

Harvard Delays in Reporting Fraud

Six months passed before officials made public a confessed case of fraud, and then only after NIH questioned suspicious data

In May 1981, several young researchers at the Harvard Medical School watched in astonishment as one of their colleagues flagrantly concocted data for an experiment. Here was proof, the stunned eyewitnesses later told their superiors, of something they had suspected for months—that their colleague was systematically faking a major part of his prodigious output.

John Roland Darsee, 33, with nearly 100 published papers and abstracts to his name, confessed to that one case of fraud but denied any other improprieties. Faced with a serious problem, Harvard Medical School authorities, including dean Daniel C. Tosteson, immediately relieved Darsee of his position and then began an examination of his vast opus. The audit, conducted over the course of the summer and fall, revealed that the original data for many of Darsee's experiments at Harvard were unaccountably missing. Despite the increasing gravity of the situation, Harvard authorities did nothing to make it public. Moreover, Darsee, who had been stripped of his position but not of his association with Harvard, continued to work at the lab and publish papers as if everything were just fine.

Harvard took public action 6 months after the admitted falsification, but only when the National Institutes of Health (NIH) compelled it to account for scientific discrepancies in a \$724,154 study in which Darsee played a major role. The NIH study was designed to assess whether certain drugs limit the damage done by heart attacks. In answering NIH queries, Harvard officials explained publicly for the first time that the problems had to do with Darsee and his inability to produce certain critical raw data.

Harvard then withdrew his results from the study, canceled his presentation of papers at a meeting, released a brief statement to the press about the admitted falsification, and formed a blue-ribbon committee to look into the matter and make recommendations about possible further action. A summary of the findings will be issued in February. NIH is also investigating the affair.

Even though Harvard officials began their investigation of Darsee's work in

May, it was not until NIH questioned results that Harvard discovered the dearth of raw data for the NIH study. At least one faculty member has criticized Harvard for delays and lack of thoroughness. Not the least troubling question in the Darsee episode is why he was allowed to remain in the lab for 6 months, especially considering the gaps in his raw data and his colleagues' contentions that much of his work had been systematically created out of whole cloth.

Harvard asserts that everything published by Darsee has been "verified," but a detailed recounting of the story raises questions on the issue.

If some of the work with Darsee's name on it were to be expunged from the literature, as might be the recommendation of the blue-ribbon committee, it would be a blow to Darsee's mentor and chief coauthor, Eugene Braunwald, 51, holder of one of the most distinguished chairs of medicine in the nation and a member of the National Academy of Sciences.

Much of Darsee's work was aimed at a hot clinical topic—how drugs and other interventions can dramatically aid recovery from a heart attack. Until the 1960's, it was thought that when the blood supply to a portion of the heart muscle was sharply reduced, the muscle died and there was little or nothing to be done about it. However, in the past decade research has shown that there is a borderline zone that may recover, and that the zone at times can be extended with various treatments.

Darsee arrived at Harvard ready to address such problems, already much praised for his quick and elegant work. He received a medical degree from Indiana University in 1974 and then proceeded to Emory in Atlanta for various residencies, attaining a remarkable pace of publication toward the end of his stay. His papers, according to at least one with whom he worked, were nothing short of magnificent. "I think John was the outstanding person I've worked with in 13 years on the faculty here," says Donald O. Nutter, executive associate dean of the school of medicine and one of Darsee's many coauthors. "He was brilliant, intellectually brilliant. He was

the hardest working individual that I've ever come in contact with."

In July 1979, Darsee joined Braunwald, the Hersey Professor of the Theory and Practice of Physic and physician-in-chief of the Peter Bent Brigham and Women's Hospitals. Darsee worked doggedly to advance his career, spending most of his waking hours in the lab. He was paid \$22,000 in 1980 on an NIH fellowship in addition to his small Harvard salary. It was a pittance compared with his earning potential had he entered the private practice of medicine. What Darsee chose instead was the prestige, status, and honor of performing research in an internationally respected lab.

Eugene Braunwald came to Harvard in 1972, his vitae at the time listing more than 450 papers and scores of memberships, honors, and awards. At Harvard, Braunwald's biomedical empire expanded, and along with it, new opportunities for his students. By 1980 he had pulled off a remarkable consolidation that joined his own department at the Brigham with that of the Beth Israel Hospital.

