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## THE ROLE OF TRUST IN KNOWLEDGE\*

The whole fabric of research is trust.

Elizabeth Neufeld

It seems paradoxical that scientific research, in many ways one of the most questioning and skeptical of human activities, should be dependent on personal trust. But the fact is that without trust the research enterprise could not function. . . . Research is a collegial activity that requires its practitioners to trust the integrity of their colleagues.

Arnold S. Relman

We do not normally notice the air we breathe. Similarly, we epistemologists have not noticed the climate of trust that is required—or so I shall argue—to support much of our knowledge. Thus, the title of this paper may seem strange, for most epistemologists and philosophers of science see no role for trust in knowledge. Although epistemologists debate various theories of knowledge, almost all seem united in the supposition that knowledge rests on evidence, not trust. After all, trust, in order to be trust, must be at least partially blind. And how can knowledge be blind? Thus, for most epistemologists, it is not only that trust plays no role in knowing; trusting and knowing are deeply antithetical. We can not know by trusting in the opinions of others; we may have to trust those opinions when we do not know.

I shall argue that this is badly mistaken. Modern knowers cannot be independent and self-reliant, not even in their own fields of specialization. In most disciplines, those who do not trust cannot know;

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those who do not trust cannot have the best evidence for their beliefs. In an important sense, then, trust is often epistemologically even more basic than empirical data or logical arguments: the data and the argument are available only through trust. If the metaphor of foundation is still useful, the trustworthiness of members of epistemic communities is the ultimate foundation for much of our knowledge.

I think my argument is applicable to many areas of knowledge. I shall take science and mathematics as my paradigms, however, for they have provided the primary models of knowing for Western epistemology for the last 350 years. I shall attempt to show how and why trust is essential to scientists and mathematicians, and assume that, if I can show this, most epistemologists will agree that we must make room in our epistemologies for trust.

The conclusion that much of our knowledge rests on trust will, I believe, have far-reaching implications. It may force basic changes in epistemology and the philosophy of science. But it is worth emphasizing that I am not here proposing a new epistemology, nor endorsing a "nonstandard" analysis, such as that of Lorraine Code,<sup>1</sup> Michael Welbourne,<sup>2</sup> or the "strong programme" of the Science Studies Unit at Edinburgh. Quite the opposite: I wish to address my argument to as many philosophers as possible. I therefore aim to work within the standard analyses of knowledge and of science. My purpose here is to call attention to a feature of modern science and mathematics which has not received sufficient attention. I leave for another occasion the large question of whether accepting my conclusion would force basic changes in epistemology and the philosophy of science or whether the idea of knowledge based on trust could be assimilated by the received views.

# I

In the early 1960s, Derek de Solla Price<sup>3</sup> observed that there was a rapid trend away from single-author papers in scientific journals. In fact, the trend is toward an ever-increasing number of authors per article.<sup>4</sup> Modern science is collegial not only in the sense that scientists build on the work of those who have preceded them, but also in the sense that research is increasingly done by teams and, indeed, by larger and larger teams. This is true for two reasons.

<sup>1</sup> *Epistemic Responsibility* (Hanover: New England UP, 1987).

<sup>2</sup> *The Community of Knowledge* (Aberdeen, Scotland: University Press, 1986).

<sup>3</sup> *Little Science, Big Science* (New York: Columbia, 1963), pp. 87–8.

<sup>4</sup> Limitations of space prevent giving references for statements I make about the current state of science or about the opinions of the scientific community. Readers interested in my sources should write me for a documented version of this paper.

(1) The process of gathering and analyzing data sometimes just takes too long to be accomplished by one person. In an earlier paper, I discussed an experiment that measured the lifespan of charm particles.<sup>5</sup> The paper reporting the results of this experiment has 99 authors, in part because it took about 280 person/years to do the experiment. Moreover, even for experiments that require less than a lifetime to run, the pace of science is often far too rapid for a lone experimenter to make any contribution at all by doing them. One of the authors of this paper on charm particles, William Bugg, estimated at the time of publication that, within three years, some other team would come up with a technique that gives a considerably better measurement and that, within five years, the paper would no longer be of interest. Five years later, his prediction has been borne out.

