But other researchers aren't persuaded that Roy's technique is all that new. Crystallume, a Menlo Park, California, firm, has several patents for techniques that produce "diamond ceramics" by subjecting a preshaped aggregate of diamond particles to CVD conditions. Even Roy's more basic speculation that the Penn State synthesis might be occurring by a novel solid-to-solid transformation, which has never been seen at the low pressures of the technique, has its doubters. J. Michael Pinneo of Crystallume and others suspect instead that carbon from the solid is probably first vaporizing into gaseous fragments and then redepositing a diamond onto the solid's surface. That would make it "a variant on a lot of previous [CVD] research," says John Angus of Case Western Reserve University, one of the field's most respected practitioners. Still, "if he can convert large pieces of porous graphite into diamond, this would be positive," Angus adds.

The kinds of expert judgments Angus and Pinneo were offering are precisely what constitutes peer review. In considering what inspired the Penn State diamond makers to short-circuit that process, clues might be found in market analyses that project synthetic diamond to become a multibillion dollar business by the end of this decade, a fact Roy pointed out in the press conference. The MRL, which coordinates a multicompany diamond research consortium, is well aware of the financial stakes—and the zeal of their worldwide competition. Hence the PR-for-peer-review switch, which Roy and MRL director Russel Messier say was blessed by the university's provost and by patent attorneys. "We [at universities] have not been efficient at converting research into patented, protected technologies," says Messier. "This [PR campaign] is forging new policy at Penn State for protecting important results."

MRL's self promotion has generated results: The *Wall Street Journal* wrote a piece based on Roy's press release, as did several magazines including *Science* and *Chemical & Engineering News*. Only time and scrutiny by other researchers will decide if the science Roy so eagerly publicized last week will prove as innovative as his PR tactics.

Peer Review: Treacherous Servant, Disastrous Master

CHARLES W. McCUTCHEN

Peer review, out of control, makes science a jungle where politics rules and fraud is tolerated.

Science has become a profession: grants and research contracts are what it lies on. Whereas a rich dilettante like Lord Rayleigh could retire to his country estate and do acoustics or whatever else he wanted, modern scientists must sing for their supper. They do not sing to their patron, the U.S. tax payer. They sing to other scientists, who wield over them the power of professional life and death via peer review.

Peer review, the evaluation of a specialist's work by others in the same field, is an inevitable consequence of specialization. Example: though anyone can tell if a bridge design is truly bad—the bridge collapses—it makes sense to have other engineers check the plans before the bridge is built. Science uses peer review to determine which projects to pay for and which articles to publish, and, recently, to judge cases of alleged misconduct.

Peer review suggests trial by a jury of one's peers, a jewel in the crown of Western democracy—surely an excellent model. But it takes more than a jury to have a fair trial. A lynch mob is also a peer panel. Rules and procedures—jury selection, rules of evidence, the requirement that evidence be heard in public—and a judge to interpret and enforce them are necessary if fallible people are to render fair decisions. Specialist peer review is fraught with biasing influences. Specialists compete with one another and, at the same time, fight collectively for their profession.

Peer review is at best a treacherous servant, but scientists often forget that a jury trial is more than a jury, and act as if the use of peers automatically sanctifies the resulting decisions. Establishment scientists have been treated well by peer review: scientific administrators use it. Both want to believe in it, and the need engenders beatification by faith. "Peer review is the distinguishing characteristic of science," they say. "It makes science what it is."

They are right—in a way. Every scientist is an informal peer reviewer. A scientist's work affects science only if others accept it. But formal review of grant applications, manuscripts, and fraud allegations also makes science what it is, and here human failings can yield improper decisions whose practical consequences and poor ethics propagate throughout science.

Peer review resists investigation. Only insiders know the details of each decision. They may not tell the truth, and the technical background needed to extract the facts is hard for outsiders to learn. Lacking the omniscience of Orwell's Big Brother, we must be content with horror stories of reviewing gone wrong. Though such stories do not directly reveal the frequency of mistakes, they show which human failings are involved, and thus the likelihood of trouble and how to reduce it.

