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NEWS AND COMMENT

NAS: A Face-lifting for the Proceedings

In an off-the-cuff rating of scientific journals in the United States, the *Proceedings of the National Academy of Sciences* (PNAS) would inevitably rank high. However, there are those who contend that its prestige rests too heavily on the reputation of the academy itself and that the PNAS is not as good as it might be.

"Most people who take a critical attitude feel that much of what we publish is pretty poor," admits John Edsall, of Harvard University, who has just completed a tour of duty as chairman of PNAS's editorial board. Robert Sinsheimer, of Caltech, who succeeded Edsall as chairman, agrees that there is a need to "improve the quality" of the PNAS and to broaden the scope of its papers to elicit greater reader interest.

And so the editorial board recently began implementing its quality campaign, aiming a dart directly at NAS members themselves when it issued an edict: No member may submit more than ten papers to the PNAS per year.

Under previous policy, a member was at liberty to submit as many papers as he wished, secure in the fact that virtually all of them would be published. Early in the 58-year-old history of the *Proceedings*, the academy ruled out any thought of subjecting papers to a refereeing system, on the grounds that it would delay publication and, implicitly, question the judgment of the person who sent in the paper. (All papers published in the PNAS must be by, or sponsored by, an academy member who assumes total responsibility for their propriety and scientific excellence.) Unless some extraordinary circumstance prevailed, members' papers and those sponsored by members sailed easily through the PNAS's somewhat perfunctory editorial screening.

According to Jerold Last, managing editor of the PNAS for slightly more than a year, the editorial board could submit a paper to review if it seemed to be particularly poor or "inappropriate," but, in practice, the board has very seldom done this. The individual members were very much in control of what appeared in what is, acknowledgedly, *their* journal.

One indication that not everyone was satisfied with this state of affairs appeared a couple of years ago, when a new policy required that sponsored papers be reviewed by a third party, selected by the sponsor. Under that regulation, a written review must accompany every sponsored paper. Although a member may overrule a reviewer's negative opinion and submit a paper in spite of it, the member then has to explain the reasons for his action to the editorial board, in writing.

That attempt at quality control seems not to have produced the desired results—thus, the ten-paper limit and the demise of the presumption that everything an academy member submits is necessarily good. "The members will have to be more rigorous in judging the papers they submit," says Sinsheimer. "A few members have been submitting far too many papers and abusing the privilege."

Certainly a member who has used up his quota can get around the limit by asking another member to sponsor a paper for him. However, the assumption is that members will not go to that trouble for any paper bordering on mediocrity and, therefore, that the obvious dodge to the ten-paper rule will present no serious problems.

So far, the reaction to the new limit has been vanishingly small. Last reports that one member wrote to complain that the limit is too high, another that it is too low, and no one else has been heard from. "But," he says, "I really don't expect to hear much about this until later in the year, when members

suddenly realize they've used up their quota. That's when they'll feel the impact of this regulation." Some members wryly speculate that, once the impact of the limit hits, their fellows will withhold papers until late in the year and flood the academy's editorial offices with good material in the fall. If they're right, the December issue of the PNAS should be as rich as the Neiman Marcus Christmas catalog.

Although as many as 50 percent of the members never submit a thing to the PNAS, according to Last, others submit 25 or more papers a year. In response to an inquiry by *Science* about the effect of the limit, Robert Berliner, deputy director for science at the National Institutes of Health, quipped "It's going to be tough on members at the NIH." They are among those who have become accustomed to submitting large numbers of papers to the PNAS.

Robert Huebner, head of one of the largest and wealthiest programs in the National Cancer Institute (NCI), told *Science*, "I probably make as much use of the PNAS as anyone," explaining that he is frequently asked to sponsor papers from the more than 200 associates he has through his NCI contracts, "all of whom are good or else they wouldn't be my associates." Nevertheless, he says that he tries to submit only the best of the five or six he receives every week. The ten-paper limit, he says, is "good" and will get him off the hook, giving him an excuse for rejecting papers. He will be allowed only two more this year.