(2) Even more important for the purposes of epistemological analysis, research is increasingly done by teams because no one knows enough to be able to do the experiment by herself. Increasingly, no one could know enough—sheer limitations of intellect prohibit it. The cooperation of researchers from different specializations and the resulting division of cognitive labor are, consequently, often unavoidable if an experiment is to be done at all. No one particle physicist knows enough to measure the lifespan of charm particles. Indeed, Bugg reports that no one university or national laboratory could have done their experiment. None of the authors of such a paper is in a position to vouch for the entire content of the paper.

Teamwork is pretty standard fare within the empirical sciences. But it is not completely unknown in mathematics as well, due to the many areas of specialization required to complete some proofs. As just one example, consider J. Korevaar's<sup>6</sup> recounting of a critical stage in Louis de Branges's proof of Ludwig Bieberbach's conjecture (a conjecture in complex analysis which dates back to 1916, but which had resisted proof for almost 70 years):

For relatively small  $n$ , de Branges could verify immediately that the sums . . . are positive on  $(0, \infty)$ . But what about larger values of  $n$ ? At this stage de Branges went to his numerical colleague Gautschi at Purdue University for help. He told Gautschi that he had a way of proving

<sup>5</sup> "Epistemic Dependence," this JOURNAL, LXXXII, 7 (1985): 335–49.

<sup>6</sup> "Ludwig Bieberbach's Conjecture and Its Proof by Louis de Branges," *The American Mathematical Monthly*, XCIII (1986): 505–14. I owe this reference to Carl Wagner, who also informs me that there is now a much simpler proof of the Bieberbach conjecture which avoids reliance upon Askey's work.

the Bieberbach conjecture, but needed to establish certain inequalities involving hypergeometric functions. Would Gautschi be willing to check as many of these inequalities as possible on the computer? Gautschi wrote a suitable program with a feeling that he might soon hit a value of  $n$  for which the consistent positivity of expressions . . . would come to an end. Much to his surprise, however, he discovered that the crucial expressions were positive for all values of  $n$  which he tried:  $2 \leq n \leq 30$ . Thus at this time, assuming that the theoretical work was correct, de Branges and the computer had verified the Bieberbach conjecture for all  $n$  up to 30!

How to continue? Gautschi had the idea to call Askey at the University of Wisconsin, the world's expert on special functions. At first Askey was incredulous that the supposed positivity of sums . . . would prove the Bieberbach conjecture. However, he realized very soon that those sums were essentially generalized hypergeometric functions of a very special kind which are known to be positive (*ibid.*, pp. 512–3).

Specialization and teamwork are thus inescapable features of much modern knowledge acquisition. This point is not merely a genetic point about “the context of discovery.” Classical epistemology can admit—though usually not much is made of this fact—that trust plays a role in the origins of someone's knowledge. But specialization and teamwork apply in the “context of justification” as well. It is quite likely that no one mathematician has or will ever have the logical justification for each step in de Branges's proof. Those (like Askey) who know enough about hypergeometric functions probably do not know enough complex analysis to verify other parts of the proof; those (like de Branges) who know enough complex analysis have not mastered Askey's work. Possibly one mathematician could learn enough different mathematical specialties to grasp each step in de Branges's proof. But mathematicians do not think that would be a particularly useful thing to do, especially since no one can learn everything in mathematics. (Askey's work on special functions is apparently a small niche.) And clearly one particle physicist could never have sufficient justification for any claim at all about the lifespan of charm particles.

What are we epistemologists to say about de Branges's proof or the 99 physicists' measurement of the lifespan of charm particles? I think cases like these force us to make very basic choices: either we can modify our epistemological theories, or we can cling to them and deny that they could possibly be cases of knowing, since they fail to meet our requirements.

The latter option would be to say that experiments or “proofs” requiring teamwork could not possibly yield knowledge or even rational belief. Why not? Because knowing requires good reasons for

believing, and none of the mathematicians or physicists has sufficient reasons (except in testimonial form—more on that later) to accept the conclusions of their papers.

Moreover, it will not do epistemologically to have many tiny shreds of the empirical evidence or fragments of the mathematical proof in many separate minds. For it is the interconnection, the structure of these bits of evidence into a unified whole, that enables them to add up to a justified conclusion about charm particles or the Bieberbach conjecture. Since no one has sufficient evidence to justify the conclusion, there is no one who knows. But there is no knowledge without a knower, so the lifespan of charm particles is not known and could not be known. At least, not by humans. And de Branges has produced nothing that could count as a proof.