PEER REVIEW AND GRANT GIVING

The federal government uses a variety of ways to decide how to fund science. Department of Defense (DOD) managers can fund whoever they like, without having to get advice. They do not compete for contracts with the scientists they might choose. Instead, they shine in the success of the programs they manage, and should something go wrong in a program, the manager is responsible. These are all good features. Unfortunately, managers are subject to agency politics.

As consultant to a small firm, I watched the Navy fail to give a fair hearing to our best idea, *Sea Knife*, a fast boat of strange but simple shape that rides smoothly in rough water. We decided that the Navy's small-boat people would not admit that a craft by outsiders might be better than theirs. But having figured a way around this obstruction, we were funded to build *Wavestrider*, a faster though rougher-riding and more complicated boat. We got unrelated contracts to explore far-out and ultimately unsuccessful forms of marine propulsion. I think these

were funded because they threatened no powerful group within the Navy.

At the National Science Foundation (NSF), too, managers make the final funding decisions but with the advice of peer reviewers. Managers benefit from the peers' specialized knowledge but have the authority to correct for peer bias. As at the Defense Department, should something go wrong, the program manager is responsible. For years, George Koo Lea, director of the fluid mechanics program, supported Van Chao Shein Mow, now of Columbia University, for work that was never novel and true at the same time. Workers in his field, the lubrication of animal joints, who disagreed with the professor had trouble getting funded by NSF.

At the National Institutes of Health (NIH), where I have worked since 1964 in biomechanics, optics, and fluid mechanics, peer reviewers effectively make the final decisions; managers are nearly powerless. In each discipline, a peer panel—the study section—evaluates grant applications. By secret ballot, each panel member gives an application a numerical score, and these scores largely decide its fate. An upper, advisory council can fund projects slightly out of the order of their scores without attracting comment, as can program managers. But when whistleblower Robert Sprague, a grantee for many years, did well in the study section but lost out at the advisory council, the event made news.

Since peer review puts a scientist's future at the mercy of competitors, is it any wonder that career issues are a respected, if unadmitted, influence on decisions? Would we not expect mutual assistance pacts to be accepted facts of life? Should we be surprised that politics is especially rife in disciplines funded by NIH, where the power of scientists over one another is essentially unchecked? Van Mow receives three-fourths of all NIH support of research in joint lubrication and still accomplishes little. For years, those with contrary views received none. Support for research on lubricin, the lubricating chemical in joints, ceased in 1982.

Since power over grants confers power elsewhere, dissent in joint-lubrication research appears only in unrefereed publications such as conference reports and public lectures or in journals of distant fields. Timothy Harrigan and his then supervisor, MIT biomedical engineer Robert Mann, made an important contribution to the theory of how cartilage in joints deforms. Refused by the *Journal of Biomechanics*, it was accepted by *Archives of Rational Mechanics*.

If NIH grant administrators made the final funding decisions, they could be called to account for permitting cronyism. But peers are the ultimate authority, and because they exercise this power in secret, no one is accountable. This unaccountability makes the NIH system attractive to management. When a Professor Mow seems to have an inside track, NIH blandly declares that his success shows that his colleagues think well of him. Whoever gets funded, NIH can say the decision was out of its hands. However deserving Dr. X from Rep. Y's district may be, administrators can say they have no way of influencing X's funding. Privately, NIH officials admit there is politics in study sections but say it is a price worth paying to insulate grants in biomedical science from national politics. The cost was surrendering control of funding to scientific politicians.

NIH has one potential lever. The executive secretaries of study sections, who are NIH employees, appoint section members and could use that power to suppress the politics. Although members, who serve for four years, cannot succeed themselves, they expect a large say in the choice of their successors. A section secretary could threaten, "If you misuse your power, your successor will not be from your faction." But such action would require support from NIH management, because section secretaries are not famous scientists. The support would not be given, since NIH conceals this power of appointment. The handbook describing the study sections says that their members are "selected by the NIH" but supplies no details. The impression given is that peer review is above the vulgar mechanics of the appointing process. Without support from above, an executive secretary would need great courage to stop a determined cabal from controlling NIH funding in a discipline.