Darsee flourished in this environment. Between 1979 and 1981 he published close to 100 papers and abstracts, many of them coauthored with his mentor. When Braunwald's two-volume, 2000-page magnum opus came out in 1980 (*Heart Disease: A Textbook of Cardiovascular Medicine*), Darsee was the coauthor with Braunwald of the chapter on diseases of the pericardium. In 1980, Darsee was made an instructor at Harvard's Department of Physiology, and a full-time appointment to the faculty seemed not far off. Best of all, Braunwald, who already presided over two complete laboratories, at least \$3.3 million of NIH funds, and nearly a score of researchers, was considering setting up a separate lab for Darsee at Beth Israel. In the competitive world of Boston biomedicine, a promotion of this kind, at Darsee's early age, would ensure a dazzling career.

Such was the situation in the spring of 1981. Yet there were those who were far from enthralled by Darsee's rapid climb up the biomedical ladder and his prodigious record of publication. The people

with whom he worked daily—two fellows and a technician in the lab—were suspicious of his vast output. "This thing did not come up over night," says Edward Brown, 34, who has since gone on to a faculty position at Stony Brook. "You suspect it for months." Based on work they had seen Darsee perform, Brown and the two others deduced that an abstract Darsee was preparing to send off for publication contained not a shred of actual research. According to Robert A. Kloner, who runs the lab at the Brigham, the three came forward on 18 May with charges of fakery.

Kloner is listed as a coauthor on many of Darsee's papers, and the allegations must have initially sounded farfetched. "They came to me," he says, "with their suspicions but no proof." Kloner nonetheless investigated and found what he calls "irregularities." He then asked

Darsee to show him the raw data—the mass of material that, in a cardiovascular lab, can include preserved sections of dog hearts, electrocardiogram tracings, lab notebooks, and myriad measurements of blood flow and chemical composition. Darsee said he would pull some of them together.

What he did instead, according to Kloner, was to go into a room within the laboratory complex and start taking hemodynamic readings off one of the dogs in an experiment. As the chart paper came out, he marked it day 1, day 2, and so forth, making it look as though the data had been taken over the course of several days. All the while, several of his colleagues were watching the performance in awe, not believing Darsee would ever be so brazen. When later confronted by Kloner, Darsee admitted to the falsification but said he did it only be-

cause he had thrown the real raw data away. This short retention of raw data was for an experiment allegedly performed only months earlier. The exact aim of the fudged experiment and the names of the coauthors on the abstract have never been made public. Soon after the admission of guilt, the fellows told Kloner they suspected Darsee was cheating in a systematic way in order to achieve results so consistently clean and voluminous.

How could such a situation ever have occurred? A lab chief might well be in the position of knowing first hand what work was being done in the lab, although this is not always the case. In any event, it was not Kloner's responsibility to monitor the generation of all the raw data. Part of the problem was that the lab was populated with a loose federation of aspiring young researchers—often working

Coping with Fraud

A key issue in the Harvard affair is whether the senior members of the lab were sufficiently prompt in bringing Darsee's admitted falsification to the attention of federal officials, in subjecting his vast opus to critical scrutiny, or in alerting the scientific community to potential problems. Harvard is leaving judgment on these issues to the blue ribbon committee. "We had confidence in the work that was published," says Kloner, "but that is part of the purpose of the investigation: to help us determine what additional measures are necessary." Asked if those measures might include the retraction of papers, Kloner replied: "It depends on what they feel. I think the people on the committee are high-powered and we will respect their opinion. . . . There are no rule books that one can go to. That is part of the purpose of the committee."

Though rule books do not exist, other institutions have dealt with incidents of falsification in a manner quite different from Harvard. In the celebrated 1974 case of the patchwork mouse, Robert A. Good, then president of the Sloan-Kettering Institute for Cancer Research and mentor of the scientist under fire, relieved William T. Summerlin of his post on the same day as his admission of fakery and proceeded to launch an investigation into all his work. "Good knew," according to a book on the affair, "that his stance must be one of rigid adherence to the code that binds all scientists together. Under this code, any misrepresentation of an experiment would jeopardize the offender's career unless and until a committee of his peers has investigated the matter and found him blameless." Less than 2 months later, a five-person committee from Sloan-Kettering recommended that Summerlin's "association with the Institute be terminated."

One of the most publicized cases occurred in 1980, when a junior researcher at Yale medical school, Vijay Soman, in the course of a scientific audit admitted to falsifying work in a single paper. One week later, he was asked to resign by

his boss and coauthor, Philip Felig. All the data from his lab were immediately impounded in preparation for an audit by a researcher from outside Yale (*Science*, 3 October 1980, p. 38). When the audit revealed not only scientific problems but a dearth of raw data, papers by Soman and Felig were retracted, eventually a dozen in all. Most of these retractions were the result of questions, not proof, concerning duplicity. Raw data were available for only three of the retracted papers. The rest had come under a cloud.

