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Perspective

The PNAS way back then

Saunders Mac Lane, Chairman Proceedings *Editorial Board, 1960–1968*

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This essay is to describe my experience with the *Proceedings of the National Academy of Sciences* up until 1970. Mathematicians and other scientists who were National Academy of Science (NAS) members encouraged younger colleagues by communicating their research announcements to the *Proceedings*. For example, I can perhaps cite my own experiences. S. Eilenberg and I benefited as follows (communicated, I think, by M. H. Stone): "Natural isomorphisms in group theory" (1942) *Proc. Natl. Acad. Sci. USA* **28,** 537–543; and "Relations between homology and homotopy groups" (1943) *Proc. Natl. Acad. Sci. USA* **29,** 155–158.

The second of these papers was the first introduction of a geometrical object topologists now call an "Eilenberg–Mac Lane Space." This idea was immediately accepted by leading topologists, with later detailed presentation by the authors in specialized journals. The *Proceedings* presentation of this idea helped Eilenberg and me by its promptness.

The first of these papers is a more striking case; it introduced the very abstract idea of a "category"—a subject then called "general abstract nonsense"! When Eilenberg and I submitted a full presentation in 1945 (to the *Transactions of the American Mathematical Society*), we feared that the editor would turn it down as too "far out," not really mathematics. So Eilenberg, who knew the editor well, persuaded him to choose as referee a young mathematician—one whom we could influence because he was then a junior member of the Applied Mathematics Group at Columbia University (war research), where Eilenberg and I were then also members, and I was Director.

Happily the full paper was accepted, but the subject itself was well off beat and not generally recognized, and it was ignored till 1958, when a student of Eilenberg's made an important breakthrough. So in this case publication in the *Proceedings* was perhaps vital at the start; Category Theory is now accepted. In other words, without the *Proceedings*, this idea might well have been buried, unpublished.

These circumstances may have played in part in my own election to the NAS in 1949, at the age of 40.

Later rumors had it that a young biochemist, whose paper had been rejected by the *Journal of Biological Chemistry*, managed to persuade an Academy member to communicate his result to the *Proceedings*. This paper presently led to a Nobel prize; the rumor continues to assert that Wendell Stanley, then *Proceedings* chairman, told this to many colleagues, who responded; with fast publication in mind, they came up with more communication to the *Proceedings.*

About 1958 a senior member of an NAS nominating committee noticed that it was a long time since the Section of Mathematics had been represented on the Council. I was nominated. In those early days there was no competition by multiple candidates, so I was elected.

In 1959, apparently just before a meeting of the Council, President Detlev Bronk got the news that Prof. Stanley was ill and wished to resign the position of Chairman of the *Proceedings*. Bronk knew that the *Proceedings* then carried lots of math. The Council met. Detlev was not a man to put off till to tomorrow what might be done today. So he looked about the table in that splendid Board Room, spotted the only mathematician there, and proposed to the Council that I be made Chairman of the Editorial Board. Then and now the Council did not often disagree with the President. Probably nobody then knew that I had been on the editorial board of the *Transactions*, then the flagship journal of the American Mathematical Society.

Soon after this, the Treasurer of the Academy, concerned about costs, requested the introduction of page charges for papers published in the *Proceedings*. At that time mathematicians did not have large grants, so most of them stopped submitting to the *Proceedings.*

At that time I probably thought that the *Proceedings* was a rousing success (biochemistry plus timely research announcements). So the indication was for full speed ahead and damn the torpedoes. I had also studied at Yale and firmly believed that Josiah Willard Gibbs was the greatest American scientist of the 19th century. I knew that the famous Gibbs–Wilson book on vector analysis had succeeded in setting vector notation in stone in every book in physics. I had first learned the real facts about calculus from a book by Edwin Bidwell Wilson, *Advanced Calculus*, a text not encumbered by the modern-day nonsense of calculus "reform." And I had heard the same E. B. Wilson hold forth in meetings of the Academy, educating new members as to what the Constitution and Bylaws actually said (E.B. really knew).

