When Does Intellectual Passion Become Conflict of Interest?

Financial conflicts of interest are very much in the news in science, particularly in the cutting-edge fields of biology that border on biotechnology. As the financial stakes grow, the confusion is likely to grow as well, until the scientific community settles on rules and procedures for dealing with conflicts between research and profit (see story on page 616). But in talking with researchers about potential financial conflicts, *Science* heard one refrain over and over again: that money problems are simple compared to the intellectual conflicts of interest that scientists have always had to deal with.

What did those researchers mean by intellectual conflicts of interest? They were referring to the fact that, although science is often thought of as a dispassionate pursuit of facts, in reality it is much more than that. Scientists are, after all, human beings. They often begin their work with a hypothesis and become deeply invested in it, long before peers regard it as credible. Along the way to proving a thesis, therefore, scientists must be sustained by something that approaches faith. And, as paleontologist-essayist-historian Stephen Jay Gould says, it is a "pervasive fact of human existence as social beings" that we find it extraordinarily difficult to step outside our own convictions and see them through the eyes of a detached observer.

Every researcher relies on personal intuition to some extent, so the important question is: When does a scientist's enthusiasm for an idea cross the line that separates passion from obsession? It doesn't take a sociologist to recognize the extreme cases. Working scientists can —and do—readily identify peers whom they regard as having become advocates, no longer capable of reading evidence in an evenhanded way. But sometimes those advocates are right. And in these rare cases, science is advanced by the determined, committed, even the obsessed individual, not by the doubting peers.

To examine the intertwined positive and negative aspects of commitment to one's own hypotheses, *Science* chose three cases in which researchers seemed to have an unusual per-

PROFILE 1

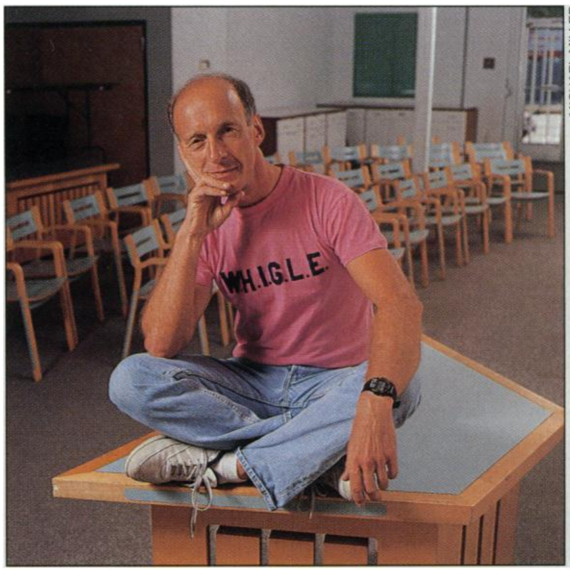
Sex on the Brain

Simon LeVay, a brilliant neurobiologist who has found a link between human brain structure and homosexuality, is gay himself. He freely discusses this aspect of his life. Indeed, he took temporary leave from his research position this year to direct a new advocacy foundation to promote the interests of gay people in Los Angeles—the West Hollywood Institute for Gay and Lesbian Education. And LeVay recognizes that some people might question his ability to do objective research while at the same time leading a public campaign on a related topic. His answer is that he can do both by maintaining his customary, high scientific standards.

LeVay studied as an undergraduate at Cambridge University, received a Ph.D. from the University of Göttingen, taught at Harvard University, and is now a staff scientist (on leave) at the Salk Institute in San Diego. In 1991 he became a celebrity. Fame arrived when he published a paper in *Science* (30 August 1991, p. 956) reporting that the size of one particular nucleus in the brain may be correlated with male homosexuality. LeVay reached this conclusion after measuring the size of part of the anterior hypothalamus associated with sexual behavior. He found that the third interstitial nucleus (INAH-3) is half as large in women and in homosexual men as it is in heterosexual men.

If correct, this suggests that gay behavior may be a product of genetics and biochemistry rather than culture—a fact that could bring about a broad reappraisal of homosexuality. Some, including LeVay, argue that this research will help remove the stigma of being gay.

LeVay acknowledges that "in the gay community, [the *Science* paper] is taken very much as supporting the notion that people are born gay or straight, as ammunition against people who think that being gay is a sort of willful perversity." He adds: "I share that feeling; like most gay men, I feel I was born gay...though rationally I have to say that [my conviction] certainly doesn't prove it." In speaking about his research, LeVay says, "I try to be clear about just how much is my feeling and my political views, and how much is what science actually has shown. But it sometimes does get blurred...on talk shows, for example."