If this conclusion is unpalatable—as I think it is—we need an epistemological analysis of research teams, for knowledge of many things is possible only through teamwork. Knowing, then, is often not a privileged psychological state. If it is a privileged state at all, it is a privileged social state. So, we need an epistemological analysis of the social structure that makes the members of some teams knowers while the members of others are not. An analysis of testimony and testimonial evidence will provide a start on this project and also the next step in the present argument about the role of trust in epistemology.

## II

It is the testimony of one scientist or mathematician to another that connects the bits of evidence gathered by different researchers into a unified whole that can justify a conclusion. By accepting each others' testimony, individual researchers are united into a team that may have what no individual member of the team has: sufficient evidence to justify their mutual conclusion.

Elsewhere, I have developed an epistemological principle, "the principle of testimony" (*op. cit.*):<sup>7</sup>

- (T) If *A* has good reasons to believe that *B* has good reasons to believe *p*, then *A* has good reasons to believe *p*.

This principle is general enough to capture the epistemic structure of appeals by a layperson to the intellectual authority of experts. But we are speaking here, not of laypersons, but of research scientists, and of research scientists within the domains of their own special-

<sup>7</sup> Cf. also "Evidence, Testimony, and the Problem of Individualism—A Response to Schmitt," *Social Epistemology*, II (1988): 309–21. I owe the name, 'principle of testimony', to Frederick Schmitt.

ties. So if such researchers are sometimes knowers, we must consider a stronger version of the principle of testimony:

(T') If *A* knows that *B* knows *p*, then *A* knows *p*.

The problems epistemologists have seen in testimonial evidence are evident in both T and T'. Our epistemological training leads us to ask: How can *A* know that *B* knows *p* unless *A* herself first knows *p*? How can *A* even know that *B* has good reasons unless she herself has those reasons and knows that they are good reasons? Of course, *A* can learn from *B*, but how can *A* know through *B*?

These are good questions, important questions. But the epistemological requirement implicit in the rhetorical version of these questions would render testimony epistemically useless. In order to ground her knowledge that *p*, *A* appeals to *B*. Why? Clearly, the whole point of appealing to the testimony of others is that they know things we do not. If this were not the case, basing belief on testimony would be pointless at best, hence nonrational or irrational. The appeal to *B* must be able to strengthen *A*'s reasons for believing *p*; *A* now knows *p* at least partly because she knows that *B* knows *p*.

To count as good testimonial evidence for *p*, testimony must be working well (more on that below). But when testimony is working well, belief based on testimony is often not, as traditional epistemology would have it, a poor, second-best substitute for direct evidence. On the contrary, belief based on testimony is often epistemically superior to belief based entirely on direct, nontestimonial evidence. For *B*'s reasons for believing *p* will often be epistemically better than any *A* would/could come up with on her own. If the best reasons for believing *p* are sometimes primarily testimonial reasons, if knowing requires having the best reasons for believing, and if *p* can be known, then knowledge will also sometimes rest on testimony.

Nor is this conclusion always dependent on *A*'s limited competence in the domain of whether or not *p*: the only respectable beliefs anyone has or could ever have about charm particles must be based largely on the testimony of others. In fact, a belief based partly on second-hand evidence will be epistemically superior to any belief based completely on direct empirical evidence whenever the relevant evidence becomes too extensive or too complex for any one person to gather it all. For in all such cases, one can have sufficient evidence only through testimony. We are thus driven to accept the stronger principle of testimony: if *A* knows that *B* knows *p*, then *A* knows *p*. We must modify our epistemologies to make them compatible with this principle.

Testimonial evidence has potential problems as well as strengths, however, and they arise from the same feature: in order for testi-

mony to be useful, *A* cannot already have *B*'s reasons. So, if *A* accepts *p* on *B*'s say-so, those reasons (*B*'s reasons) which are necessary to justify *A*'s belief are reasons which *A* does not have. Sometimes it is feasible for *B* to share with *A* all the evidence necessary to justify the claim that *p*. But usually not. Indeed, if *A* and *B* come from different disciplines or even different specialties within the same discipline, *A* often will not know what *B*'s reasons are, much less why they are good reasons for believing *p*.

