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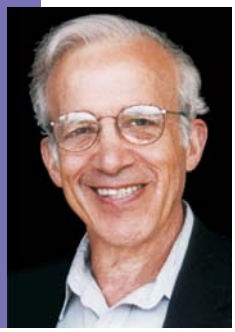

What Sam said

Daniel Kleppner

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Samuel Goudsmit is best known in the world of physics for discovering, with George Uhlenbeck, that the electron possesses spin. He deserves equal credit for founding *Physical Review Letters* (*PRL*). Sam, as he was known to all, was born in Holland in 1902 and became a student of Paul Ehrenfest at the University of Leiden. In 1928 he and Uhlenbeck made their great discovery. Sam joined the physics department at the University of Michigan, where he pursued atomic and nuclear theory and then spent the war years working on radar. When peace returned, he joined Brookhaven National Laboratory, and in 1952 he became the editor of *Physical Review*. He created *PRL* in 1958 and served as its editor until 1974, when he retired.[1] Sam died in 1978. An obituary by Maurice Goldhaber appeared in PHYSICS TODAY, April 1979, page 71.

While searching for ideas for a talk that I had impulsively agreed to give in honor of *PRL*'s golden anniversary, I read the many editorials Sam wrote as he steered the new journal through its early years. His essays turned out to be a delight because he held strong opinions, loved language, wrote precisely, and had a wonderful sense of humor. Sam's vision for *PRL* combined an elevated view of physics with a somewhat tempered view of physicists. Reading Sam's editorials today, one hardly knows whether to laugh aloud or weep that his world of physics, in which one could reasonably aspire to be broadly knowledgeable and follow important advances in every field, has vanished, blown away by the explosive growth of physics and the physics community, the increasing complexity of research, and the revolutionary impact of digital communications.

Sam's concept for *PRL* grew out of the Letters to the Editor section of *Physical Review*. By today's standards, those letters were blissfully brief. For instance, the invention of the maser (not yet so named) was described in slightly less than a page and a half.[2] Although Sam judged that the right length for a letter was about a page and a quarter, he reluctantly gave way to pressure from the community for longer letters. (All the following quoted excerpts are from *PRL* and are accessible in their entirety from the online version of this column.) Sam could not, however, conceal his distaste for length: "[A] point on which we wish to comment is the persistent increase in the average length of published Letters. If we were convinced that the lengthening was accompanied by an increase in clarity, we would at least be tolerant of it and perhaps even welcome it. In fact, we do not believe that this is the case."

Fighting for clarity

Sam was passionate about clarity and unforgiving about sloppy writing. Recalling his earlier experience editing the *Physical Review*, he wrote: "*The Physical Review* differs considerably from *The Saturday Evening Post* in its reader to author ratio. We suspect that for *The Physical Review* this ratio sometimes is even less than the critical value of one. We derive this from the observation that occasionally a paper is received in such a deplorable state of preparation that it seems unlikely that the author himself has read and checked it." Some years later, having evidently failed to stamp out bad writing, he resumed his fight for clarity: "The horrible style and obscurity of many papers, together with the author's conviction that he is a better exponent than Faraday was in his famous Christmas Lectures, leads to insurmountable misunderstandings. The lack of elementary teaching experience of most research workers may be the basis of this problem."

In matters of style, Sam was adamant in opposing neologisms. He summarized his case by an Olympian pronouncement: "We find that [neologisms] are often ungrammatical, frequently ugly, sometimes chauvinistic, likely to be obscure, and usually unnecessary." Contractions and abbreviations also generated ire. "Excessive use of contractions and abbreviations is often a sign of laziness of authors in writing their papers. . . . Must we look forward to the day when cyclotrons will be called cyons and Hamiltonians become Hams?"

Although Sam himself was a respected theorist, he could not resist tweaking other theorists. He greeted *PRL*'s first New Year's issue with: "May the New Year bring us no more new particles and fewer but more successful theories." Some years later, having been stung by publishing a seriously flawed theoretical proposal, he wrote: "Colleagues complain that authors of some theoretical Letters fail to recognize even the minimum of implications of their brilliant proposals. Had these authors given their subjects just a little more time and attention before bursting into print, they might have found encouraging support, or, more often, violent disagreement with experimental results."

From time to time Sam spoke out as the conscience of physics: "We occasionally receive complaints about Letters on theoretical physics. . . . Often the complaint is based on the belief that the authors of many Letters are merely staking claims in the hope that their likely but not really well-founded propositions may be proven correct by later experiments. Experimental physicists resent that these theorists then demand full credit for their premature guesses—and that they hope that everyone will forget the ones that were not confirmed.

