(CBE) to do just that. However, his views are not heartily endorsed by the group, and a CBE policy statement on dual publication that has been in draft form for more than a year cannot break out of committee, in part because the members cannot reach agreement on the question of the press.

"I believe this is an overemphasized issue," says Edward Huth, chairman-elect of the CBE. "Some editors like to feel that their material is exclusive, but I think this is exaggerated with regard to the press. Our function, as I see it, is to enable scientists to report fully on their research from their own points of view—not a reporter's. Personally, I'm not too concerned about the medical press putting journals out of business."

It is interesting, however, that some of Huth's scientific colleagues do not share his optimism in this last regard. In the 11 May issue of the *New England Journal of Medicine* ironically enough, Eugene Braunwald of the University of California at San Diego, predicts that in the future scientists will rely on the press for information about what is going on and that journals will come to serve a solely archival function.

Like the biologists, the physicists have had their problems with the press and vice versa. Samuel Goudsmit, of the Brookhaven National Laboratory, is editor-in-chief of the journals published by the American Physical Society and the editor of *Physical Review Letters*, the APS publication most likely to be a bone of contention with reporters in the matter of prior publication because it publishes research notes quickly. Goudsmit believes that his policy which, he says, is clear to both physicists and reporters, is agreeable to both sides. "What I object to," he says, "is an investigator who reports extensively through the press *before* presenting his work either at a meeting or in a recognized journal. We do not like it if a man holds a press conference before he submits his work to us and will not publish it if he does. But we do not object to publicity that comes after presentation at a meeting. That constitutes formal presentation to one's peers which is all we ask."

Philip Abelson, president of the Carnegie Institution and editor of *Science*, opposes erecting a rigid editorial policy and says that *Science* is flexible in its attitude. At times, he points out, news accounts may even stimulate interest in a paper when it appears in print.

After long and heated debate on the Ingelfinger rule at Hershey, Ingelfinger admitted that he could be persuaded to modify his position if there were sufficient reasons. Now, he says, without retrenching very far, "The persons who should decide this issue are the people in the academic community. I never hear from them about it, though they write letters about everything else on their minds. If there were strong objections to my policy, I'd drop it."

—BARBARA J. CULLITON

Nobelists: Piccioni Lawsuit Raises Questions about the 1959 Prize

In what could prove to be the Earle Stanley Gardner detective story of science, a University of California physicist has filed suit against two Nobel laureates, Emilio Segre and Owen Chamberlain of the Lawrence Berkeley Laboratory, charging that they cut him out of participation in a crucial experiment that he designed, and hence out of the recognition and income that would have attended his sharing with them the 1959 Nobel prize. The experiment definitively proved the existence of the antiproton; subsequently, the existence of antimatter became generally accepted.

Oreste Piccioni, a 56-year-old nuclear physicist, filed suit last week in Alameda County Superior Court seeking $125,000 in damages and an admission by Segre and Chamberlain that the design of the 1955 antiproton experiment was really his. Despite the fact that rumors of theft and lack of proper credit are rife in many branches of science, particularly concerning the awarding of Nobel prizes, this appears to be the first time that a scientist who claims he was professionally mulcted has sought redress in court.

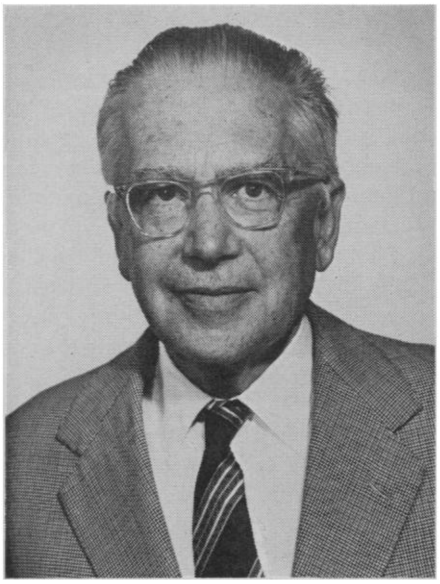
Piccioni left Italy in 1946, went to the Massachusetts Institute of Technology, stayed there 2 years, and moved to Brookhaven National Laboratory, where he remained until 1960. He is now a professor of physics at the University of California, San Diego. Piccioni is best known for his work in detecting the antineutron, which was done at the Lawrence Radiation Laboratory in the mid-1950's. Prior to 1948, he had been a cosmic ray physicist, and since then his work has been in experimental nuclear physics.

Segre and Chamberlain, for their part, are offering only "no comments" through their secretary. They will soon have to file an answer to the complaint, but Piccioni's lawyers, Meyers and Jacoby of Beverly Hills, say there is no way of knowing when the case may come to trial.

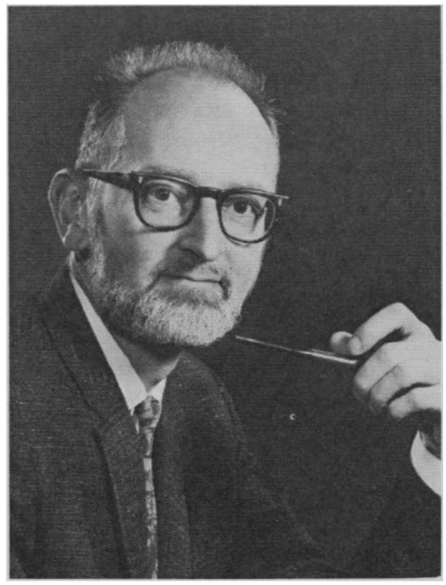
Segre would be a well-known scientist even without the discovery of the antiproton. He is a discoverer of technetium and a codiscoverer of astatine and plutonium-239. Chamberlain is best known for his association with the antiproton experiment; he also worked on the Manhattan Project. Both Chamberlain and Segre are members of the National Academy of Sciences; Piccioni is not.