MICHAEL MILLER

Could it be that LeVay's convictions about homosexuality, which predated his research, somehow affected his results? It's a fair question for any researcher and doubly so in this case, since the analytical method required by the kind of work LeVay does is unavoidably subjective. The technique involves reading tissue slides to determine the size of the INAH-3 nucleus, which is made of the same type of cells as the surrounding tissue and therefore has no sharp boundary. Hence, expert judgment must enter in. To make the slide-reading process as objective as possible, Roger Gorski of the University of California, Los Angeles, who works on the same questions, decided to use more than one slide interpreter. At first he used three, requiring that disagreements be settled by compromise. With experience, Gorski learned that two readers were enough.

LeVay didn't go to these lengths—he read his slides himself—but says he took steps to ensure objectivity. Like other scientists, he says, "I do the usual things you do to avoid bias, like doing the work blind...using regular statistical procedures...talking to other scientists and getting their input." Why did LeVay rely on himself as the only slide reader? Because "I didn't have any colleagues" to help out. He insists there was no way for his personal views to intrude, since students encoded the slides and data before he interpreted them.

That's enough to reassure most peers, including Gorski's colleague, postdoc Laura Allen. She says, "You'd have to argue that Simon wasn't being honest to fault him, and I have no reason to believe he's not very, very honest—and very well trained." Besides, she says, the results were "not unexpected," since they confirmed a possibility she and Gorski had raised earlier. The fact that LeVay had a personal interest doesn't trouble her, either: "If you study Alzheimer's disease because your grandmother has Alzheimer's, you also have a personal interest," she says. Indeed, she adds that "if you're doing an important study, hopefully you'll have some passion for it."

Another peer, Dick Swaab of the Netherlands Institute of Research in Amsterdam, notes that it is "impossible" to run a truly objective study using human brains. The reasonal investment in their research. The cases are varied. One involves a neurobiologist with a deep personal and social stake in the outcome of his own work. Another focuses on an archeologist criticized by his peers for being too quick to announce earth-shaking claims. A third involves an accomplished space physicist whose defense of an offbeat theory has put him at odds with most of his field. These cases differ in detail, but each raises the question of how scientists can retain their passion while maintaining enough detachment to prevent commitment from hardening into obsession.

These cases may seem atypical, yet almost every researcher *Science* talked to on this subject acknowledged that intellectual conflicts of interest—or potential conflicts—are pervasive. The key difference among scientists, they said, is not between those who have conflicts and those who do not, but in how the potential conflicts are handled—whether the researcher has the detachment required to be the severest critic of his or her own work.

All researchers tend to "mythologize" their research, says Boston University's philosopher of anthropology, Misia Landau. And this isn't necessarily bad, she adds, because it takes self-confidence to push ahead. Landau thinks "the most inspired work gets done in light of some hypothesis" that serves as a "guiding paradigm." Yet scientists must also be ready to drop a cherished idea the moment better information comes along. It's important, she says, to "practice a certain self-reflection."

In the absence of that self-reflection, an advocate becomes so deeply invested that it's almost impossible to let go, even in the face of contrary evidence. "Any theory can be patched, by ad hoc addition of assumptions to fit with existing data," writes psychologist Anthony Greenwald of the University of Washington in Seattle, who has analyzed problems scientists have in developing good research strategies. The goal is to "disconfirm" an idea, Greenwald writes, not confirm it. Otherwise, the scientist risks becoming "ego-involved" in the idea and "may be willing to persevere indefinitely," despite negative results.

Albert Barber, vice chancellor for research at the University of California, Los Angeles, explains that all good graduate programs try to "teach people rigor. . .to disprove what they think rather than to prove it. You have to keep reminding people that they do have a bias [favoring their own ideas]; they can prove something a dozen times without it being true." Like researchers in the real world, Barber's students quickly learn that if they don't challenge their own ideas, others will.

–Eliot Marshall

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son: There are no "controls," and the subject material is quite variable. Results can be affected by the patient's age, type of disease, differences in therapy, speed of death, methods of tissue fixation, and other factors—many of which are undetermined. Swaab worries about these technical flaws, but not the personal bias of the interpreter. For example, Swaab thinks LeVay could have made his results stronger by counting the number of cells within the INAH-3 structure, rather than just measuring the volume. This would rule out errors due to swelling or shrinkage, which might be caused by disease or chemicals. But as for personal stakes, Swaab says, "I don't think they influence the type of measurements" that he and LeVay make.