A point on which Yale officials have been criticized is the delay with which they investigated the initial charges, which were brought by a young NIH researcher. It took almost a year of pressure before the first audit brought about Soman's admission of fakery. During a congressional hearing on fraud in biomedical research, held in March and April of 1981, senior researcher Felig said that part of the reason for the delay was that he had ignored the raw data for the experiment under fire. "Accepting Dr. Soman's list of patients rather than examining the patient's charts was a mistake," he told Representative Albert Gore, Jr.'s (D-Tenn.) subcommittee on investigations and oversight. "A subsequent review of the charts failed to provide substantiation of the diagnosis of anorexia nervosa in at least one of the patients and this would have provided evidence casting doubt on the veracity of the data."

The Darsee affair at Harvard in some respects shows similarity with other episodes. Harvard officials, like those at Yale, seem to have neglected to speedily examine all the raw data—otherwise the problems with the AMPIM study might have emerged earlier. In other respects, Harvard stands in contrast. It waited 6 months after an admission of fakery before calling together its committee, and during that time allowed Darsee to remain in the lab with whatever data he had in his possession, raw or otherwise.

—WILLIAM J. BROAD

with little supervision. "For Darsee's earlier work," says Braunwald, "the reason we have complete confidence is that the raw data were inspected at the most fundamental level at the time they were gathered. But then his role changed. After 18 months or so it is not the custom to hold onto a fellow's hand at every turn." Even when supervision was close, it was carried out by the youngest of Braunwald's lab chiefs. Kloner, who runs the lab at the Brigham for Braunwald, is in fact a year younger than Darsee. Kloner himself had been a fellow in the lab until he was appointed chief in July 1979.

Kloner reported the misdeed to Braunwald at the end of the week, Friday, 22 May. A brief statement about the events was put out to the news media 7 months later by Tosteson. "An intense investigation was immediately undertaken, in the course of which the individual admitted this serious misconduct. His academic and clinical appointments at Harvard and at the Brigham and Women's Hospital as well as his Fellowship from the National Institutes of Health were promptly terminated. None of his work that could not be verified has been presented to the scientific community."

This statement is a condensed and in certain ways misleading version of what happened. Harvard indeed terminated Darsee's fellowship in June, but did not tell NIH why. There was, moreover, no immediate move by his superiors at Harvard to pull work Darsee had submitted for publication or to stop him from publishing other work—even though, according to Braunwald, the examination and verification of his work was not completed until that fall. Darsee may have lost his appointments in May, but these papers and abstracts list his address as the "Brigham Hospital and the Harvard Medical School." At the end of May, for instance, ten of Darsee's abstracts were accepted by the monthly journal *Circulation*, in anticipation of his presenting papers at the annual meeting of the American Heart Association (AHA) in November. Darsee is lead author on nine of the abstracts. Of the 4000 or so authors whose abstracts appear in the October *Circulation*, Darsee is topped by only one, who has 11 abstracts. According to AHA officials, the abstracts could have been withdrawn up until the very end of July.

Six of the Darsee abstracts are from Harvard, the rest listing his address as Emory. Five of the Harvard abstracts are coauthored with Kloner and Braunwald. In addition, papers by the trio kept appearing in publications during the

summer and fall. Darsee as lead author and Kloner as coauthor have a detailed, nine-page report in the October issue of the *American Journal of Cardiology*, describing drugs that limit the damage caused by heart attacks. In the November issue of the *Proceedings of the National Academy of Sciences*, a paper appears that is coauthored by Braunwald, Kloner, and Darsee. A line at the top of the article reads: "Contributed by Eugene Braunwald, August 3, 1981."

Perhaps Harvard officials thought that Darsee would eventually be rehabilitated. Certainly, Harvard took no public action during this 6-month period that would have hindered Darsee from resuming his career.