Piccioni alleges that, during the winter of 1954, he revealed to Segre and Chamberlain his design of an experiment to prove the existence of the antiproton. According to his legal brief, this design was unique in that instead of looking for the postulated antiproton by observing its annihilation process—the conventional approach—Piccioni sought to detect the predicted particle by measuring its time of flight. He allegedly proposed to do this by using a double magnetic lens spectrometer, as well as a Cerenkov counter to give the experiment redundancy. According to Piccioni, such magnetic lenses were in use before 1954 at Brookhaven.

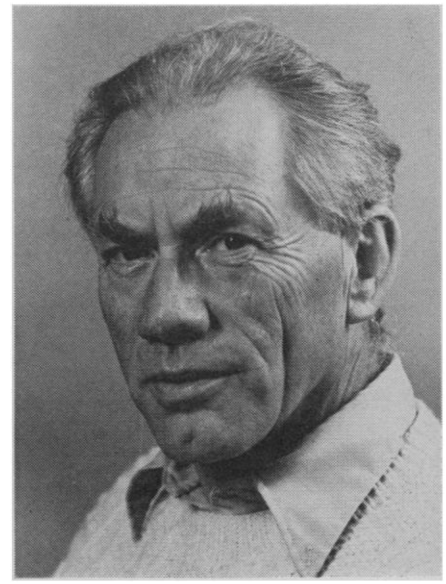
Piccioni's lawyers are expected to argue that an oral contract, either express or implied, existed between Piccioni and Segre and Chamberlain at the time he allegedly unveiled his design



Emilio Segre



Owen Chamberlain



Oreste Piccioni

to them, that, if the experiment was carried out, Piccioni would participate with them. Promises of one sort or another are common enough in science, but this appears to be the first time such purported dealings could become part of a court proceeding.

Then in October 1955, Segre and Chamberlain conducted an experiment in which they used similar apparatus; their colleagues at the time were C. E. Wiegand and T. J. Ypsilantis. Piccioni apparently had no role.

Two other features of the lawsuit have interesting legal implications for much that is now considered commonplace in academic scientific life. One is Piccioni's charge, outlined in the brief, that, in the years since the successful antiproton experiment, Segre and Chamberlain "cautioned" Piccioni against making public disclosures of his contributions to it and that they "threatened" that, should he do so, he would be denied access to the facilities of the Lawrence Radiation Laboratory. The brief also claims that Segre and Chamberlain made "promises" to Piccioni that "favors would be granted him if he would refrain" from making his role public. This appears to be why Piccioni waited 18 years to bring suit.

A second interesting feature is that, while it is generally accepted that to be credited for an experiment in publications and lectures enhances one's reputation, the suit will try to prove that the reverse is also true. Piccioni's lawyers are expected to argue that, by forfeiting public recognition of his role in the antiproton experiment, lectures, and publications, Piccioni "was prevented from profiting from his labor and research in the form of profits, commissions, royalties." This is one basis of his claim to financial redress and a public admission.

A number of physicists at the national accelerator laboratories and elsewhere who were contacted declined to comment directly on the lawsuit or on the events of 1954 to 1955 with which it is concerned. However, more general discussion about the implications of such a court action for physics and physicists sparked some interesting comments.

One reaction was the strong implication that, by dragging such matters into court, Piccioni was breaking the rules. Various sorts of cheating among scientists have been alleged since Newton's day, and the suspicion of cheating has been a frequent subject of study by historians and sociologists of science. A survey in 1967 and 1968 of over 200 British high energy physicists, for example, found that over one-sixth of them believed earnestly that some of their work had been stolen at some point.

However, the other side of the coin, in terms of professional ethos, is that the rumors of ill conduct are generally allowed to remain just that. The requirement of gentlemanly politesse prevents these allegations from becoming very public, no matter how wounding the alleged backroom backstabs. As one scientist put it, "It's all right to talk about these things over beer—you can get some sympathy from others then"; but he implied that sympathy over beer was preferable to redress in a public forum.

A second point raised is the problem of awarding proper recognition in the field of high energy physics, which has been characterized by ever-bigger accelerators. Now, the very scale of any experiment involves many people and vast advance planning; there can be as many as 20 names to a single scientific paper. This obviously raises questions about who should be credited for which part of the work. As one physicist commented, in part facetiously, perhaps the machines should be getting the prizes, not the men.

A third point mentioned was that there are no hard and fast rules in science as to what constitutes cheating. There is only a host of tribal custom, equal only to the British Constitution in its inability to be committed to paper. The scientist is faced with hundreds of situations involving fuzzy ethics, whether he is overhearing a cafeteria conversation or refereeing a paper which has not been published.

Another question which arose is what the Piccioni lawsuit implies for the Nobel prize itself. A number of those contacted expressed fears that "messy" arguments over the experiment which led to the 1959 award and the "mudslinging" that might occur could have the effect of tarnishing the glory of the prize.

But one prominent physicist was moved, instead, to offer a McLuhanesque analysis of the present reward system. The extent of communications, travel, and interchange among scientists today, he said, has made the lone individual, working in his laboratory on the conception and execution of an experiment, obsolete. Just who is responsible is increasingly blurred, yet "we are still distributing credit as though we were living in the last century."

—Deborah Shapley