LeVay has run into criticism, however, from a few researchers in other fields. Some think it's simplistic to link human sexual behavior to specific brain structures and suspect that LeVay has allowed his own motivations to influence his conclusions. For example, John DeCecco, psychologist and director of the Center for Research and Education in Sexuality at San Francisco State University, scoffs that LeVay is "definitely on a political crusade." He thinks LeVay is "under the erroneous impression that if he can prove this is biological...people will leave gay people alone and respect them."

Another critic in this camp, William Byne, a psychiatrist at Columbia University, faults LeVay for not obtaining good sexual and medical histories on the people whose brains he examined. The AIDS virus may affect testosterone levels, Byne says, and this could affect the size of the INAH-3. He wants LeVay to share all his data so he can double-check the results. But critics aren't any more immune to intellectual conflict than those they criticize. In fact, LeVay doesn't like the sound of Byne's demand because Byne "has the reputation of someone with a chip on his shoulder," and his request sounds like a "commission of inquiry." Although LeVay agrees that "science should be an open matter," he decided to turn down the request.

For Simon LeVay himself, advocacy is not something that must necessarily be avoided in science. He sees no need to create special barriers between his role as an advocate and his work as a scientist—other than following standard scientific rules for keeping data blind. Indeed, although he acknowledges that he and his field are controversial, he doesn't think that in principle his situation as a scientist is really that different from the fundamental situation of any researcher. "Everyone," he says, "has some place they're coming from; every scientist is a human being."

–E.M.

PROFILE

The Perils of a Deeply Held Point of View

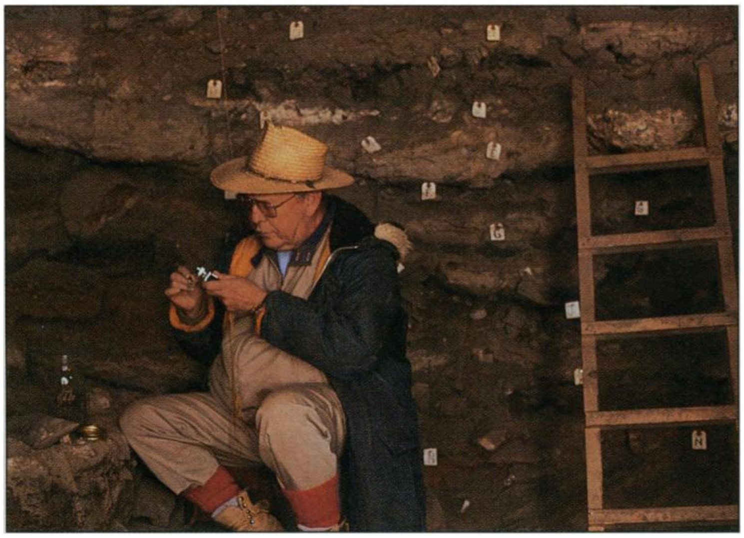
Richard "Scotty" MacNeish, a feisty 74-year-old archeologist, member of the National Academy of Sciences, and excavator of New World sites, is a model of the committed scientist. For as long as anyone can remember, he's been trying to disprove a theory about the "first Americans" held by many of his colleagues. Specifically, MacNeish disagrees with the orthodox view that the first human settlements in the New World began no more than 12,000 years ago. The date is pegged to some unusual stone weapons found in the 1920s at Clovis, New Mexico. No weapons of this type, or evidence of a culture that might produce them, have been found in North or South America with an earlier date. So the "Clovis-first" theory has prevailed for decades.

Indeed, the Clovis paradigm has survived many direct assaults, including several by MacNeish. For example, in the 1970s, MacNeish published a list of 12 claimed pre-Clovis sites in South America alone, including a cave at Pikimachay, in Peru, that he excavated himself. However, his evidence did not persuade doubters to stop doubting, nor did it get textbook editors to put an earlier date on human entry to the Americas. MacNeish regards this resistance to his work as "ingrained conservatism." "It's damn near a 100-year-old tradition," he says, to dismiss evidence of early cultural sites in the New World. People who draw the line at Clovis have spent a lot of "time and effort building up evidence" for their view. If somebody like himself comes along and challenges it, "they're going to fight for what they think is right," MacNeish says.