Thus, the blindness of *A*'s knowledge that *p*: those reasons which are necessary to justify *p* (and *A*'s belief that *p*) are reasons which *A* does not have. Obviously, since she lacks part of the evidence that justifies the claim that *p*, *A* is limited in the extent to which she can effectively scrutinize or challenge *B*'s claim about *p*. And yet we are to say that *A* knows that *p*, despite this blindness, this lack of the evidence necessary to justify *p*, this inability to evaluate the case for *p*?

Strange as this may seem, this is what we must say, unless we wish to maintain (1) that there can no longer be knowledge in many scientific disciplines because there is now too much available evidence (!); (2) that one can know *p* only by ignoring most of the best evidence for *p* (!); or (3) that some knowledge is known by teams or communities, but not by any individual person. Although I believe this third option has more plausibility than is generally acknowledged (it may also be the view of C. S. Peirce and John Dewey), in this paper I shall continue to pursue the idea that *A* does know *p* and that we need to modify our accounts of knowledge and rational belief to account for *A*'s knowledge.

Now, given the fact that *A* does not/cannot have *B*'s reasons, *A*'s position is really this:

- (1) *A* knows that *B* says that *p*.
- (2) *A* believes (and has good reasons to believe?<sup>8</sup>) that *B* is speaking truthfully, i.e., that *B* is saying what she believes.
- (3) *A* believes (and has good reasons to believe?) that *B* (unlike *A*) is in a position, first, to know what would be good reasons to believe *p* and, second, to have the needed reasons.
- (4) *A* believes (and has good reasons to believe?) that *B* actually has good reasons for believing *p* when she thinks she does.

<sup>8</sup> The reason for the parenthetical question in this statement and the next two is that I am undecided about what to say about implicit trust. If *A* trusts *B* implicitly, she will often not have or even feel the need to have good reasons to believe what *B* says. I think epistemic communities in which the climate of implicit trust prevails have real advantages over those in which good reasons for trusting are felt to be needed and are then supplied.



Although obvious, it is important to note two things about *B* and her contribution to *A*'s good reasons. First, unless *B* believes what she is saying, *B*'s knowledgeable ability about *p* will not give *A* good reasons to believe *p*. Thus, *A*'s good reasons depend on whether *B* is truthful, or at least being honest in this situation.

Second, even *B*'s truthfulness will not give *A* good reasons to believe *p* if *B* believes she has good reasons when she does not. So, in addition to being truthful, *B* must, first, be competent—she must be knowledgeable about what constitutes good reasons in the domain of her expertise, and she must have kept herself up to date with those reasons. Second, *B* must be conscientious—she must have done her own work carefully and thoroughly. And third, *B* must have “adequate epistemic self-assessment”—*B* must not have a tendency to deceive herself about the extent of her knowledge, its reliability, or its applicability to whether *p*.

Although the usefulness and the rationality of belief based on testimony stem from the fact that *A* does not, often even cannot, have *B*'s reasons for believing *p*, this fact also reveals that *A*'s reliance on *B*'s testimony must include reliance on *B*. The reliability of *A*'s belief depends on the reliability of *B*'s character. *B*'s truthfulness is part of her moral character. Competence, conscientious work, and epistemic self-assessment are aspects of *B*'s “epistemic character.” I shall return to this point frequently, and ‘character’ will refer to these moral and epistemic qualities. (Although competence is not a character trait per se, it standardly depends on character: becoming knowledgeable and then remaining current almost always requires habits of self-discipline, focus, and persistence.)

In short, *A* must TRUST *B*, or *A* will not believe that *B*'s testimony gives her good reasons to believe *p*. And *B* must be TRUST-WORTHY or *B*'s testimony will not in fact give *A* good reasons to believe *p*, regardless of what she might believe about *B*. A team of scientific experimentalists, for example, must both trust each other and be worthy of that trust or their experiment will not give anyone enough good reasons to believe their conclusions.

We thus reach another epistemologically odd conclusion: the rationality of many of our beliefs depends not only on our own character but on the character of others as well; the rationality of many of our beliefs depends on what others do and hence is not within our individual control. This is perhaps not a strange conclusion when we think about the various ways in which we as laypersons depend on others to have the evidence that supports our beliefs. But it becomes much stranger when we realize that this dependence on the character of others applies even to some of the epistemically best beliefs, i.e., to those of the top experts within their own fields of expertise.