So cronyism proceeds. In 1976, Mow and Peter Torzilli published two spectacularly erroneous papers on joint lubrication. NIH knew experts held the papers to be nonsense. They predicted such a rapid flow of fluid through the porous joint cartilages that viscous losses would have generated heat 100 million times faster than mechanical work was being done on the joint. Yet Torzilli replaced Mow when the latter left the Orthopedics and Musculoskeletal Diseases Study Section in 1984.

INHERENT FAILINGS OF THE PROJECT GRANT SYSTEM

Politics is particularly bad in biomedical research because biomedical scientists directly control the flow of money that supports their disciplines. But even without politics, today's grant system, in which scientists propose future research projects to an agency, would be bad. The system gained its popularity after World War II when there were fewer scientists and most projects were funded. But even in those flush times, the Office of Naval Research, Atomic Energy Commission, and National Science Foundation all refused Donald Glaser when he asked for funds to develop the bubble chamber, later the standard device for observing particles in high-energy physics.

The great ideas in science in the next few years will be those not yet thought of. The system ought to select people likely to think them, but, alas, it is inherently biased against such speculation. Granting agencies want certainty, and reviewing peers fear unexpected discoveries by their competitors. As NIH puts it in a pamphlet for grant applicants, "Reviewers prefer limited clear goals that can be realistically approached; rather than broad, multiple questions or vague goals the attainment of which is open to doubt."

The caution of officialdom displays itself in a 1940 report from the Gas Turbine Committee of the National Academy of Sciences: "Even considering the improvements possible . . . the gas turbine could hardly be considered a feasible application to airplanes mainly due to the difficulty in complying with the stringent weight requirements." Thus did great men, including engineer fluid dynamicist Theodore von Karman, evaluate the turboprop and jet engine.

The project grant system ignores the range of human talents. As well as inflicting anguish on inventive people, it has no official niche for promoters, people who make enterprises go, people like Vladimir Zorykin who persuaded RCA to spend its money developing television. The grant system does not eliminate such promotership; it just perverts it. Promoters pretend to be great and impossibly active scientists to get money in promoter-scale quantities. They sign every manuscript from their laboratories and accumulate reputations earned by the work of others.

The grant system disrupts the chains of authority and loyalty between scientists and universities. Each university scientist is like a pirate ship raiding the U.S. Treasury. The university provides docking space; in return, the scientist provides for his or her keep, and preferably more, out of grants. To the scientist, the university is a leech; to the university, the scientist is a prima donna. In the middle of these cross purposes, students are supposed to be taught.

PEER REVIEW OF JOURNAL ARTICLES

When peers referee journal articles, they perform a valuable service. They find mistakes and sometimes fraud, and they form a trial reader-ship whose reactions show what to change to hold a reader's attention. A referee who knows the field can clarify what is and is not novel in a manuscript. Competent reviews take hours or days of hard work and are a tribute to those who do them.

Unfortunately, the power of referees, usually anonymous, permits self-interest, jealousy, revenge, and other unworthy motives to influence decisions. Dozens, probably hundreds, of letters to the editor over the years show that nastiness in reviewing contributes to a general unpleasantness in the publication process and in science as a whole.

Reviewing weeds out good manuscripts as well as poor ones. Frederick Lanchester's 1894 circulation theory of how wings lift, Chandra Bose's photon statistics in 1924, Enrico Fermi's theory of beta decay in 1933, Herman Almquist's discovery of vitamin K_2 in 1935, Hans Krebs' citric acid cycle in 1937, and Raymond Lindeman's trophic-dynamic concept in ecology in 1941 all were turned down at least once. Charles Fourier and Gregor Mendel had trouble getting published. We will never know how many deserving manuscripts remained unpublished.

The time and energy spent fighting to be published are lost forever. Lindeman died before his article appeared, and the delay Almquist suffered may have cost him a share in a Nobel Prize. The discoveries by Fermi, Almquist, Krebs, and Lindeman were held up only for a short time, but the circulation theory of lift was delayed over a decade.