Just imagine my pleasure finding that this E. B. Wilson was still the Managing Editor. His (later) *History of the Proceedings of the National Academy of Sciences* is careful to state that (as just a manager) he never set policies for the *Proceedings*. But I can assure you that he was happy to help educate the young; in four years he wrote me about 50 letters in this cause. As a result it would not have occurred to me to consult the NAS Constitution or the NAS Council as to *Proceedings* policy—I had the real source right there on tap!

I followed standard procedure: papers by members or communicated by members were of course accepted. There was no refereeing. I did know that the then editorial assistant (Ms. Josephine Williams) usually acknowledged papers by members with enthusiastic praise for the brilliant results presented. I suspected that this praise was sometimes overdone and might not have been based on deep knowledge, but I did not interfere; after all, Ms. Williams managed matters well, so saved me lots of trouble.

On some occasions there were rejections, but such occasions were really rare, both scientifically and diplomatically—I would not have rejected a paper unless I was quite sure that I would get away with it! I recall only two cases.

A member of the NAS whom I thought to be quite old submitted a manuscript which tended to support this diagnosis. I rejected that paper. I do not believe I used a referee.

Linus Pauling communicated a paper by a medical friend who was a doctor in a Glasgow (U.K.) hospital for cancer patients. The paper presented evidence for the good effects of vitamin C. It was my clear impression that by this time most experts were very dubious about Pauling's views on vitamins and cancer. I rejected the paper; the expected blast from Pauling did not appear.

In 1967, the President of the Academy, Fred Seitz, invited a prominent politician, Emilio Daddario, to give an address at the autumn 1967 meeting of the Academy. He came complete with the manuscript of his speech. President Seitz communicated this manuscript to the *Proceedings*. (At that time the journal *Issues in Science and Technology* did not exist.) I considered this paper totally inappropriate for the *Proceedings*, but I was aware of the possible extent of my mandate. Moreover, I had just recently arranged to have John Edsall appointed as Associate Chairman of the Editorial Board, and I may have felt that an eight-year term of office was enough. In any event, I resigned the *Proceedings* chairmanship.

I continued to admire the *Proceedings* from a distance, as do my fellow mathematicians. We have a tradition in these matters. In France, E. Cauchy from about 1840 published so many of his important results in the *Comptes Rendues* that the authorities at the Academie des Sciences introduced page limits. But also Fourier and Cauchy were responsible for the failure to publish there the revolutionary work of Evariste Galois (who used group theory to settle famous questions about the solution of polynomial equations of higher degree). Galois died in a duel, unpublished and misunderstood (till 1870).

Today the *Comptes Rendues* still publishes decisive papers by young mathematicians, communicated by seniors. For example, in 1948 Academician Jean Leray, recently returned from a German prisoner-of-war camp, gave totally obscure lectures at the College de France on a wholly mysterious idea of a "spectral" sequence. Two young doctoral students, Armand Borel and Jean-Pierre Serre, attended. After a bit Serre found the lectures quite obscure, so dropped out. Borel continued and presently sought Serre out. "Jean-Pierre, you can actually prove good theorems with those 'spectral sequences'." Together they worked out one such theorem; a senior Academician communicated this to the *Comptes Rendues*. Both Borel and Serre used the spectral technique in their subsequent doctoral theses; both of which made them famous:

Borel, A. & Serre, J.-P.: (1951) "Impossibilitè de fibre un espace euclidien par des fibres compact" *C. R. de l'Acad. des Sciences* **33,** 680–682.

The point is simple: prompt publication by newcomers in an Academy journal, as sponsored by seniors academicians, is vital to scientific progress. This process should be unduly inhibited by those seniors who don't get it. (This can happen to a referee.)

Incidentally, on a visit to Paris in 1947, I attended one of Leray's lectures, at a time when a doctoral student of mine had already invented half of the "spectral" idea. I didn't get it at the time, but only later when I read Serres' 1952 thesis, which made spectacular use of this idea.

Conclusion: In France, papers communicated by seniors really did matter. Now referees are used for all such papers.