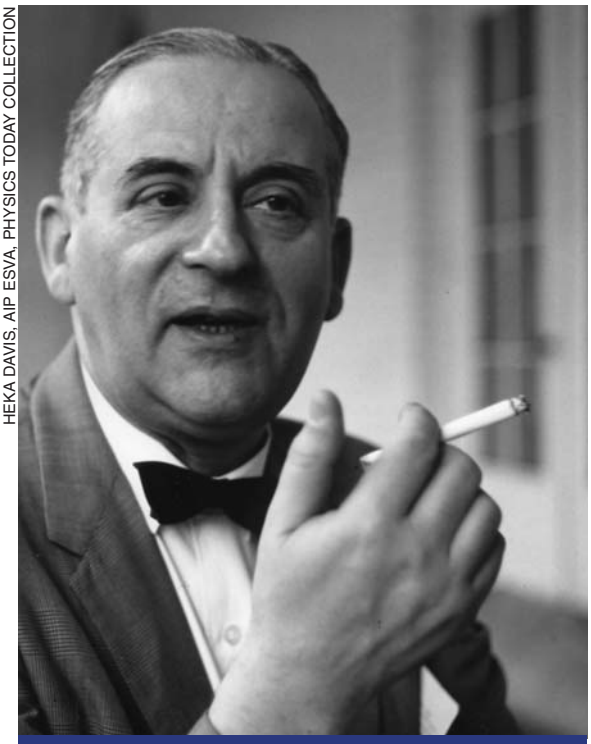
"Not that the experimenters themselves are always blameless. They sometimes publish Letters containing conclusions which are plausible enough but which are derived from insufficient data inadequately checked for systematic errors or other spurious effects."

Sam occasionally chided physicists about their manners, for instance neglecting to share results with colleagues by publishing the results in a refereed journal before running to the press. He offered a novel argument: "While we welcome the intensified interest of the layman in physics research we recog-

nize that formerly crackpots often made the front page with their spectacular stories, and this still happens occasionally. We are sure that our authors do not wish to be confused with these pseudoscientists in the minds of the public."

Refereeing and paranoia

Considering that peer review lies at the heart of scientific integrity, its introduction to *PRL* was remarkably casual. The inaugural issue described it this way: "Since there is little time, or none at all, for refereeing, most of the decisions for acceptance and for minor alterations will have to be made in the Editor's office." The "Editor's office" included Sam's colleagues at Brookhaven National Laboratory, who served as informal advisers. In time manuscripts were sent elsewhere for review. That was the era of typewriters and carbon paper, and the mechanics of production were unbelievably clumsy. The loss of a manuscript by an inattentive reviewer, for instance, could cause a serious hardship. However, then, as now, the most contentious issues were judgments of scientific quality. After eight years of experience, Sam wrote: "For many years rumors have spread about the editors' methods of selecting referees. Some authors believe that we pick the name appearing in the first footnote; others are sure that we keep a secret file of their worst competitors and select a referee from them. The rumors also claim that editors always make the wrong choice, though in fact authors often thank the referee for his constructive remarks."



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Samuel Goudsmit

Sam returned to the anguish of refereeing in 1970. "There are still authors who believe that referees and editors are biased against them. We doubt that they can be convinced that this is not so. A necessary condition for being a successful research worker is a touch of paranoia. . . . A good researcher must fear that others are trying to pirate his ideas, to delay his experiments, and to scoop him." He went on wistfully to present a utopian vision: "I have known a few physicists who lack this essential trait. When someone publishes their results, they take it as proof that their work was worthwhile and they are happy to start on something else. They are the kind of people who read other people's articles carefully and rejoice in the success of others. They do not get the recognition they deserve except from their own pupils, and any recognition comes later in their careers."

Jolts from new technology

In the age of typewriters and carbon paper, there was no practical way to share scientific papers before publication. Consequently, the advent of the Xerox machine had an enormous impact on scientific communication. But Sam, a traditionalist, was appalled by the prospect of unrefereed preprints circulating through the scientific community: "Recently a few theorists have proposed . . . a central register of preprints and other unpublished reports and a rapid distribution of lists of preprint titles and authors. We believe that this is highly undesirable, as it would raise the unrefereed and unedited preprint to virtually the same status as a formal publication. An author could claim priority on the basis of the registry date of his paper." Although I feel certain that he would later have changed his opinion, he pursued this perceived enemy with a malicious wit: "The next step might be to equip theorists with portable recorders so that all their statements about physics, including those uttered in their sleep, would be preserved on tape. The contents of the tapes would be transmitted electronically to interested colleagues via a distribution center; computers coded with key words could scan the tapes for information relevant to each user's interests. Hopefully such a system might result in such chaos as to make priority assignments impossible, and the great advances in theoretical physics would become anonymous, just like the great achievements in the art of ancient Egypt."

Sam went on to make a prediction that was uncannily prescient of things that actually came to pass three decades later: "[It] is obvious that the centuries-old system of communication by journal publication is no longer adequate however much it expands, and that significant changes are needed. The nature of the changes is not yet known. Perhaps they will be as radical as was the invention of the alphabet in its time."

One wonders how Sam would have reacted to the physics arXiv when it first made preprints instantly available to the scientific community. Some saw the arXiv as a threat to scholarly publication because it bypassed refereeing, but my guess is that Sam would have reacted as the American Physical Society ultimately did, by embracing the new technology rather than fighting it. He felt strongly that the journals should respond to the community will, writing at one point, "The journals must reflect the standards of the profession rather than those imposed by editors. Consequently, the only regulatory factor is the opinion which the community of physicists expresses."

Less obvious is how Sam would have regarded the custom that has developed of physicists publishing their most spectacular discoveries in scientific magazines rather than in journals that are directly accountable to the physics community. I imagine that he would have resigned himself to the change in community spirit but would somehow have managed to convey his regrets in words pithier than I could muster.

I thank George Basbas for helpful comments.

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2. J. P. Gordon, H. J. Zeiger, C. H. Townes, *Phys. Rev.* **95**, 282 (1954). ■