science's many peer review systems. For example, most scientific journals accept articles for publication based on evaluations by experts. And in most fields, those experts are not told the identity of the authors whose articles they are reviewing. Although not being told the author's name does not guarantee his or her anonymity – because in many fields a well-connected reviewer can guess the identity of an author from the content of the article – it supports universalism nonetheless, both in practice and as an ideal.

Communism states that scientific knowledge – the central product of science – is commonly owned. Originators of ideas can claim recognition for their creativity, but cannot dictate how or by whom those ideas are to be used. Results should be publicized, so that they can be used as widely as possible. This serves the ends of science, because it allows researchers access to many more findings than they could hope to create on their own. According to Merton, communism not only promotes the goals of science but reflects the fact that science is a social activity, or that scientific achievements are collectively produced. Even scientific discoveries by isolated individuals arise as a result of much earlier research.

Disinterestedness is a form of integrity, demanding that scientists disengage their interests from their actions and judgments. They are expected to report results fully, no matter what theory those results support. Disinterestedness should rule out fraud, such as reporting fabricated data, because fraudulent behavior typically represents the intrusion of interests. And indeed, Merton believes that fraud is rare in science.

Organized skepticism is the tendency for the community to disbelieve new ideas until they have been well established. Organized skepticism operates at two levels. New claims are often greeted by arrays of public challenges. For example, even an audience favorably disposed to its claims may fiercely question a presentation at a conference. In addition, scientists may privately reserve judgment on new claims, employing an internalized version of the norm.

In addition to these "moral" norms there are "cognitive" norms concerning rules of evidence, the structure of theories, and so on. Because Merton drew a firm distinction between social and technical domains, cognitive norms are not a matter for his sociology of science to investigate. In general, Merton's sociology does not make substantial claims about the intellectual content of science.

Institutional norms work in combination with rewards and sanctions, in contexts in which community members are socialized to respond to those rewards and sanctions. Rewards in the scientific community are almost entirely honorific. As Merton identifies them, the highest rewards come via eponymy: *Darwinian biology*, the *Copernican system*, *Planck's constant*, and *Halley's comet* all recognize enormous achievements. Other forms of honorific

reward are prizes and historical recognition; the most ordinary form of scientific reward is citation of one's work by others, seen as an indication of influence. Sanctions are similarly applied in terms of recognition, as the reputations of scientists who display deviant behavior suffer.

In the 1970s, the Mertonian picture of the ethos of science came under attack, on a variety of instructive grounds. Although there were many criticisms, probably the three most important questions asked were: (1) Is the actual conduct of science governed by Mertonian norms? To be effective, norms of behavior must become part of the culture and institutions of science. In addition, there must be sanctions that can be applied when scientists deviate from the norms; but there is little evidence of strong sanctions for violation of these norms. (2) Are these norms too flexible or vague to perform any analytic or scientific work? (3) Does it make sense to talk of an institutional or overarching goal of something as complex, divided, and evolving as science? These and other questions created a serious challenge to that view, a challenge that helped to push STS toward more local, action-oriented views.

Ethos and Ethics

Social norms establish not only an ethos of science but an ethics of science. Violations of norms are, importantly, ethical lapses. This aspect of Merton's picture has given rise to some interesting attempts to understand and define scientific misconduct, a topic of increasing public interest (Guston 1999a).

On the structural-functionalist view, the public nature of science should mean that deviant behavior is rare. At the same time, deviance is to be expected, as a result of conflicts among norms. In particular, science's reward system is the payment for contributions to communally owned results. However, the pressures of recognition can often create pressures to violate other norms. A disinterested attitude toward one's own data, for example, may go out the window when recognition is importantly at stake, and this may create pressure to fudge results. Fraud and other forms of scientific misconduct occur because of the structures that advance knowledge, not despite them.

Questions of misconduct often run into a problem of differentiating between fraud and error, both of which can stand in the way of progress. The structural-functionalist view explains why fraud is reprehensible, while error is merely undesirable. The difference between them is the difference between the violation of social and cognitive norms (Zuckerman 1977, 1984).

Such models continue to shape discussions of scientific misconduct. The US National Academy of Sciences' primer on research ethics, *On Being a*

Box 3.1 Is fraud common?

There are enormous pressures on scientists to perform, and to establish careers. Yet there are difficulties in replicating experiments, there is an elite system that allows some researchers to be relatively immune from scrutiny, and there is an unwillingness of the scientific community to level accusations of outright fraud (Broad and Wade 1982). It is difficult, then, to know just how common fraud is, but there is reason to suspect that it might be common.

Because of its substantial role in funding scientific research, the US Congress has on several occasions held hearings to address fraud. Prominently, Congressman Albert Gore, Jr. held hearings in 1981 in response to a rash of allegations of fraud at prominent institutions, and Congressman John Dingell held a series of hearings, starting in 1988, that featured "the Baltimore case" (Kevles 1998).

David Baltimore was a Nobel Prize-winning biologist who became entangled in accusations against one of his co-authors on a 1986 publication. The events became "the Baltimore case" because he was the most prominent of the scientific actors, and because he persistently and sometimes pugnaciously defended the accused researcher, Thereza Imanishi-Kari. In 1985, Imanishi-Kari was an immunologist at the Massachusetts Institute of Technology (MIT), under pressure to publish enough research to merit tenure. She collaborated with Baltimore and four other researchers on an experiment on DNA rearrangement, the results of which were published. A postdoctoral researcher in Imanishi-Kari's laboratory, Margot O'Toole, was assigned some follow-up research, but was unable to repeat the original results. O'Toole became convinced that the published data was not the same as the data contained in the laboratory notebooks.

After a falling-out between Imanishi-Kari and O'Toole and a graduate student, Charles Maplethorpe, questions about fraud started working their way up through MIT. Settled in Imanishi-Kari's favor at the university, Maplethorpe alerted National Institutes of Health scientists Ned Feder and Walter W. Stewart to the controversy. Because of an earlier case, Feder and Stewart had become magnets for, and were on their way to becoming advocates of, the investigation of scientific fraud. They brought the case to the attention of Congressman Dingell.

In the US Congress the case became a much larger confrontation. Baltimore defended Imanishi-Kari and attacked the inquiry as a witch-hunt; a number of his scientific colleagues thought his tack unwise, because of