MacNeish was in the news again this year as a shaker of orthodoxy, telling an audience in Chicago that he has now found "incontrovertible proof" that would establish a 30,000-year antiquity for human settlements in North America. His proof consists of hundreds of objects collected recently at Pendejo Cave at Fort Bliss, New Mexico. "This is the one that's going to finish off the skeptics," he told *The Washington Post*. "This time we knew exactly what kind of evidence it was going to take to convince people" (*Science*, 21 February, p. 920).

Back home at the Andover Foundation for Archeological Research in Andover, Massachusetts, for which he is scientific director, MacNeish discussed his evidence in a phone interview. It includes "500 objects" made of stones—many foreign to the cave—which he thinks were chipped by humans, a large buffalo bone about 35,000 years old with evidence on it of human chopping, remains of eight hearths, an animal toe bone with a projectile point in it, several human fingerprints on clay dated at 30,000 to 35,000 years old, and even a human mongoloid hair of about the same age. More important, MacNeish says, he has 32 carbon-14 dates in neat chronological order from distinct levels within the cave, establishing a clear context for all the objects. The authenticity of the material, MacNeish insists, has been checked not just by himself but by "10 or 15 other experts."

The case sounds overwhelming—until you talk to other experts, even some who agree with MacNeish's point of view but who worry about his style of scientific presentation. For example, Alan Bryan of the University of Alberta in Edmonton, a "dirt archeologist just like MacNeish," considers himself a friend and ally but remains a skeptic on the artifacts in Pendejo Cave. MacNeish eventually "may be able to demonstrate his case," says Bryan, "but I don't think he has all the evidence to do it yet." Bryan thinks MacNeish was "just premature in announcing...that this is going to shake the universe."



Looking for trouble? Richard MacNeish in Pendejo Cave, New Mexico, where he says he has found evidence of a 30,000-year-old human dwelling, evidence some of his colleagues find hard to accept.

The Pendejo claims also draw friendly fire from James Adovasio of Mercyhurst College in Erie, Pennsylvania. He excavated the Meadowcroft Rock Shelter in Pennsylvania, which many consider the most credible pre-Clovis candidate site in North America. "Judging by what I have seen to date in print on Pendejo," says Adovasio, "I don't think many people—except other disciples of remote antiquity—are ready to believe that the site is as old as he says it is, whether or not he has human hair."

Why does the skepticism run so deep, even among fellow iconoclasts? Most objections fall into two broad categories, both connected to MacNeish's style as an anti-Clovis crusader. One focuses on his habit of anticipating the results of his research with bold predictions in the lay press. The other has to do with his standards for "artifacts," which some of his colleagues consider too generous.

As Bryan says, "Scotty tends to jump to conclusions....It's just the way he works. If he's convinced of something, he just pushes on." Sometimes this leads him to announce what his collaborators will find—even before they've found it. He "tends to make the claims before their studies are finished," says Bryan, who adds, "If I were tackling [Pendejo Cave], I wouldn't have made any announcement for a year or two."

This concern is echoed by archeologist Brian Fagan of the University of California, Santa Barbara, and David Meltzer of Southern Methodist University in Dallas, Texas, both skeptical of pre-Clovis claims. Meltzer says: "You can't just play to the choir; you've got to play to the skeptics."

Despite the questions people raise about Pendejo Cave, they admire MacNeish as a scientist and teacher, and they concede that he may prove right. Meltzer says MacNeish is a "tremendously productive and important scholar" who has inspired many students during his career as a professor of anthropology at half a dozen colleges in the United States and Canada. According to Fagan, MacNeish "has enormous stature in archeology," particularly for his research on the origins of agriculture in the New World.

MacNeish defers to no one when it comes to justifying his own methods and conclusions. He grumbles that some critics haven't bothered to visit Pendejo for a firsthand look at the evidence, adding that he thinks it's essential to get into the field. He is proud that he wrote "more site reports as an undergraduate" than some of the critics have written in their lives. As for the quality of the artifacts, MacNeish says, "I will agree that from many standpoints they are extremely crude," but the reason for this, he insists, is that they were made for simple, "spur-of-the-moment butchering" and were not elaborately prepared. More than anything else, the context in which they were embedded makes them credible, he claims. The charge that he jumps the gun in publicizing his research, MacNeish says, is "nonsense." He blames the publicity on the press, which has been "breathing down my neck." Finally, MacNeish rejects the argument that he went to Pendejo with preconceived plans to attack the Clovis thesis. "Honest to God," he says, "I went into that cave because there were corn cobs up on top," and "I was interested in early agriculture." When he found evidence of pre-Clovis humans, he says, he "was as surprised as anyone."

–E.M.