By forcing members to make more hard-nosed judgments about papers, the editorial board hopes it will not have to exercise its prerogative of reviewing (and possibly rejecting) manuscripts any more now than in the past. However, observers describe the board as a body "with a mind of its own" that is determined to see something come of its efforts. Its feelings about accepting papers dealing with therapeutic matters (see related item, p. 409) are indicative of this.

(The fact that the PNAS has been expanding rapidly in the last few years, growing at a rate of about 20 percent per year, also figured in the action to curtail indiscriminate submission of papers. However, the desire to improve quality appears to have been the primary motivating force.)

The PNAS editors hope the new limit will screen out papers that report comparatively routine experiments which, although perfectly valid, could just as well be published in specialty journals. That in itself, Last believes, would help make the PNAS more interesting to its 9000 subscribers.

In another effort to liven things up, Sinsheimer has just sent letters to all newly elected NAS members, "inviting" them to write a general review piece on the work that presumably earned them their membership. These reviews would serve the double purpose of introducing the members to each other and of expanding the scope of the PNAS to include discussions of biological subjects that physicists can under-

Academy Turns Down a Pauling Paper

In an extraordinary move, the editorial board of the *Proceedings of the National Academy of Sciences* (PNAS) has rejected outright a paper by Nobel laureate Linus Pauling, who has been an academy member since 1933. In the rejected paper, Pauling extends his ideas about the therapeutic value of vitamin C from the common cold to cancer.

As a result of its action against Pauling, the PNAS board has formulated a stringent policy about publishing papers that propose, or "come near to proposing," therapy for disease. It has also raised a thorny question about whether academy members have the *right* to publish whatever they wish in the PNAS.

On 1 May, Pauling received a letter from John Edsall, then chairman of the editorial board, indicating that the PNAS would not accept the paper because of its therapeutic implications. According to Pauling, his paper suggested that ascorbic acid may play a role in cancer control by strengthening tissues and, therefore, could be effective in preventing the infiltration of those tissues by malignant cells in the body.

Edsall, who makes no bones about having rejected Pauling's paper, points out that the editorial board considered the issue at some length during its spring meeting. It finally decided that, because of the broad clinical implications and the possible harm that could come to cancer victims, Pauling's paper simply could not be published in the PNAS. The official position is that the members of the editorial board were not competent to judge the soundness of the clinical aspects of the paper. In the future, members will be advised, as a matter of policy, that papers dealing with therapeutics should be submitted to medical journals.

As far as the right versus privilege to publish angle of this situation goes, Edsall concedes that many NAS members do view their access to the PNAS as a right. Certainly, he notes, the traditional policies of the board have tended to support that feeling. Pauling himself, for example, has published in the PNAS on vitamin C twice in the last 2 years. Even though his papers were accepted with what Edsall terms "extreme mental reservations," the overriding feeling was that Pauling had a right to express his views, in spite of the fact that most other NAS members took issue with their scientific validity.

Pauling has not yet decided whether he will challenge the board's rejection and take the matter before the full membership at its annual meeting next spring. But, he says of the board's action, "This sort of censorship is pretty dangerous." Furthermore, he maintains that the board's powers are *solely* advisory and cites passages from the history of the *Proceedings* to back himself up. In two places, Edwin Wilson, managing editor of the PNAS from 1915 to 1964, recounts the academy's decision not to referee member's papers but to leave it to the member himself to assume responsibility for the scientific merit of his publications. ". . . The powers of the board and the managing editor are clearly only advisory relative to what appears in the *Proceedings* . . . ," Wilson wrote. Pauling's contention is that, if the editorial policy is going to change, it must be with the accord of the full membership. At the moment, whether he is right, or even whether he will push his case, remains to be seen.—B.J.C.

stand and vice versa. A similar request to new members 10 or 12 years ago, Edsall recalls, was a resounding failure, because no one responded.

Invited papers dealing with broad, interdisciplinary social and political issues are also seen as a means of enlivening the PNAS. Sinsheimer plans to solicit these "primarily from NAS members," but not exclusively so. In the past, the PNAS has stayed away from papers dealing with policy. To an extent, the academy has also, and the proposed change in stance reflects the academy's expressed desire to address itself to public issues.