The oddness stems, I contend, from the individualistic bias of most epistemology—with its penchant for epistemic self-reliance and self-sufficiency, and its flight from any form of epistemic vulnerability. But if our epistemic authorities are unreliable, we simply have no alternative but to hold less rational beliefs. Either we must then accept the testimony of unreliable authorities or we must rely on our own relatively inexpert and uninformed judgments.

Now, if *B* must be reliable in order for her testimony to be reliable, it seems that *A* must know *B*—at least to the extent of knowing that *B* is both morally and epistemically reliable—before *A* can have good reasons for believing *p* on the say-so of *B*. But scientists usually must rely on scientific testifiers who are not personally known to them. Clearly, this is often true for scientific testimony embodied in the literature. It is sometimes true even among members of a research team. (The team that measured charm particles was scattered over three continents, and Bugg reports that he knows only 10 or 12 of his 98 coauthors well enough to be able to form any judgment about the quality of their work.)

If *B* is not personally known to *A*, there are two strategies for attempting to ascertain the reliability of *B* and thus her testimony. The first is to check with someone who does know *B* and the quality of her work. This strategy can be expressed by extending the principle of testimony:

*A* has good reasons for believing *C* (also *D*, *E*, . . .) has good reasons for believing *B* has good reasons for believing *p*.

Normally, however, *A* will not be in a position to ascertain *C*'s reliability as a testifier about *B*, and *C* will often rely on the testimony of still others in order to form her judgment about *B*. This is not merely a philosophical quibble: as we shall see, the difficulty of gathering dependable testimony about the reliability of *B* is a problem of considerable practical import in contemporary science. Still, by repeating this procedure, i.e., checking with several knowledgeable people in *B*'s field, *A* will be able to ascertain *B*'s reputation within *B*'s discipline, and this surely will give *A* some evidence about the reliability of *B* and her testimony. But this process does not obviate the need for trust—it only redistributes and refines that trust.

There is a second strategy for attempting to ascertain the reliability of *B* when *B* is not personally known to *A*. That is to get a second opinion about the truth of what *B* has said. Often, *A* can find a *C* (also *D*, *E*, . . .) who is independent of *B* and who is also knowledgeable about whether *p*. If *C*, *D*, and *E* corroborate *B*'s testimony, *A* will have better reason to believe it.

We shall return to these strategies below, for they have played a

central role in explanations of the special reliability of scientific testimony and, consequently, of the scientific process itself. The second, under the rubric of "replication of experimental results," has been especially important.

### III

I have claimed that trust in the testimony of others is necessary to ground much of our knowledge, and that this trust involves trust in the character of the testifiers. But there is a basic objection to this thesis: that prudential considerations alone are sufficient to guarantee that the members of a scientific community will be truthful and also constantly vigilant against self-deception.

Michael Blais has developed this objection in his paper, "Epistemic Tit for Tat."<sup>9</sup> Blais argues that trustworthiness can be modeled as a strategy—indeed, the only prudent strategy—for a member of a scientific community. Blais acknowledges that trust is essential to science, because cooperation is essential, but he maintains that the type of cooperation at work does not require trust in the moral sense. "Only cooperation, as defined . . . in game theory and as illustrated in the Prisoner's Dilemma, is necessary for the justification of vicarious knowledge" (*ibid.*, p. 370). In science, cooperation means not defecting in the knowledge game—in other words, not cheating by fudging, fabricating, or otherwise publishing unreliable results. "Defection means succumbing to the temptation of leaving the other players in the knowledge game with the sucker's pay-off, while attempting to maximize immediate gain" (*ibid.*, pp. 370–1).

Blais echoes the common faith of scientists and philosophers that peer review and replication of results will detect defectors. "What count are factual results that are reproducible. If results cannot be reproduced, they may simply be rejected" (*ibid.*, p. 371). "Peer review and blind refereeing ensure that, in the long run, defectors should be found out" (*ibid.*, p. 372). Since Blais maintains that, in the game of science, the punishment for defecting is permanent exclusion from the game, he concludes that defecting is very imprudent.