PROFILE 3

Small Comets/Big Flap

It may be true that every scientist starts with a personal commitment. But not all commitments develop in the same way; some, for example, become deeper and more difficult to put aside over time. This can happen if a researcher proposes a theory that puts him at odds with his peers. As he defends his ideas against criticism, battle lines become hardened and exchanges grow heated, making it almost impossible for either side to admit that it was mistaken to begin with.

This is exactly what seems to have happened to Louis Frank, an accomplished space physicist at the University of Iowa, winner of the National Space Act Award and protégé of James Van Allen, discoverer of the "Van Allen belts" of radiation that encircle Earth. Frank is now working without controversy on a plasma physics experiment for the U.S.-Japanese Geotail satellite, scheduled for launch as *Science* goes to press. But since 1986, Frank has also been embroiled in a different matter: a fight about comets.

It began when Frank tried to understand some anomalous dark spots in ultraviolet images of Earth's atmosphere captured in 1981 by the Dynamics Explorer (DE) satellite. Frank, principal investigator for the instrument, was haunted by the spots: "We couldn't get rid of them" with any simple explanation, he says. After years of analysis, Frank suggested in 1986 that they might be produced by midget comets. The DE images showed, according to his calculations, that every minute, about 20 truck-sized chunks of ice disintegrate above Earth's atmosphere, each one dumping about 100 tons of water on the planet. The dark spots in the pictures, he claimed, were 50-kilometer-wide vapor clouds blocking Earth's ultraviolet glow.

Almost no other space scientist agrees with this theory, though two have found evidence that lends some support. One of these researchers was the late Clayne Yeates, a staffer at the Jet Propulsion Laboratory. Using a special asteroid-hunting telescope, he found traces of light that he thought might fit Frank's description of small comets. But asteroid expert Tom Gehrels of the University of Arizona, who had loaned his telescope for the search, disagreed.

Another scientist who came up with favorable data is John Olivero, a meteorologist at Pennsylvania State University. He says he set out to disprove Frank's thesis in 1986 by searching through his own microwave data on the upper atmosphere for evidence of large water bursts. Expecting to find at most three—an amount within the random noise level—he instead found 113. The results, Olivero says, were "too darn close" in scale and frequency to Frank's prediction to be dismissed.

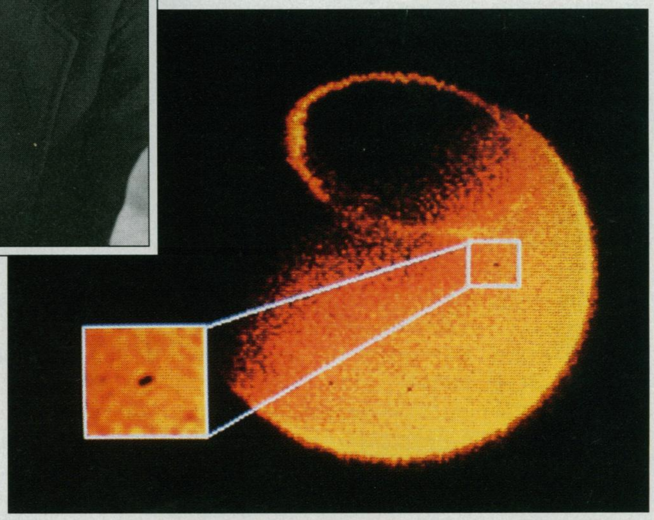
Although two of Olivero's graduate students wrote theses based on this research, Olivero himself still hasn't published an article on it. Why not? Olivero says he must run additional tests to rule out possible sources of noise, and he just hasn't had the time or the money, adding, "This is not a subject that's easy to get funding for." Olivero claims a friend at the National Science Foundation told him "not to even think of submitting a proposal" because "reviewers would cut you to shreds." He's hoping someone else will try to replicate the work.

Most space scientists, however, believe that the dark spots on Frank's satellite were caused by random instrument noise. Frank says that he and his colleagues, John Sigwarth and John Craven, investigated this possibility and concluded it just didn't fit the data. Craven, now at the University of Alaska, Fairbanks, agrees that he can't think of "any rational explanation" based on instrument error, either. Craven points out that the equipment was thoroughly tested before flight, and of the three identical sensors, only the one tuned to a particular ultraviolet frequency produced spots. The best alternative, Frank decided, was to go with the small comets. Frank says he knew his life would be easier if "I just threw [the report] back in the drawer." But he claims that would have been "morally incorrect."