Stripped of his appointments but not of his association with Harvard, Darsee remained in the lab. In particular he was allowed to work on a \$724,154 multi-institutional study sponsored by NIH. This study was known as AMPIM (Animal Models of Protecting Ischemic Myocardium), in which drugs were tested on dogs to see if they could limit the damage done by heart attacks. In addition to Braunwald's lab at the Brigham, groups were participating from Duke, the Duke/

the trouble centered on an experimenter named Darsee. "They called to say they had a serious problem," says Jerome G. Green, head of extramural affairs at the heart institute, "and they were told, well, you're just going to have to go ahead and describe it and let it all hang out." At the meeting in Bethesda, lab chief Kloner laid out the whole story. Says Keith A. Reimer, a cardiologist from Duke who was at the meeting: "The data were too clean, and when you started plotting them to see what types of correlations would show up, based on the work of other labs, the correlations did not show. There were a number of inconsistencies. The Harvard people had no explanation, aside from the fact that John Darsee had been involved in the collection of the data."

The Harvard data also showed that the drugs under study—ibuprofen and verapamil—limited the damage to heart muscle when a coronary artery was blocked. Darsee, in fact, had published similar conclusions about ibuprofen in many of his abstracts and in the October paper he coauthored with Kloner. But while Darsee's data presented at the AMPIM meeting showed a protective effect for

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VA Medical Center, and Johns Hopkins. The study had been under way at Harvard since September 1979, and Harvard officials decided to let Darsee continue to work on the project throughout the summer. Their rationale, according to Tosteson, was that he had already made major contributions to the project and it was close to completion.

Late in September 1981, the Harvard team sent its portion of the AMPIM data to the NIH in preparation for a group meeting in Bethesda at which the participants would, for the first time, discuss unblinded results. (Darsee and Kloner in April had presented blinded results at an NIH meeting.) On 6 October, however, NIH called Harvard to say there were peculiar problems with the data from Braunwald's lab, and that the investigators should be prepared to discuss them at the group meeting on 30 October. Harvard at that time did not comment on the situation. It was not until 3 weeks later, just 2 days before the meeting, that Harvard confided to NIH officials that

the drug, the data from other centers in the NIH study did not. The final problem, according to Green at NIH, was the basis for the Harvard conclusions. "They said at the meeting that part of the problem was the lack of raw data. They did not have the raw data for the blood flow." Such data were critical for the study, because blood flow showed whether the drugs allowed more blood to get to the heart muscle. Harvard withdrew the data from the study.

The news about Darsee was now starting to circulate in the biomedical community at large.

In November, 6 months after Darsee admitted the falsification, Harvard took the steps that might be expected to follow instantly upon the discovery of scientific fraud and the hint of widespread problems. It was too late to withdraw the six abstracts printed in the October issue of *Circulation*, but Darsee was prevented from presenting the full explanation of the abstracts at the AHA annual meet-

ing, held from 16 to 19 November in Dallas. (He did, however, present papers at the meeting based on the four Emory abstracts.) And despite the new questions the AMPIM study had raised about the veracity of his work, he was still allowed to visit Braunwald's lab. In early December, Tosteson formed the blue-ribbon panel to look into the whole affair.

The attitude of Harvard officials during the unfolding of the affair had been based on the presumption of innocence. Darsee, a fellow with impeccable credentials and a bright future, had admitted to one act of fakery but denied any other improprieties. (Darsee, in a brief telephone interview, would neither confirm or deny this or any other point of the story.) The investigation, according to sources close to the lab, at first substantiated this faith. The first work they examined—that which had been carried out when Darsee first arrived in the lab and already had been published—looked perfect, raw data and all. It was only when Kloner and Braunwald began to go over more recent work that problems emerged with the raw data. "They began to find holes," according to one source, "and Darsee's defense was, well, you never told me to keep the data. And so they were left with a situation where it looked bad, but there was no definitive proof of systematic cheating." The period in which the volume of raw data starts to fall off is after Darsee had been at the lab for a little over a year—in other words, the point at which supervision relaxed.

Given the one admission of data fabrication, an observer might see the absence of supporting data for other research as shifting the burden of proof to Darsee. That is not what happened at Harvard. Tosteson attributes the delay to Braunwald's desire to "manage" the situation. "The record will show that Braunwald was extremely anxious to try and manage the situation in such a way as to fulfill the promise that he saw in Darsee," says Tosteson. "It has to do with the changing perceptions of the extent and nature of the falsification. It had to do with increasingly wide looks at what Darsee was doing." The question of innocence was rendered moot, however, when the AMPIM study compelled Harvard authorities to explain to NIH that they had a problem. Asked why the absence of raw data for the AMPIM study was not discovered earlier, sources close to the lab say that at that time of the NIH query—some 5 months after Darsee acknowledged the falsification—Kloner and Braunwald still had

Fear as a Form of Pollution

The Nuclear Regulatory Commission (NRC) ran into a legal surprise on 7 January when a federal court ruled that before allowing the power to be switched on at the Three Mile Island plant, the NRC must concern itself with popular fears about the reactor, regardless of whether or not the fears have a rational basis.