At least one scientific community, however, the biomedical research community, has had its faith in replication and peer review shattered by a number of spectacular and highly publicized examples of research fraud.<sup>10</sup> Within biomedical science, the names of

<sup>9</sup> This JOURNAL, LXXXIV, 7 (1987): 363–75. Blais has reaffirmed this view in a more recent article, "Misunderstanding Epistemic Tit for Tat: Reply to John Woods," this JOURNAL, LXXXVII, 7 (1990): 369–74.

<sup>10</sup> No one knows how widespread fraud in science is. But a survey mailed to more than 2,100 scientists in six fields—physics, chemistry, biology, economics,

the fraudulent researchers have become well-known—John Darsee, Robert Slutsky, John Long, Vijay Soman, William Summerlin, Mark Spector, Stephen Breuning, and now Thereza Imanishi-Kari. Although there are other sloppy, careless, or deceptive research practices that may be even more damaging to the reliability of scientific testimony, “scientific misconduct”—commonly defined as plagiarism or the fabrication, falsification, or deliberate misrepresentation of data—is the most blatant example of defection in the knowledge game. The phenomenon of scientific misconduct reveals that a more thorough-going trust than mere strategic trust is involved in science. The consensus within the biomedical sciences is that neither peer review nor replication is likely to detect careless, sloppy, or even fraudulent research.

The number of really well-qualified referees for peer reviews is often inadequate, given the quantity of articles submitted and the complexity and multiplicity of techniques involved in research. Furthermore, an internally consistent and plausible fabrication cannot be detected by referees, since they do not examine the original data or the gathering of that data. Slutsky’s fraud, for example, was detected only because of his statistical naivete and also his very bad luck (two of his papers were read in quick succession by an astute reader). Those who investigated the Slutsky case maintain that his fraudulent papers could have been read independently for years without arousing any suspicion.

Nor will careless or fraudulent research normally be detected by replication, for the structure of modern science acts to prevent replication, not to ensure it. It is virtually impossible to obtain funding for attempts to replicate the work of others, and academic credit normally is given only for new findings.

When replication is attempted, it will not always detect fraudulent papers. In fact, replication paradoxically will support rather than unmask those fraudulent papers which happen to have correct conclusions. Thus, fraudulent papers announcing results that are predictable extensions of basic work done by others are quite likely to pass the test of replication. Even when attempted replication fails to produce similar results, there are often other explanations. Thus, when a group of Swiss researchers failed to replicate some of Dar-

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psychology, and sociology—revealed that nearly one-quarter of the respondents personally knew someone who had falsified data and 2/5 believed that some of their own work had been plagiarized (M. Davis, *The Perceived Seriousness and Incidence of Ethical Misconduct in Academic Science*. Unpublished Ph.D. thesis. The Ohio State University, 1989).

see's work, they considered various explanations for their inability to confirm his findings, but they did not consider the possibility of fraud. And both Darsee and Spector argued that their experiments were too delicate to work unless the experimenter had extremely subtle skills. For these reasons, Relman,<sup>11</sup> a respected observer, has concluded that "fraudulent data may be rapidly identified in an area of great importance where research activity is intense, but that is probably not true in most fields" (*ibid.*, p. 1416).

We can see why Blais's argument must fail. Game-theoretic arguments such as Blais's rest on two assumptions. The first, which Blais acknowledges, is that the relationship be durable: "a cooperative strategy . . . has little chance in the short or medium term; only long-term relationships permit it to hold its own" (*op. cit.*, p. 368). The second assumption is, obviously, that the other players will recognize when they have received the sucker's pay-off.

These assumptions are the Achilles' heel of Blais's application of game theory to the issues of scientific testimony. Usually, the cooperative relationship is precisely the short- or medium-term relationship. By the standards of game theory, 20 or 30 trials is not a very long run, but a researcher can substantially enhance her career if she can successfully publish several fraudulent articles. Also, as we have seen, it is often very difficult for others to detect defectors. Indeed, there is a class of lapses from acceptable scientific practice which are not discoverable by anyone else, since only the researchers have the actual empirical data upon which their paper is, presumably, based. Even if lab logbooks are kept in a form that allows inspection by others, one would need to have been present during the experiment to know that the logbooks are not themselves fraudulent.