My experience has been similar. Since Lord Rayleigh's time, it has been known that the wave nature of light spreads the image of a point source into a blur whose shape on the focal plane is described by the two-dimensional Fourier transform of the lens aperture. The image projected by a square lens is a diamond-shape array of checkerboard squares. I realized that a Fourier transform relation between aperture and image also holds true in three dimensions. When I attempted to publish this fact in the *Journal of the Optical Society of America*, referees rejected it. The editor kindly published it in response to my plea. This relation is now the starting point for calculating the three-dimensional resolving power of confocal scanning microscopes.

This evidence is anecdotal, so, by current convention, those who find it uncomfortable can ignore it. But in 1977 Michael Gordon wrote in the *New Scientist* that Henry G. Small of the Institute of Scientific Information had found "a significant negative correlation between referees' evaluations of [highly cited chemistry] papers and the number of citations the papers subsequently received." Low citation scores followed high opinions by referees, and vice versa.

The inability of peer reviewers to judge good papers should be no surprise. A discovery is usually a better-than-his-or-her-average product of a brighter-than-average (or perhaps simply unusual) scientist; the resulting paper will likely be reviewed by an ordinary scientist, operating at an ordinary degree of inspiration, and possessing human imperfections. Truly novel papers may not be understood. Those understood will be envied and perhaps rejected with one excuse or another. In 1844, J. J. Yaterston tried to publish a paper that anticipated by several years the kinetic theory of Clerk Maxell and Ludwig Boltzmann. A referee pronounced it "nothing but nonsense, unfit even for reading before the [Royal] society."

These famous examples of rejected discoveries end with World War II. With the rise of grant-supported science, few manuscripts are unembellished reports of discoveries. A discovery is too valuable to reveal in a journal article until it has been used in grant applications. By the time most discoveries are published, they are already on the rumor circuit, and the papers announcing them include data generated in work the grants paid for.

It is follow-up papers that most scientists write and that referees are most likely to approve. A paper starkly describing something new looks strange and will be treated like the proverbial ugly duckling. An example: theoretical treatments of a plate planing on the surface of water like a surfboard demand that a sheet of fluid be ejected forward from under the plate. I found this not so in practice. Instead, there is a tumbling mass of foam where the plate meets the water. When I tried to report this in the *Journal of Fluid Mechanics*, none of the four referees disputed my findings, and three complimented my work. But the four were unanimous that my manuscript could not be published in the journal. One said my account was too sketchy even for a grant application. So far as I know, official fluid dynamics has not yet acknowledged the phenomenon, though my article is now Appendix D in *Planing* by Peter Payne.

Publication can lead to jobs and research support: NIH hired me as a result of my publications on joint lubrication. By denying publication to unadorned discoveries, refereeing obstructs this career channel and drives innovators to the granting agencies and ultimately to the establishment. A deadening uniformity is enforced. Dilettantes are squeezed out, not because they are bad scientists but because they do not belong to the union. This is a major loss. A Parisian gardener was the first to reinforce concrete with steel. Lanchester, inventor of the circulation theory of lift, was a mechanical engineer, not a fluid dynamicist. The inventor of the traveling-wave amplifier was trained as an architect, and two musicians invented Kodachrome.

Adding to the number of scientists by drawing from the fat middle of the bell curve of ability may retard rather than accelerate progress. As reviewing peers, the new recruits may silence and starve better scientists out of science. This happened to Douglas Kenyon, who once calculated the flow of water though joint cartilage. He now works for the Marathon Oil Co., and calculates the flow of petroleum through rock.

I call the cooperation of referees with the establishment an "evolved conspiracy." Referees, doing what their personal devils make them do, force innovators into the arms of the establishment, and the establishment is happy with this fact. Were it unhappy, changes would be made.

MISUSING PRIVILEGED INFORMATION

Reviewing of journal articles and grant applications gives reviewers the intellectual pleasure of interacting with authors and proposers, as well as education that, I suspect, has led to more advances than generally realized. These rewards are legitimate. Some rewards are not.