As it happened, Frank found a receptive editor in Alex Dessler, a theoretician in space physics at Rice University. This proved to be a mixed blessing. Dessler had just taken up the reins of the *Geophysical Research Letters* in 1986, announcing on arrival that the journal would seek to publish more adventurous, controversial papers. He accepted Frank's original small comet report. In doing so, Dessler overruled the advice of two reviewers, sticking by his pledge to publish unorthodox theories, even knowing most would be judged wrong. But later, Dessler made it his personal mission to debunk Frank's thesis.

Dessler may have been egged on by the criticism of his peers. The space science community has been harsh not only on Frank's thesis but on Dessler's decision to publish it. And the tone of the opposition has grown sharp over the years. One physicist who asked to remain anonymous said, "I think a lot of people hate [Frank's] guts" for publishing the thesis and hold Dessler "responsible" for churning up a fruitless debate.

Indeed, says Penn State's Olivero: "Dessler has been criticized for allowing too much leeway" in the journal. Though Olivero found the debate stimulating, he says that "people have come after" Dessler. As a result, Dessler has expressed remorse for letting the debate run to 55 pages. Then, in the August 1991 issue of the *Reviews of Geophysics*, Dessler wrote a long attack on Frank's theory, summing up all the faults that others have pointed out over the years, concluding that "things look bleak for the small-comet hypothesis." In a phone interview, Dessler described this coup de grâce as "my penance" for having started the debate in the first place. (Frank and Sigwarth have written a 66-page rebuttal that's now under review.)



Comet's tale. Physicist Louis Frank interprets dark spots in an ultraviolet image of Earth *(right)* as small comets raining down at a rate of 20 per minute—but many of his colleagues disagree.

What really annoys some critics is the sense that Frank is so competent and experienced that he "should know better" than to print such hard-to-swallow ideas, says Bruce Cragin of the University of Texas, Dallas. Many of Frank's peers just wish the subject would go away. Olivero thinks some people view it as "not science" at all but a kind of theological debate.

A few of Frank's colleagues say they are more disappointed by the community's harsh response than by Frank's argumentation. Olivero feels this way. So does John Murphree, a physicist at the University of Calgary, and principal investigator on a Swedish satellite similar to DE. Murphree disagrees with Frank's thesis, but says Frank has "been dealt a disservice by the community at large," because it has responded so "negatively." Frank "has been very imaginative" in responding to comments, says Murphree, following all the rules of scientific discourse, while the critics have been "very aggressive" in attacking him.

Frank insists in a popular book he's written about the controversy (*The Big Splash*) that he "made no adjustments" in the theory merely to escape criticism, as his critics accuse him of doing. And he says the debate has become personalized because of Dessler's stake in repairing his own reputation. The critics have tried to ridicule him, Frank claims. He points out that *EOS*, the official newsletter of the American Geophysical Union, has never covered the substance of the small-comet debate. But last March it ran a photo of Dessler's dog, describing him as a loyal believer in the small-comet thesis.

"Everybody wants to prove me wrong," says Frank, and that makes the discussion "a lot more emotional" than it ought to be. Does he think the pressure has made it hard for him to read the evidence objectively? No, Frank says, all it takes to disprove the thesis is some solid physical evidence. "If somebody comes up with a real definitive experiment, like a good imager with real good time resolution, that shows [the comets] are not there...I've got no problem saying, 'Well, that's the way it is.' " Besides, he adds, "my life is not small comets."

Indeed, even one of Frank's severest critics, Thomas Donahue of the University of Michigan, Ann Arbor, says: Frank's "achievements are enormous. . .I'm a member of the [National Academy of Sciences], and I don't mind saying I think Louis should be a member." But Donahue also thinks "it was a great pity that a person of his stature and his achievements got off on this track." The story of Louis Frank suggests that whether an investigator is wrong or right, the investments that he—and his critics—develops can make it very hard to weigh the evidence coolly and calmly.

–E.M.

PEER REVIEW

NSF Deals With Conflicts Every Day

Intellectual conflicts of interest may seem a rarefied topic, but they are a daily concern for institutions such as the National Science Foundation (NSF). Like other granting agencies, the NSF must rely on peer committees to evaluate the quality of research, and the agency counts on reviewers to put aside personal interests while sitting in judgment. And in dealing with intellectual conflicts, the agency's mission must not only maintain a review process that is fair, it must also ensure that the process appears fair to the outside world.