A group of citizens called People Against Nuclear Energy (PANE) won their petition that the NRC be required to consider the psychological stress it might inflict by permitting an undamaged reactor at Three Mile Island to resume operating. The reactor, known as TMI-1, is the twin of the one that went amok on 28 March 1979. As far as the NRC is concerned, the plant is ready to run, and no one has presented any rational case for delaying its operation.

Nevertheless, the U.S. Court of Appeals for the District of Columbia granted the citizens' petition and rejected the NRC's brief. PANE's petition was not based on anything to do with nuclear technology, nor on the substance of its own complaint that the public's health and well-being are at risk. Instead, PANE made its argument on strictly legal and procedural grounds. It said that federal environmental law requires the NRC to do something it has flatly refused to do: that is, to consider the social and psychological impacts of turning on the power.

The NRC has never recognized any responsibility to entertain this kind of public concern. When PANE tried to get the NRC to examine the problem of human stress last year, the NRC refused. NRC lawyers told the court that the agency's duty is to protect the public's physical health and safety, no more. The plant is safe on technical grounds, the NRC claimed. Public fears which are not based on technical considerations, the NRC argued, fall outside its purview. These fears should be handled by mental health professionals. The NRC insisted that neither the Atomic Energy Act (which deals with radiation hazards) nor the National Environmental Policy Act (which demands impact statements) requires the agency to deal with psychological problems.

The judges on the appeals court voted two-to-one to reject the NRC's logic. They told the NRC that it may not permit the utility to resume operation of TMI-1 until it has complied with the court's order. The NRC must first "prepare an environmental assessment regarding the effects of the proposed restart of the nuclear facility at Three Mile Island Unit One on the psychological health of neighboring residents and on the well-being of the surrounding communities." Then the NRC must decide whether the law requires the preparation of a complete environmental impact statement. After all that is done, the NRC may decide what to do about TMI-1.

It is not clear how far the court intended to go in requiring the NRC to take account of psychological factors in future cases. PANE framed its brief narrowly, saying that the stress found among the people near Three Mile Island was unique. In no other decision has the NRC had to consider the aftershocks of a bad nuclear accident, PANE argued. The court may seize upon the narrowness of this argument and apply the decision in like manner, focusing on the situation at Three Mile Island and saying as little as possible about the NRC's general procedures. But this seems unlikely, for the court order stipulates that the NRC must prepare a "statement of the reasons for its determination that psychological health is not cognizable under the Atomic Energy Act." The judges' opinion was not released at the time the ruling was given. It may not be out until late January.

The important question is whether other utilities which have not had an accident like the one at Three Mile Island will now have to come to grips with public fears of nuclear power when seeking a license. Although the NRC officially maintains that this will not be the case, it is already gearing up in private to develop an expertise in this area. The first workshop in how to evaluate psychological stress in large populations will meet under NRC auspices in Washington in the last week of January. This may mark the beginning of an entirely new field: psycho-environmental law.

—ELIOT MARSHALL

not gotten around to thoroughly examining Darsee's work in progress.

Braunwald says he has no doubts about anything that has been published. "The *PNAS* paper in November or any other paper that bears his name, I have total confidence in," he told *Science*. The nagging question, in light of the slow pace of the investigation done in areas such as the AMPIM study, is how much weight to put on these assertions. Perhaps only a detailed audit will be able to resolve the issue.

Had the situation been addressed with vigor right at the start, perhaps such outside advice might not have been necessary. "These things have to be dealt with in a forthright way," says David Dressler, a member of the Harvard biochemistry department who in the early 1970's coauthored several papers with a junior researcher who later admitted cheating. The papers were promptly withdrawn. "To have this thing circle back from the NIH raises questions of institutional behavior. In my own case, I never thought about handling it through the university but informed the scientific community right away. . . . The idea of somebody coming down to see the original data, implying that papers had been sent off based on work the senior researcher had never looked at, is quite amazing to me."

A major mandate of the blue-ribbon committee* is to pass judgment on Harvard's handling of the affair. But the chairman of the committee, Richard S. Ross, dean of Johns Hopkins School of Medicine, has already expressed some views on the issue. Said Ross, after the committee held its first meeting: "The Harvard people have been anxious to do it the way they've done it, to avoid letting it smoulder like it did at New Haven. I mean they've come clean, and they've got some external people to look at it. The idea is that we are going to do it vigorously and let the chips fall where they may."