An obvious misuse of privileged information is rejecting or delaying a competitor's paper. The anonymity of referees ordinarily renders this untraceable. In 1978 Vijay Soman and Philip Felig rejected an article on anorexia by Helena Wachslicht-Rodbard and others to ensure priority for an article of their own. The action was detected only because the offenders plagiarized the reviewed article, and their manuscript was sent to Wachslicht-Rodbard for review.

Under cover of anonymity, reviewers can steal ideas from grant applications and manuscripts. There have been many private complaints by apparent victims. Theft is hard to prove, but it is known that the composition of the first material that was superconducting at the temperature of liquid nitrogen was leaked from a paper that Maw-Kuen Wu et al. submitted to *Physical Review Letters;* the leak was revealed because yttrium was wrongly called ytterbium in the manuscript. This error turned up on the grapevine.

A few proved cases do not show that stealing is common. But the rewards are large, especially now that professors must win grants to get tenure and promotions. It is bad form for victims to complain in public. Indeed, it is half-accepted that big fish will appropriate the success of little fish. Jocelyn Bell's discovery of pulsars won a Nobel prize for her superiors but not herself. There was an outcry but not of the size the injustice deserved, nor did the superiors seem embarrassed.

PEER REVIEW AND FRAUD

The current attempt to deal with scientific fraud is science's first brush with formal self-regulation. Self-regulation of any profession runs afoul of collective self interest and pack loyalty. When disciplinary committees operate in secret, these influences have full rein. Need I enlarge on the ineffectiveness of the disciplining of doctors by doctors?

Though a few fraud cases are famous, most investigations have been ineffective: a top NIH administrator told me that no university can bring itself to use the word "misconduct." He exaggerated. A very few small fry have been found guilty—for example, the unfortunate Lonnie Mitchell of Coppin State College in Baltimore. He had his grant application prepared by a professional writer who plagiarized someone else's application that Mitchell had provided as a model. Alas, the plagiarizee reviewed Mitchell's proposal.

The vast majority of scientists who stand accused before a university bar of justice are exonerated. Tim Beardsley recently reported in *Scientific American* that the accused was found guilty in only 16 of 110 cases completed by the Office of Scientific Integrity (OSI) since it took over as NIH's fraud squad in early 1989. According to Lyle Bivens, head of the Office of Scientific Integrity Review (OSIR), which oversees OSI for the Department of Health and Human Services (DHHS), NIH has reversed only one university exoneration. At face value, this says that most fraud charges are baseless, but we have only the word of the universities and OSI that this is true. Details of the exonerations, including the names of accuser and accused, are secret. (I am suing DHHS under the Freedom of Information Act in an attempt to lift this secrecy.) Where secrecy has been penetrated, exonerations have been found to be mistaken. Both the University of Wisconsin and OSI declared James Abbs innocent of Steven Barlow's charge that he had forged an illustration for a journal article by making a smoothed tracing of a figure in an article co-authored by Abbs and Barlow. *Neurology* has published a letter to the editor in which I demonstrated the relationship between the figures. Abbs' published response gave no satisfactory explanation of the resemblance.

A little-known case is revealing. The University of Medicine and Dentistry of New Jersey, and later OSI and OSIR, all told Gene L. Trupin that he was wrong in claiming that Barbara Fadem had stolen his research. OSI and OSIR ignored obvious signs of trouble. Just one example: in defending herself and other members of the university faculty against a lawsuit by Trupin, Fadem said that a journal article he and she co-authored proved that Trupin knew certain facts when the article was submitted. Court records show the facts in question were added to the article at the proof stage, 10 months after the date of submission. OSI knew about this dodge at the time it found Fadem innocent. It also knew that the suit was settled out of court in 1988 with a $60,000 payment to Trupin.

As long as NIH's watchdog is blind to evil when it wants to be, is it any wonder scientists learn that ethical pliability is a professional necessity, and find it prudent to discover that what looks like fraud is a "scientific disagreement," an "error," or "sloppiness"?

