucleic acid intermediate in the synthesis of the tripeptide glutathione. Our department chair Alton Meister was a leader in the glutathione field, and Meister did not believe the published work. Shortly after papers appeared retracting the fabricated findings, Meister published a sarcastic conference abstract formatted so that the first letter of each line made up an acrostic that read "no ribonucleic acid." The message was clear to us. If you commit fraud, you are going to get caught. And if someone in your lab commits fraud, you are going to be very embarrassed. Don't do it, and don't let it happen to you. The scientific community was committed to research integrity. At the time, however, aberrations were managed as internal family matters.

After doing postdoctoral work, I developed my own research laboratory in the cell biology department at UT Southwestern Medical Center. In addition to my research work, I had started teaching a philosophy of science course to our graduate students using an unlikely combination of writings by William James, Ludwik Fleck, Alfred Schutz, and David Hull. In 1987, I published my first book about the philosophy and sociology of science called *The Scientific Attitude* (Westview Press, 1987), which was based on my course. Although research integrity was the subject of a 1981 Congressional hearing entitled *Fraud in science*, the topic still was perceived as an internal matter by the science community. In the first addition of *The Scientific Attitude*, I devoted only one and a half pages to fraud in science. Subsequently, things changed rapidly. During 1988-1990, research integrity became the focus of multiple governmental and science society meetings. From many points of view, the question arose: "Was misconduct threatening the health of the scientific enterprise?"

How to define research misconduct was controversial. In addition to fabrication (making up the data), falsification (changing the data to fit the hypothesis), and plagiarism, the National Institutes of Health proposed a definition that included the clause, "deception or other practices that seriously deviate from those that are commonly accepted within the scientific community for proposing, conducting or reporting research." The biomedical research community represented by the Federation of American Scientists for Experimental Biology rejected that clause

It is our view that this language is vague, and its inclusion could discourage unorthodox, novel, or highly innovative approaches which in the past have provided the impetus for major advances in science. It hardly needs pointing out that brilliant, creative, pioneering research deviates from that commonly accepted within the scientific community.

It was clear to me that ambiguity inherent in the practice of science was central to the controversy about what should count as misconduct. I argued for that idea first in a 1996 editorial in *Science Magazine* and frequently since.

After 1989, NIH made it a requirement for NIH supported graduate programs to teach principles of research integrity. Two years later, our dean asked me to convert my philosophy of science course for the cell biology graduate students into a research integrity introduction for all our graduate students. I agreed to do so. Also, I updated *The Scientific Attitude*. In the 2nd edition (Guilford Press, 1992), the one and a half pages devoted to fraud morphed into a full chapter about research misconduct and was the starting place for my research integrity lectures.

Introducing ambiguity

Science students and scientists in general rarely reflect on the philosophical ideas underpinning their work. To introduce ideas about ambiguity requires challenging them to engage with and re-examine their taken for granted assumptions about the nature of science. Now I like to offer students the following paradox. The 2010 Singapore Statement on Research Integrity calls for the principle *Honesty in all aspects of research*. But in a 1962 essay called *Is the scientific paper a fraud?*, Sir Peter

Medawar writes that research publications distort science and represent "a totally mistaken conception, even a travesty, of the nature of scientific thought." I suggest that both ideas are correct; how is that possible?

Science comes in three versions: textbook, linear model, and everyday practice. From the first two versions, one can learn a lot about the theory of science but not so much about what actually happens in the laboratory or the field. *Honesty in all aspects of research* reflects the linear model of science, which is typical of science education. According to the linear model, the path from hypothesis to discovery follows a direct line guided by objectivity and logic; facts are waiting to be observed and collection; researchers can be objective and dispassionate observers. *The scientific paper is a fraud* is part of everyday practice of science. Reading science autobiography such as Watson's *The Double Helix*, quickly makes it clear that the path to discovery is anything but linear, and that the researchers involved are anything but disinterested!