Late in December, NIH decided it too will investigate the Darsee affair. A panel of five or six outside experts will travel to Boston on behalf of NIH during the early part of 1982, according to Green. "It is virtually certain that we will tell Harvard to go ahead and repeat the

[AMPIM] study at no cost to the government, but not until the investigations are over."

The Darsee affair is but one of several such incidents that have emerged with apparently increasing frequency during the past few years. One who has addressed the question of why such a trend may be occurring is Robert H. Ebert, former dean of Harvard Medical School and currently president of the New York-based Milbank Memorial Fund. "I don't condone this behavior," he says, "but certain things in our culture disturb me about all this. One is the enormous importance that is attached to the number of publications by committees that consider people for promotion. . . . And I don't want to be critical of any individual in this current affair, but I think that with the pressure to produce, the pressure for publication, I'm not sure the supervision is as close as it should be." Ebert also believes that such incidents are far more frequent than is often acknowledged. "There is a borderline falsification that is more common than anybody knows, in which you are anticipating the results you are going to get when you put in an abstract. That whole environment is bad. There should be such a great value put on accuracy that it would never occur to anybody to do that. It is kind of a moral issue of our times."

Perhaps Ebert is correct, and supervision is not what it has been in the past. The fellows in the lab at the Brigham were suspicious of Darsee's prodigious output, having sat next to him day after day. Their contentions of widespread fakery, however, at first met with a fair amount of skepticism. Perhaps, if senior investigators Kloner and Braunwald had been closer to the actual happenings in the lab, they too would have been more immediately concerned. Clearly, it was Braunwald's hope that Darsee would succeed at Harvard in a spectacular way. But such hopes are best fulfilled in an atmosphere of oversight and intellectual sharing that lies at the heart of the relationship between master and apprentice. As has been demonstrated in cases of dishonesty at other universities, a lack of guidance can cause such hopes to come crashing down in ruin. The various groups investigating the Darsee affair have a challenging mandate. They need to find not only why the incident at Harvard occurred, but how such episodes might be prevented in the future.—WILLIAM J. BROAD

A second story will report the conclusions of the blue ribbon committee investigating the Harvard affair.

Nonprofit Journals Share Mail Rate Boost

Nonprofit organizations lost much of their preferred status at the post office when their special mail rates were sharply increased effective 10 January. Rates for seven classes of so-called preferred users were boosted unceremoniously as a result of a cutback in federal funding. Scholarly journals published by nonprofits are affected by the increases. The average cost of mailing a copy of *Science*, for example, rose from 6.6 cents to 11.3 cents, up about 70 percent.

The rate increases resulted from a reduction from \$800 to \$614 million in the federal subsidy for preferred rates to the U.S. Postal Service. The 10 January rate rise reflected the acceleration of a planned step-by-step increase of preferred rates aimed at ultimately making the Postal Service revenues sufficient to cover the costs of handling preferred mail. The levels mandated for 10 January had been scheduled to take effect in 1987.

The increases affect second-class mail used for scholarly journals; third class, used for bulk mailings; and fourth class, used heavily by libraries. Colleges and universities will find it considerably more costly to mail catalogs and recruiting material.

For nonprofit publishers, no across-the-board increase in costs can be estimated because of the complex formula used to establish rates for individual publications. Because the decision to raise the rates was made just before Christmas and given little publicity, most nonprofits are still assessing the implications of the rise for their budgets. Among nonprofit publishers of periodicals, the increases will apparently hit weeklies harder than monthlies or quarterlies because of a relatively large increase in the so-called piece rate charged for handling individual copies of periodicals. E. G. Sherburne, publisher of *Science News*, a nonprofit weekly with modest advertising income, said that the rate increases would force a rise in *Science News* subscription rates.

Mailing costs for individual publications are set according to a complicated formula which imposes differing rates based on weight of pages devoted to editorial text and to advertising,

*Also on the committee are A. Clifford Barger, professor of physiology at Harvard; Baruj Benacerraf, a professor of comparative pathology at Harvard and president of the Sidney Farber Cancer Institute; Burton S. Dreben, professor of philosophy at Harvard; Saul J. Farber, dean for academic affairs and professor and chairman, department of medicine, New York University School of Medicine; Gerald Frug, professor of law at Harvard; Robert I. Levy, dean of Tufts University School of Medicine; and Joseph B. Martin, professor of neurology at Harvard.