One might think a determined whistleblower could force OSI to conduct a real investigation. Not so. Once OSI receives an accusation, it tells the whistleblower little or nothing. As the whistleblower who got the Abbs case reopened, I was volunteered no information: OSI's predecessor office did tell me to prepare a 10-minute presentation, but I was never summoned to make the presentation, nor told it was called off. Meanwhile, OSI's impenetrable secrecy encouraged Abbs to complain that his constitutional rights to due process were being trampled. He sued and won on a technicality. DHHS is both appealing the verdict and, as the judge required, going through the steps laid out by the federal Administrative Procedures Act.

Universities routinely use peer panels to investigate and judge fraud. This shifts responsibility but does not get justice done. A powerful accused scientist or pack solidarity can frighten a panel into seeing no evil. The panel that the University of Wisconsin convened to investigate Abbs' alleged faking ignored blatant inconsistencies in his submission. For example, Abbs falsely claimed that accuser Steven Barlow had displaced one record before comparing it with the other. The public gaze might shame a panel out of doing a whitewash, but panels operate in secret. Incredibly, in its filing under the Administrative Procedures Act, DHHS proposes that determinations of guilt no longer be printed in the *Federal Register*. Secrecy, secrecy, ever more secrecy.

Secrecy gives full rein to subterranean forces, and a major scientist can bring great force to bear. Panels at MIT, Tufts, and NIH all said, wrongly, that no misconduct was involved in a paper co-authored by Thereza Imanishi-Kari, Nobel laureate David Baltimore, and others. It is a matter of record that Baltimore used both a letter-writing campaign

and professional lobbyists in an unsuccessful attempt to get Congress to halt Rep. John Dingell's (D-Mich.) investigation of the matter. (It was Rep. Dingell's investigation that finally forced NIH to mount a real investigation of its own.)

Media interest in the Baltimore affair is more than instinctive celebrity chasing. Fake work impedes progress much more if a major scientist is involved than otherwise, because others must pretend to agree with it if they want jobs or grants. I know of no attempt by other scientists to duplicate the precise experiments in the Baltimore affair. Scientists supposedly delight in proving one another wrong, but they hesitate to embarrass someone with power and the willingness to use it.

Because no one at NIH is accountable for the decision to fund Professor X, no one feels betrayed, no one is angry or ashamed if X commits fraud. So NIH washes its hands of the matter and passes off the consequent cover-up as political realism. As an official in the Department of Health and Human Services said to me about OSI: "They have to compromise." Expedient exonerations are excused as being for the good of science. If the public got the idea that a lot of fraud exists, the argument goes, it might not support research. The whistleblower is, figuratively, given a loaded pistol and told to do the proper thing.

According to the *New York Times*, retired Harvard microbiologist Bernard Davis believes it would have been better had the Baltimore affair been dropped. The biomedical science establishment would rather let fraud continue than have it publicized, a policy that will keep fraud going forever. Concealment requires that the sinners keep their funding. Abbs and his laboratory received millions in government support after the initial brushing off of the complaint against him. So long as such scientists are protected and fed, their species will multiply.

By not using its control over who gets funded, NIH has given up the power that would go with being pay master. Despite signing 57 billion a year in checks for research, NIH was unwilling or unable to prevent MIT's whitewash of Imanishi-Kari, Wisconsin's of Abbs, and numerous similar instances.

Were NIH to invoke its power of the purse, a university might say it was applying improper influence, a confrontation NIH evidently fears. James Wyngaarden, ex-director of NIH, and Joseph E. Rall, ex-deputy director for intramural research, have both said that universities have run ineffective investigations, but NIH has never punished—or even tongue-lashed—them for doing so. Nor has it said that running a bogus investigation is unethical. Yet unless NIH greatly expands OSI, the agency will depend on university investigations of fraud.

Compare NIH with NSF, where managers make the final decision about who gets funded. With responsibility comes accountability—for such odd decisions as siting the National Earthquake Engineering Research Center in Buffalo, N.Y., rather than in California. One can also question the reasons for moving the National Magnet Laboratory from MIT to Florida State University. But whatever one may think of them, these decisions show that NSF has power. If NSF wanted a university to investigate a fraud, the school would remember the movability of laboratories before doing a whitewash. Perhaps this power is reflected in the apparent lack of fraud in the parts of science NSF funds.