In what follows, I will describe what I think are some of the most important ambiguities that I share with my students to challenge their taken for granted assumptions about science. I present science to them making a distinction between what I call the two circles of science, discovery and credibility. Discovery emphasizes the cognitive features of science and includes not only what philosophers typically call discovery, but also justification. That is, if discoveries are not adequately justified, then they will not get past the peer review oversight process to become discovery claims made public. Credibility emphasizes the social features of science. Once a discovery claim becomes public, the credibility process begins and corresponds to the path by which discovery claims become discoveries and sometimes textbook facts.

Discovery

In everyday practice of science, the path to discovery is convoluted with lots of dead ends. Failure is frequent. Why is discovery so hard? The Greek philosopher Plato argued that discovery is not just difficult, but impossible. In the *Dialogues*, in response to Meno's question about discovery, Socrates responds…"that a man cannot search either for what he knows or for what he does not know. He cannot search for what he knows – since he knows it, there is no need to search – nor for what he does not know, for he does not know what to look for." In practice, discovery requires searching for what is beforehand unknown and not yet recognizable, but how is that possible I ask the students?

Plato's paradox captures the problem that every researcher encounters. Already known and expected knowledge can act as an impediment to discovery by constraining investigators from seeing and thinking anything more. Claude Bernard, one of the first experimental physiologists, emphasized that typical outcome in his 1865 classic, *An Introduction to the Study of Experimental Medicine*, "Men who have excessive faith in their theories or ideas are not only ill prepared for making discoveries, they also make very poor observations."

Research experiments can be divided into three categories: heuristic, demonstrative, and – most common – failed. Heuristic experiments offer researchers new insights into the problem under investigation. Demonstrative experiments re-work heuristic findings, if necessary, into a form suitable for making discovery claims public (i.e., adequate justification). Failed experiments arise when results are inconclusive or uninterpretable, which may occur for many reasons including technical errors, uncertain methods, or poor study design.

Given the extent of failed experiments, published papers typically contain only a small portion of the data collected. Ten notebooks frequently can be reduced to ten figures. Why is it ok, I ask the students, to be so selective and discard so much data? In addition to presenting only a selected set of data, research papers also typically rewrite history to present a logical and internally consistent account of the studies. Just as failed experiments are omitted, so will be failed hypotheses that have been discarded and older experiments at one time believed to be demonstrative but reinterpreted or discarded in light of later findings.

Francois Jacob in *The Statue Within: An Autobiography* (1988) notes that:

> Writing a paper is to substitute order for the disorder and agitation that

> animate life in the laboratory . . . To replace the real order of events and discoveries by what appears as the logical order, the one that should have been followed if the conclusions were known from the start.

Rather than the scientific adventure, the plot of research papers is the scientific method. The challenge is to make sure that one's papers are intellectually honest knowing that in an absolute sense, they are false!

Moreover, the pressure to produce is great. Deciding to study a particular research question takes it for granted that that prior research was somehow incomplete or incorrect; that adequate methodological, infrastructure, personnel, and financial resources are available in the laboratory to answer the question; and that finding a new answer will be worth the effort. Because resources of time, money and personnel are limiting, carrying out one project almost always means that something else will not be accomplished. Being wrong ultimately can result in failure in one's career aspirations as a scientist. Reflecting on the potential impact of failure helps students understand the difficulty of being objective and dispassionate in the way imagined by the linear model of science.

Experimental Design

Because the answer is not known in advance, carrying out an experiment requires guessing what will be the outcome. The guess becomes the basis for study design. Every experiment tests the investigator's explicit hypothesis about how things are/will turn out and at the same time the implicit hypotheses about adequacy of the methodology and design selected. Observing an answer different from that expected could be because the hypothesis is wrong, or the experimental design is wrong. Consequently, conclusions always will be potentially ambiguous. Don't give up a good hypothesis just because the data do not fit, at least not at first. Popper's ideas about falsification might be relevant to linear science, but in everyday practice of science the significance of falsifiability is aspirational, i.e., researchers being open to the possibility of being wrong.