The dual assignment of ensuring both the actuality and the appearance of fairness comes up in a variety of cases stemming from the fact that the research community in any field is likely to be small. James Hays, director of the NSF's earth sciences division, says, "It's not at all unusual" to discover that someone serving on a grant review panel for a program is seeking a grant from the same program. The agency can't eliminate these overlaps altogether, but NSF does, according to Hays, take steps to minimize conflicts. If a scientist is sitting on a panel judging proposals—and at the same time seeking funding—the panel is not asked to judge that person's proposal. Instead, NSF sends it out for an extensive mail review.

This technique may limit the potential for conflicts of interest, but it cannot eliminate the potential altogether, since peer review by its very nature requires scientists to make decisions that affect the careers of friends and competitors. As Daryl Chubin of the U.S. Office of Technology Assessment writes, peer review "pits competitors for scarce resources against each other in interchangeable roles: proposer and would-be authors one moment, reviewers and referees the next." One step that might help, in Chubin's opinion, is for reviewers to sign their comments. He argues that "anonymity, confidentiality, and secrecy merely obscure inevitable differences among the negotiating parties, thus forcing such conflicts to be worked out in a 'backroom,' hidden process."

But Chubin's idea for bringing peer conflicts into the open hasn't caught on. Even when he tried to apply it in a journal he was editing, Chubin says, he found that "surprisingly few" would agree to sign their initials. The usual reason given for this reluctance, Chubin says, is that "relationships among reviewers and the community are so fragile" that many would rather not write candid reviews if their identities were known.

Beyond techniques like not allowing a panel to review a grant proposal from one of its own members, NSF—and other government agencies—must rely heavily on the judgment of program officers to handle potential conflicts. But those staffers need to know of hidden conflicts, too, which is why NSF—and most other agencies—now require reviewers to fill out detailed forms on their affiliations and investments, and on spouses' interests. They are also asked to disclose any "perceived" conflicts, and they may be asked to recuse themselves from voting on certain proposals affecting their own institution.

But the program officers themselves don't come on board automatically understanding their new role as referees. Most are coming out directly from the scientific community, where they may have received little formal training in such matters. To help new program officers understand their role, the NSF holds a special seminar each year, run by attorney Robert Crangle. The aim is to instill a sense of fair play. "Almost by definition," says Crangle, someone coming for the first time from the research community to the NSF staff "will have scientific biases and believe in a certain subtheory or sub-discipline." In a 3-day seminar, Crangle educates the newcomers to "the culture" of grant competition, as well as to the rules of science funding, using case studies to highlight typical problems. After that, the program officers are left to rely on their own judgment and discretion.

Such judgment may or may not be an adequate safeguard against intellectual conflicts of interest. But the existence of seminars such as Crangle's, along with the other methods developed by NSF, shows that the community has been dealing with intellectual conflicts far longer and more explicitly than it has been struggling with financial conflicts, particularly those that affect cutting-edge biology.

–E.M.

HISTORY OF SCIENCE

Intellectual Conflicts–Boon or Bust?

Advancing science through intellectual conflict of interest? This may seem counterintuitive, but history proves that reliance on personal conviction can advance a field, not simply lead it down a blind alley. Take astronomy, a field that has produced many intellectual conflicts over the centuries, perhaps because in this field facts are often much tougher to come by than theories. As a result, commitment to a particular theory often involves a subjective element, and, as two examples from the 1920s show, the subjective element may be a boon as well as a bust.

Harlow Shapley, the Harvard University astronomer who in 1920 demonstrated that the sun is not at the center of the Milky Way and that the Milky Way is much larger than anyone had realized, provides a prime example of subjectivity turning into success. Historian of science Owen Gingerich says that today some of Shapley's methods might seem "wildly arbitrary." To cite one example, Shapley "threw out" some measurements from variable stars that just didn't seem right to him. Shapley didn't fully explain why he did it. But in retrospect, Shapley's instinct was right, although some of the details in his work were wrong.

"At the cutting edge [of science] a certain kind of intuition assists genius," notes Gingerich, a faculty member at the Harvard Smithsonian Center for Astrophysics. If Shapley had done his analysis using every scrap of data available, he might have produced a balanced but inconclusive report. Instead, says Gingerich, Shapley followed intuition and was able to "plow boldly ahead and make a dramatic breakthrough."