The idea behind these anticipated changes in the staid PNAS, Last comments, is to encourage NAS members and others to use their journal and read it. "We don't expect to compete for attention with *Science* or *Nature* just yet, but maybe someday," he muses.

—BARBARA J. CULLITON

Science in Britain: Research Councils Lose Some Autonomy

After 8 months of deliberations, the British government has given its cautious approval to a controversial plan for reorganizing British science. The effect of the decision will be to shift funds for science from the independent research councils to government departments, which will use the money to commission their own research. The plan represents an attempt to encourage more mission-oriented research in a system that has traditionally emphasized the freedom of the scientist to go wherever his curiosity takes him.

The plan was devised by Lord Rothschild, chief of a recently created advisory group to the Cabinet. The basis of the new arrangements will be the customer-contractor principle, Rothschild's device for guaranteeing the relevance of research done with public funds. The "customers" will be the departments of state, principally the Department of Health and Social Security (DHSS) and the Ministry of Agriculture, Fisheries, and Food (MAFF). The "contractors" will in all probability be drawn from the same groups who are doing the work today—the universities and the research council laboratories. The only thing that will change is the route by which the money reaches them—but that, of course, can make an important difference.

The storm of protest that greeted the Rothschild report (*Science*, 5 November 1971, p. 572) has won the research councils several important concessions. For a start, the total amount of money to be transferred from their budgets has been trimmed from £25.5 million to £20 million, figures that should be compared with the £56 million total annual budget of the three councils affected (all figures at 1971 prices). Furthermore, the changes are to be phased over 3 years. In the first year, 1973–1974, only £10 million will be taken from the research councils; the accumulated total will be only £20 million by 1975–1976. Additionally, and most significantly, the research councils have managed to have written into the white paper a clause which declares that "the expectation is that it [the money transferred] will be spent to commission applied research work from the Research Councils." Rothschild's original report made no such recommendation, which meant that the departments could have spent the money anywhere—even in the United States, for example. The crucial word here is "expectation." "Not quite as strong a word as we would have liked" a research council source admitted to *Science*, "but still we think it's a small victory."

Another small victory is a clause allowing the research councils to turn down commissioned work if they have "good grounds"—if, for instance, they do not think the project scientifically feasible. Thus the pure flame of research council independence has been preserved.

Lord Jellicoe, who, despite his antique title (he is the Lord Privy Seal), is responsible for British science policy, introduced the white paper at a House of Lords press conference. (Such are the security precautions at the Palace of Westminster these days that several correspondents had to cool their heels in an antechamber until policemen could be persuaded to issue passes. Fortunately, Lord Jellicoe was late, too.) The Lord Privy Seal declared the white paper "a landmark in open government." "We welcomed the discussion," he said, "although we didn't anticipate there would be quite so much. We took very careful account of the representations."

That much, at least, is abundantly clear from a careful reading of the paper. Short of defeating the Rothschild proposals altogether, or cutting them down so ruthlessly that little remained, the research councils could not have hoped for a much better outcome. Discussions now going on will determine exactly which projects will be transferred from research council to departmental control to make up the total cash transfer of £10 million for 1973–1974. Lacking such a list of programs, the cash transfer is simply a figure plucked from the air, without any substantive justification, but Lord Jellicoe was not to be drawn into admitting which projects would change hands. "Both sides have a damn good idea what is to be transferred," he said, "but I'm not going to dot the i's and cross the t's today." Sir Alan Cottrell, the government's Chief Scientific Adviser, estimates that all should be known "by the end of the year."

The assumption, quite clearly, is that there are already programs within the research councils that can be taken over lock, stock, and barrel by the departments—at which time they will be transformed into a contract between the customer (the department) and the contractor (the research council). Such a change is well short of revolution. Nevertheless, voices were not long in being raised against it. The *Guardian*, which has campaigned against the proposals, described the concessions as "two faltering steps away from a wrong policy." The London *Times* expected "something a little more imaginative" —and cleared its correspondence page for action.