Reliance upon inside informants—co-investigators or others in the lab—is widely recognized to be crucial to the detection of scientific misconduct. But this reliance is also problematic, for many reasons. Detection is often very difficult for those not in the defector's field of specialization, which compounds the problem in multidisciplinary work. Moreover, co-investigators are often not the ones who will be damaged by the defection, since the prevailing ethos within science is that joint authors are not responsible and should not be penalized for the fraudulent or sloppy work of one member of their team. (Consequently, there is a temptation to collaborate with someone you personally know to be a defector—to leave, for example,

<sup>11</sup> "Lessons from the Darsee Affair," *New England Journal of Medicine*, CCCVIII, 23 (1983): 1415–7.

the collection of data to someone you know to be "sloppy.") Finally, there is a whole range of deterrents to informing on one's colleagues in the lab, ranging from loyalty, to reluctance to meddle in others' affairs, to the absence of adequate protection for scientific whistle blowers. Virtually all observers agree that the confidentiality of an informant cannot be successfully protected. Some informants have paid dearly for their work in uncovering scientific fraud.

Thus, contrary to what Blais suggests, detection is often quite difficult. And, contrary to what Blais maintains, the punishment for proven defection is also often not severe. Although fraudulent researchers whose cases attract wide publicity may well forfeit their reputations and careers, others do not. Some quietly relocate to other institutions; others do not even lose their jobs. A recent survey suggests that scientists themselves do not believe that even temporary exclusion from research would result from proven fraud.<sup>12</sup>

There are reasons, of course, for less than Draconian punishment of research fraud. These include fear of liability, of unfairly tainting the reputations of co-authors, of shaking public confidence in science, or of jeopardizing the reputation and the funding of the laboratory and university in which a fraudulent researcher worked. Since a fraudulent researcher's institution can achieve the penalty of permanent exclusion from the scientific community only at the cost of a great deal of effort and unfavorable publicity, institutions have a strong incentive to settle for less severe and more private punishments.

Clearly, a researcher's prudential concerns are often insufficient to ensure her trustworthiness and thus the reliability of her testimony. General recognition of this fact naturally leads to two responses: (1) deterrents to defection should be strengthened, even if they may be insufficient; (2) if prudential reasons are not sufficient to assure trustworthy testifiers, ethical researchers are required. The need for "research ethics" is now widely acknowledged. The Institute of Medicine—sister to the National Academy of Sciences—has, for example, recently called for universities to provide formal instruction in research ethics for all science students at both the undergraduate and graduate level.

Scientists working in some other fields maintain that attempts to replicate experimental findings are still common in their sciences and thus that "it can't happen here." Perhaps there really is more replication in some sciences than in others, but it seems reasonable

<sup>12</sup> J. P. Tangney, "Fraud Will Out—Or Will It?" *New Scientist*, CXV (August 6, 1987), pp. 62–3.

to conjecture that there will be less replication (1) of long, costly, or very time-consuming experiments; (2) of experiments that require exotic equipment, substances, or specimens; (3) in those fields in which senior scientists can train their graduate students (and post-docs, residents, fellows) by having them gather new data rather than replicate existing results; (4) of experiments requiring the cooperation of multidisciplinary teams. It also seems likely that (5) considerations of the ethical use of research subjects will ensure that there will be no routine or even widespread replication of experiments involving human—or, increasingly, even animal—research subjects. These conditions apply to many sciences—and they apply increasingly as scientific research becomes more sophisticated, complex, and costly.

Peer review and attempted replication may once have been effective deterrents to fraudulent or deceptive publication. They are now much weaker deterrents. In any case, they can never have been completely effective detectors of fraudulent or misleading testimony, for, as we have seen, both have in-principle weaknesses.

#### IV

Often, then, a scientific community has no alternative to trust, including trust in the character of its members. The modern pursuit of scientific knowledge is increasingly and unavoidably a very cooperative enterprise. Cooperation, not intellectual self-reliance, is the key virtue in any scientific community. But epistemic cooperation is possible only on the basis of reliance on the testimony of others. Scientific propositions often must be accepted on the basis of evidence that only others have. Consequently, much scientific knowledge rests on the moral and epistemic character of scientists. Unavoidably so. Not because “hard data” and logical arguments are not necessary, but because the relevant data and arguments are too extensive and too difficult to be had by any means other than testimony.