Ironically, when an investigator's implicit assumptions about experimental design and methodology turn out to be incorrect, unexpected results sometimes provide important opportunities for discovery if the unexpected results are noticed as potentially useful. Max Delbrück facetiously attributed useful unintended experiments to the *principle of limited sloppiness* meaning lack of clarify (i.e., muddiness) about an experimental system – not carelessness. Charles Peirce had these sorts of experimental results in mind when he introduced abduction as the logic of discovery. (I find the conventional view of philosophers that abduction means inference to the best explanation much less interesting.)

Further complicating interpretation of experimental findings is that at the edge of discovery distinguishing data from noise rarely is clear-cut. Although heuristic principles can be helpful, an investigator's experience and intuition often will determine what counts and what does not. In any particular case, the way the results are selected by one investigator might appear arbitrary and self-serving to another– even an example of misconduct. In the 1984 Sigma Xi Research Society pamphlet *Honor in Science*, Robert Millikan's oil drop experiments are held up as an example of falsification. Several years later, when Sigma Xi awarded its annual Science and Society Award to physicist David Goodstein, Goodstein's award lecture presented a defense of Millikan focused on the definition of what should be counted as data.

Credibility

Once a discovery claim is made public, the credibility process can begin. Because researchers bring biography and personality to their work, i.e., what Fleck calls thought styles, discovery claims are inherently subjective. For a discovery claim to become a scientific discovery, the researcher must turn towards the larger community.

Making a discovery claim public allows individual researchers to transcend their own subjectivity through intersubjectivity. Intersubjectivity is at the base of all social interactions. If two individuals interchange places, then they will (sort of) see, hear, think similar things – what Alfred Schutz called reciprocity of perspectives. In science, intersubjectivity means that researchers will be able to verify and validate each others' work if it is correct. Through the credibility process, the in-

dividual researcher's existential me/here/now becomes the scientific community's anyone/anywhere/anytime. Objective knowledge is the goal, not the starting point. Paraphrasing William James' pragmatic conception of truth, "Credibility happens to a discovery. It becomes credible, is made credible by events." The community rather than the individual provides the source of objectivity in science.

And here is the final idea the I want students to consider. The more novel a discovery claim, the more likely it will challenge prevailing scientific beliefs, which instead of confirmation by reciprocity of perspectives can lead to skepticism and rejection. The history of Nobel Prizes includes many examples of novel discoveries that were either ignored or disputed for years. Albert Szent Györgyi characterized discovery as seeing what everybody else has seen and thinking what nobody else has thought, an idea captured in René Magritte's 1936 oil painting Perspicacity.

The painting shows the seated artist staring at a solitary egg on a draped table but painting a bird in full flight on the canvas. When skepticism leads one's research findings to be ignored or denigrated, success sometimes requires the individual to become an advocate – even a passionate advocate – for the work. How to become a passionate advocate for one's work and yet remain intellectually honest becomes the challenge. And in the end the community might be right.

Final Comment

If science really were a linear process based on logic and carried out by objective observers following the scientific method, then aspirational documents about research integrity along with courses based on theory of science would be sufficient for research integrity training. However, because practice of science is a more ambiguous enterprise, a more nuanced approach to research integrity education is required, one that acknowledges and makes explicit the ambiguities inherent in practice and the ethical challenges to which they give rise. Achieving research integrity requires creating a research environment that openly recognizes and engages these ethical challenges and makes explicit their sources. Accomplishing that goal in my opinion depends on introducing students to ideas from history and philosophy of science.

Further Reading

Grinnell, F. (1987,1992) *The Scientific Attitude*, 1st Edition, Westview Press, Boulder, Co.; 2nd Edition, Guilford Press, New York, NY.

Grinnell, F. (1996) Ambiguity in the practice of science. *Science*, 272:333.

Grinnell, F. (2009) *Everyday Practice of Science: Where Intuition and Passion Meet Objectivity and Logic*, Oxford University Press, New York, N.Y.

Grinnell, F. (2013) Research integrity and everyday practice of science. *Science and Engineering Ethics*. 9: 685-701.