But that doesn't mean reliance on conviction is always the route to scientific success. Adriaan van Maanen, a contemporary of Shapley's at the Mt. Wilson Observatory in California, claimed he had detected a large, internal spin in the Andromeda and other nebulae, which van Maanen, with many others at the time, thought was relatively nearby. One of his colleagues, Edwin Hubble, argued that, on the contrary, Andromeda was a distant galaxy. If Hubble was right, the nebula couldn't be spinning at the rate van Maanen calculated, for that would imply that parts of the galaxy were moving faster than the speed of light.

At the request of Mt. Wilson officials, Hubble refrained from publishing his critique of van Maanen, as Norriss Hetherington relates in a new book on this subject.* But later, when a debate erupted over the distance of this and other nebulae from Earth, Hubble brought his observations forward, and others began to look closely into van Maanen's research. Hubble's evidence showed that Andromeda and other nebulae were actually far-off galaxies and that they could not be spinning as rapidly as claimed. By the 1930s, Hubble's view was accepted as correct.

Since then, astronomers have pored over van Maanen's records to try to figure out how he came up with his detailed but erroneous results. In the end, says Gingerich, people have decided that van Maanen's work was affected by "observer bias." "No one ever accused him of consciously distorting the readings," says Gingerich. But van Maanen interpreted the data in a way that confirmed what he wanted to find. His personal investment in the research came into conflict with his analytical skills, and in the end it was science that lost.

–E.M.

*The Edwin Hubble Papers, Pachart Publishing House, Tucson, 1990. Edited and with a historical introduction by Norriss S. Hetherington.

Conflicting Views: The Readers Respond

Because the problem of conflicts of interest is such a new one in many areas of biology, *Science* is interested in obtaining your views of how specific conflicts should be handled. Please indicate your responses to the questions below and fax or mail your form to *Science*, using the information at the bottom of the form.

1. You are the editor of a major scientific journal. A prominent scientist calls and offers to write a review article on the genetics of and therapy for a disease he has been working on for 20 years. You have heard that the researcher has major equity holdings in a company developing a genetic therapy for the disease. What should you do?

a) accept the offer without question
b) send a disclosure form that asks the researcher to list any affiliations that might be relevant to his research—and publish them with the article
c) accept his offer, but have the article reviewed by people who know about his equity holdings
d) other ____________________

2. You are a university researcher hot on the trail of a new genetic method that could have enormous commercial potential. At the same time, you are a major equity holder in a company that could profit from the method. You submit a paper on the basic research relating to the method to a major research journal. Should you disclose your commercial affiliation?

a) yes, immediately and without being asked, because the journal's editors ought to know—and it's up to them to inform their readers if they choose
b) only if the editors of the journal request the information
c) only if the journal's editors ask, and then only if they do not intend it for publication but only for their information
d) disclose it voluntarily—and insist that the information be published
e) no—it's a private matter
f) other____________________

3. You're a section editor of a journal and also serve as a consultant to a hot, new biotech company. A manuscript is given to you for review written by the scientific founder of a company that competes with the one you consult for. What should you do?

a) arrange to have the paper reviewed as you ordinarily do
b) inform the journal's editor of your potential conflict but arrange for the review as you ordinarily do
c) arrange for one of your colleagues to coordinate the review
d) send the paper back to the journal editor, saying your conflict prevents you from arranging the review
e) other ____________________

4. You're the keynote speaker at a scientific meeting, describing your work on a new RNA-based technology with great commercial potential. You also are chief scientist for a company formed to commercialize that technology. When you speak should you:

a) disclose verbally that you have a commercial interest in the research you're describing
b) project a slide that describes your commercial affiliation
c) do neither of those, but be prepared to answer questions on your commercial affiliation
d) none of the above—it's a private matter
e) other____________________

5. Would you be willing to serve as a referee for a major journal if that journal required you to disclose (to the journal's editors) all investments or financial ties to companies relevant to your field of research?

☐ Yes ☐ No

6. Would you continue to publish in a major journal if that journal required that you publish a disclosure statement listing any financial ties you might have to companies closely related to your field of research?

☐ Yes ☐ No

7. Do you think there are intellectual conflicts of interest sufficiently serious to warrant disclosure?

☐ Yes ☐ No ☐ Not sure

8. If you answered yes to question 7, which of the following might warrant disclosure, and in what form?

a) you're a member of an advocacy group (the Sierra Club, say) and your work has public policy implications
b) you have very strongly held political views that could be seen as relevant to your scientific work
c) you have a deep intellectual commitment to a view that is considered quite extreme by almost everyone in your field—but you're sure it's correct

Name and title: ____________________
Institution: ____________________
Field of research: ____________________
Years in the field: ____________________
Telephone number (optional): ____________________

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