Sometimes a member of the Academy doesn't get something quite right, but such cases may better be settled by time and not by referees. I give an example. George David Birkhoff was recognized as the leading American mathematician from 1912 on (when he proved the "Last Geometric Theorem" which had stumped Henri Poincaré, then the leading French mathematician).

Later G. D. Birkhoff, in rivaling Einstein, published an announcement on "Flat space time and gravitation" (1944) *Proc. Natl. Acad. Sci. USA* **30,** 324–334. And in it he admitted that Hermann Weyl disagreed with Birkhoff. (Weyl was the number 1 German mathematician; today Weyl's views on the matter are generally accepted.) But the publication by Birkhoff put the problem out in the open. And for that matter, Birkhoff had already announced in the *Proceedings* one of his most famous results: the Ergodic Theorem. For Birkhoff this amounts to one strike-out and one home run with the bases loaded. Better both than neither.

The *Proceedings* is there to help bring new ideas promptly into play. New ideas may not always be right, but their prominent presence can lead to correction. We must be careful not to censor even those ideas which seem to be off beat.

P.S. Here are more additional samples of important papers communicated to the *Proceedings.*

The Continuum Hypothesis. The infinite set R of all real numbers is larger than the infinite set N of all whole numbers (reason: no numbered list r_1, r_2, $r_3, \ldots$ can exhaust the real numbers). George Cantor, the founder of set theory, conjectured in 1885 that there is no set both bigger than N and smaller than R (the Continuum Hypothesis). This remained a famous and unsolved problem for many years. Finally, in 1940 the noted logician Kurt Godel constructed a model of set theory in which the Continuum Hypothesis is true: *The Consistency of the Continuum Hypothesis*, Annals of Mathematical Studies (Princeton Univ. Press, Princeton, NJ).

It was widely rumored that Godel was also able to construct another model in which the Continuum Hypothesis was false. When the young analyst Paul Cohen found such a model, he submitted it to Godel, who communicated the result to the *Proceedings* in two papers: "The independence of the Continuum Hypothesis" (1962) *Proc. Natl. Acad. Sci. USA* **50,** 1143–1148 and . . . "II" (1964) *Proc. Natl. Acad. Sci. USA* **57,** 105–110. In this case the correctness of this surprising result was established by its communication to the *Proceedings* by the outstanding expert on this topic.

In 1930 John von Neumann, who had come to the United States from Hungary and Germany, discovered his "mean ergodic theorem" and submitted this to G. D. Birkhoff for possible communication to the *Proceedings*. This submission led Birkhoff to prove his famous ergodic theorem. (as above).

The *Notices* of the American Mathematical Society carried in April 1997 a discussion (Vol. 44, pp. 430–431) of the work of the famous French mathematician E. Cartan on the classification of complex simple Lie algebras and also of their Cartan subalgebras. The discussion ends with the following Editor's note:

> The classification in question was first accomplished by Bertram Kostant in two papers submitted (by Saunders Mac Lane) to the *Proceedings of the National Academy of Sciences*. The first was published [*On the conjugacy of real Cartan subalgebras I*, Proc. Nat. Acad. Sci. U.S.A., **41** (1955), 967–970], but the editors objected to the elaborate tables in the second, which nevertheless were widely circulated among those with an interest in the area. About four years later a list was published by M. Sugiura [Conjugacy classes of Cartan subalgebras in real semi-simple Lie algebras, J. Math. Soc. Japan **19** (1959), 374–434], who, upon subsequently seeing Kostant's second paper confirmed to him that the lists were identical.

I had forgotten this. At the time Kostant was a graduate student at the Department of Mathematics at the University of Chicago, where faculty members such as A. Weil knew much about Lie algebras—a subject which I had studied in Göttingen in 1931. I was then Chairman of the Department, so I was clearly in a position to judge the value of this result.

In my view this and many other cases, also in other branches of science, emphasize the way in which Academy members can recognize progress by communicating new results of others. For this reason I suggest that the communicating member in such cases should have an opportunity to challenge any negative reviews of articles which the member has communicated.