TAMING A FRACTIOUS HORSE

• *Reform the Grant System.* Suppose politics could be eliminated from NIH study sections. Suppose DOD and NSF program managers were all smart and incorruptible. The project grant system would still be a time-destroying Moloch, demanding and reviewing long applications, most of which are not funded, and it would still sponsor sure things rather than imagination. Block grants to universities would be better. The schools would decide who to support however they wished, using any system they wished, from despotism to democracy. Universities have made good choices in the past. The University of Michigan found the initial, essential money for Donald Glaser's bubble-chamber research.

Each year, universities would go to the federal government and argue for support. Let them bring citation scores, rumors of Nobel Prizes almost awarded, whatever they want. Out of this free-for-all, a formula would emerge, no doubt with loopholes and exceptions, and the negotiators would return home exhausted and tell the troops how they made out. The mutual dependence of scientists and brass would develop the loyalty upward and downward that makes institutions bearable to their members.

Under the block-grant system, everybody in a university would be in the same lifeboat and would benefit collectively from one another's success. Still, researchers would continue to compete within the school, so to dull the teeth of university politics, perhaps 10 percent of federal support should remain as grants to individuals.

• *No-fault Publication.* Specialist journals should never reject. If scientists are worth paying, they are worth hearing from. A referee who thought a paper wrong could try to argue the author out of publishing it, invoke a six-month cooling-off period, impose a length limit of

a page or two, and have signed comments published along with the paper. If no-fault publication results in a flood of garbage, it shows that scientists are creating garbage. Better we learn about this than conceal it.

General-circulation journals like *Science* and *Nature* would still reject most manuscripts they receive. Their editors, not reviewers, should make the final decision. Editors are the filter that catches reviewer misbehavior. Essay-form reviews can be windows into a reviewers motives, and having one reviewer from outside the specialty under review is a wise precaution against discipline politics.

Editors of all journals should ask reviewers to be as kind as possible, and authors should know the identity of writers of adverse reviews. A referee whose identity is known is less likely to steal from a paper, reject or delay it for professional advantage, or be pointlessly nasty. On the other hand, favorable reviews should be anonymous to discourage reviewers from trying to curry favor with authors. There is no way to keep them from informing authors privately, but the rule would remind them it is unethical.

• *Fraud.* The fraud problem reflects the ethics at the top of biomedical science. By not retaining for itself final authority over funding decisions, NIH left this power unguarded for ambitious scientists to pick up. With power came arrogance and the feeling that rules were for lesser beings. The cure is obvious. End the carving of their own cake by biomedical scientists, and the steamy politics will dry up.

If funding is not reformed, the scientific establishment will remain the problem, and the solution must come from elsewhere. John Dingell cannot interest himself in every fraud case, so the public's sense of fair play must be enlisted as a force for justice. Whistleblower and accused should know everything that occurs at every stage of an investigation so they can object and, if necessary, complain in public. The final conclusions of all fraud investigations should be made public.

If a peer panel has to make the final decision, as it might in cases of fraud, only extraordinary measures will yield justice. Because panel members are specialists judging fellow specialists, precautions beyond those in jury trials are needed to counter the effects of politics and pack loyalty. Accused and accuser, or their advocates, must have the right to question panel members in public about decisions before they become final. Unless these or very similar reforms are instituted, OSI should be closed, because it cannot yield justice.

Using peer review is like riding a fractious horse. One must understand its bad habits and never let it forget who is boss. Kept under control, peer review can yield good advice. Given its head, it will hurt people, serve the interests of the reviewing peers, and warp the institutions that use it. Where possible, peers should not make the final decisions but should advise the decision makers, who can filter peer self-interest from peers' recommendations. As a fractious horse is only as good as its rider, peer review is only as good as the program managers and editors who use it, but these people are visible and can be called to account for their decisions.