Indeed, a scientific community is often forced to rely on the testimony of one testifier. Replication of experiments is not standard. Moreover, peer review can never detect plausible and internally consistent fabrications, and attempted replication will support rather than unmask fraudulent reports that happen to have true conclusions. So, there sometimes simply are no *C*, *D*, and *E* who can corroborate or effectively challenge the testimony of *B*. And the testimony of *C*, *D*, and *E* about the professional reputation of *B* may well be seriously misleading: before their frauds were detected, some fraudulent biomedical researchers were widely regarded within their disciplines as outstanding exemplars of scientific productivity, even as scientific geniuses.

Institutional reforms of science may diminish but cannot obviate the need for reliance upon the character of testifiers. There are no "people-proof" institutions. And even if it were possible fully to police scientific research, it would still be necessary to rely on the integrity of the newly-created "science cops" and the reliability of their testimony. (There has, for example, been Congressional concern over the lack of independence of those now being assigned the responsibility of investigating research fraud.)

Science, then, is not completely different from other cooperative enterprises: the reliability of scientific testimony, like the reliability of most other testimony, ultimately depends on the reliability of the testifier, or on the reliability of those charged with ensuring the reliability of the testifier. Obviously, none of this means that deterrents to scientific misconduct are unimportant. Since knowledge of right and wrong will not deter the unscrupulous, deterrents remain important, even if insufficient. Still, the main point here is that scientific knowledge rests on trust, and not only on trust in deterrents, but also on trust in the character of scientists.

The view of Blais (and many, many others) thus becomes part of the problem: one reason the problem of fraudulent research looms so large is that for decades scientists have insisted that science is virtually fraud-proof. Philosophers have too often joined in the chorus. Inability to see the role of trust in science effectively destroys our ability to combat unreliable scientific testimony. It undermines any attempt to formulate and teach research ethics and it stifles any attempt to introduce new deterrents to fraud. A fraud-proof institution has no need for additional protection against fraud.

We must, however, guard against jumping to the conclusion that we should try to redesign our epistemic institutions so as to minimize the role of trust in knowledge. For the alternative to trust is, often, ignorance. An untrusting, suspicious attitude would impede the growth of knowledge, perhaps without even substantially reducing the risk of unreliable testimony. Trust in one's epistemic colleagues is not, then, a necessary evil. It is a positive value for any community of finite minds, provided only that this trust is not too often abused. For finite minds can know many things only through epistemic cooperation. There is, then, a very delicate balance between places for trust within epistemic communities and places for insisting on better safeguards against untrustworthy testifiers.

Philosophers of science, epistemologists, logicians, and ethicists all should be helping concerned members of scientific communities both to formulate an ethics of scientific research and testimony, and also to design structural reforms that will strengthen the deterrents



to scientific misconduct without destroying collegiality and creativity. But we may not be able to help until we get our own conceptual houses in better order. The conclusion that knowledge often is based on certain kinds of relationships between people, on trust, and consequently on the character of other people is an epistemologically odd conclusion. It is odd even for pragmatists, though pragmatists have generally had more room for epistemic community in their theories. To my mind, this oddness is symptomatic of what will be needed to assimilate an acknowledgement of the role of trust into our epistemologies. We have a lot of work to do.

Clearly, the implications of the role of trust in knowledge will reach beyond epistemology and the philosophy of science into ethics and social philosophy. I close with just one example. The prevailing tenor of twentieth-century Anglo-American philosophy has been that epistemology is more basic than ethics. On this view, ethics must meet epistemological standards on pains of bankruptcy. And the prevailing suspicion in our culture—a suspicion nurtured by philosophy—is that ethics cannot pass the epistemological test, and that there is thus no ethical knowledge. Science, in contrast, is commonly believed to be too “hard” and “objective” to require anything as mushy and subjective as ethics.

But scientific realism—indeed any theory that grants objectivity to scientific judgments—turns out to be incoherent when combined with subjectivism or skepticism in ethics. It remains true, of course, that ethical claims must meet epistemological standards. But if much of our knowledge rests on trust in the moral character of testifiers, then knowledge depends on morality and epistemology also requires ethics. In order to qualify as knowledge (or even as rational belief), many epistemic claims must meet ethical standards. If they cannot pass the ethical muster, they fail epistemologically.

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