Letters

I just finished re-reading Jay Labinger's review of Collins and Pinch's *The Golem* in the Fall *E&S*. While Labinger agrees with the authors that better sensitivity to how social factors affect scientific practice would be a good thing, in my opinion his review subtly distorts the book so as to make it seem quite a bit more extreme and polemic than it is. Whether or not "sociology of scientific knowledge" in general claims that knowledge is created by social factors, Collins and Pinch do not. Purposely or not, Labinger presents *The Golem* almost as part of the movement that claims that there is no truth, merely claims fought over by more or less powerful groups, a bit of blatant nonsense obviously antithetical to science. For example, he states that "in their view . . . choosing to favor Pons and Fleischmann's . . . results . . . can only be based, ultimately, on whether we believe in cold fusion. A dispassionate assessment of the experiments cannot be reached." This is almost a caricature of their detailed description of how the various scientific communities and subcommunities handled Pons and Fleischmann's reported results.

In a similar vein, Labinger appears to be saying that Collins and Pinch claim that it is never possible to assess the validity of an experiment without a priori acceptance of a theory. But they do not. Rather, they simply point out that when the appropriate range of outcomes of an experiment is not known in advance, some other criteria must, logically, be used to decide the validity of the experiment. These may be technical or nontechnical, and Collins and Pinch document several nontechnical reasons actually given by scientists for believing or disbelieving the results of various gravity-wave experiments.

The Golem seems to me to be presenting a much more reasonable picture of the actual doing of science, and as a practicing scientist I believe that scientists and the institution of science would be better off if more scientists read and understood it. Labinger's review would not lead many to read it, so I would like to present a different point of view.

If we simply examine the facts of what scientists do, not the theoretical or philosopical redescription of those facts, perhaps the most fundamental and obvious fact is that science is practiced by scientists. Scientists do experiments; scientists interpret experiments; scientists negotiate about how experiments ought to be interpreted; scientists agree that an experiment proves or disproves a theory. Saying "Experiment E proves theory T" is a shorthand description, albeit a useful one, one that leaves out the scientists, or the particular scientific community, that agrees that E proves T. This does *not* mean that theories cannot be verified, or disproved, or that all theories are equally valid or proper, etc. It is simply a reminder of the fact (and it is a fact, not a theory or an opinion) that the members of a particular scientific community agree, or disagree. Agreeing and disagreeing are done by persons, not theories.

Doing science, rather than something else, means committing one's self to negotiating about theories, experiments, and interpretations based on precision, rigor, and systematic investigation, rather than other things. It does not, and cannot, eliminate the necessity for judgment and skill. The exercise of that judgment and skill in no way invalidates the science. When Eddington chose to not use the Sobral results in evaluating the photos attempting to confirm general relativity (Collins and Pinch, p. 51), his behavior was not arbitrary, high-handed, or capricious; he was exercising his professional judgment that a "systematic error" had occurred. This kind of judgment is exercised in just about any experiment. No real data falls perfectly on a mathematical curve. "Experimental error" is a universally used concept, and a key part of a scientist's training is learning where, when, and how to use it.

When someone presents a result that, if accepted, would imply a tremendous fundamental change in basic theories, scientists quite naturally and appropriately seek to explain the results in another way, not for any of the illegitimate reasons often ascribed to them but simply because they are doing what scientists do: seeking the most parsimonious account of all the facts. Questioning whether the procedure reported was actually what was done, and whether the experimenter has the necessary kind and degree of skills, is an appropriate search for an explanation of the facts. Sometimes, perhaps due to lack of detailed information (as in the case of Pons and Fleischmann) it is impossible to say what went wrong, but in the judgment of respected members of that scientific community "something must have," and in this case the standing and credibility of those presenting the unusual results become important. None of this is illegitimate, inappropriate, or unscientific. It is simply how groups of human beings negotiate differences. It only seems to conflict with "the scientific method" because we are so used to language such as "this experiment proves conclusively that X" that we have taken it literally, and confused this partial description of the facts with the facts themselves.

The Golem is basically a depiction of this phenomenon, and a detailed reminder of the situation described above. I would recommend it to anyone, scientist or not, who wants or needs a better understanding of what science is, and is not.

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Jay Labinger responds:

I agree with almost everything Jeffrey says: 1) *The Golem* effectively demonstrates that doing science is intimately bound up with social activities; 2) it would be very valuable for scientists to read it (I thought I said so in the review!); and 3) my review might well leave the impression that Collins and Pinch take an "extreme and polemic" position. I would only disagree that the last is in any way a distortion, and that they do indeed claim that knowledge is created by social factors. Page 138: "Science works the way it does, not because of any absolute constraint from Nature, but because we make our science the way that we do." Still stronger versions appear in Collins's earlier work: "explanations should be developed within the assumption that the real world does not affect what the scientist believes about it . . ." and "The natural world in no way constrains what is believed to be."

Jeffrey feels that scientists should pay attention to the issues that Collins and Pinch and other science observers address, as that would benefit the practice of science. I would go further and argue that there is a huge agenda that begs for collaboration between scientists and science observers. Why isn't that happening? Pinch wonders elsewhere (in connection with cold fusion), "Despite all our work and understanding of controversies, what has our input been? Zilch. Our message is clearly not getting through, and that is the most depressing thing of all." I suggest that their emphasis on how much social factors determine knowledge would strike most scientists as a severely distorted picture of how science really works. The barrier that keeps their message from getting through is one they have done much to help build. It is ironic that Collins and Pinch and their colleagues place so much weight on the roles of negotiation and consensus-building within science, yet seem to have little interest in moderating their own positions in order to enlist scientists in a true dialogue. If my review accentuated the negative a bit too much, chalk it up to the hope of encouraging *both* sides to move.