places had to be disqualified after the event, when analysis of urine samples revealed that all had taken Dexedrine. A drug problem of a different nature, affecting professional footballers, is the use of pain killers such as novocaine to enable a player to continue playing even when injured. Vince Lombardi, for example, took the line that no player was ever injured—"A man would have to have a bone sticking out of his skin for Lombardi to let him off," says one football trainer.

Though amphetamines and steroids are taken primarily in the belief that they will improve performance, both drugs impart a psychological kick and to this extent are no different from heroin, marihuana, and the drugs used by society at large. That athletes, the supposed exemplars of clean living and respect for their own physiology, should be so deep into drugs is presumably a consequence, at least in part, of the pressures to which they are subjected. In professional football, the advent of big gates and superstars has led some managers to use any means available to keep a player on the field. In athletics, the unceasing upward march of world records has compelled trainers to demand more and harder training schedules of their athletes. Swimmers may be required to swim 5000 yards a day, long- and middle-distance runners to run 150 miles a week. "You can't ask this of these guys and expect them to submit to the average man's diet," says O'Shea. "At every meet you go to you see world records broken in one class or another. How far can you go before something gives way?" asks St. John. If athletics is already approaching the limits of normal physiology, it is maybe inevitable that athletes will turn to artificial means to coax the last twitch of energy out of a fatigued muscle or to improve upon the masculinity of potential Mr. Americas. But the gentlemen who set the rules seem happier denouncing steroids than trying to understand the trials and temptations that push today's athletes into drugs.—NICHOLAS WADE

Dual Publication: "Ingelfinger Rule" Debated by Scientists and Press

How much should a scientist tell a journalist about his work? When? If he tells too much, too soon, does a reporter's account of his research constitute publication that legitimately precludes the investigator from subsequent publication in the "professional literature?"

These questions perennially plague, and often strain, relations between scientists and the press. Whenever they are raised, as they were recently at a meeting on scientists and the media, which the Federation of American Societies for Experimental Biology (FASEB) sponsored in Hershey, Pennsylvania, representatives of both sides call for their immediate resolution, but no pat answers are on the horizon. Thus the need for resolution persists and the debate continues. It is most intense when it focuses on the "Ingelfinger rule," named after its most ardent proponent, Franz J. Ingelfinger, editor of the *New England Journal of Medicine*. It appears that the Ingelfinger rule, which many scientists and reporters consider too rigid, might bend a little.

The Ingelfinger rule concerns itself with prior publication of research in the medical news media. "In general," Ingelfinger wrote in *Science* (28 August 1970, page 835), "the *Journal*'s attitude would be influenced in a negative way if the principal ideas of an article, as well as its crucial data and most important figures, had already appeared in a medical news medium—just as the effect would be negative if the identical items had been published by a paradigm of staid medical literature." According to the rule, an investigator should refrain from cooperating with the press to any great extent—giving texts from which to quote and slides or tables for reproduction—even with regard to work that has been formally presented at a recognized scientific meeting. Herein is the rub. Many reporters and scientists believe that material presented in such a forum is fair game, that, as far as the press is concerned, presentation at a meeting constitutes publication of a sort. Therefore, following this line of thought, a scientist who cooperates with the press under such circumstances is not guilty of circumventing peer review.

Ingelfinger agrees with this point of view only in part. "Journalists," he says, "have a duty to report developments in science and medicine." He draws the line at what he considers full disclosure in a specialized newspaper or magazine.

Ingelfinger feels so strongly about this that he occasionally discards an already accepted paper because of a news report that beat the journal into print. He estimates that he drops six or seven papers a year for this reason. Thus far in 1972, he has discarded one. (He is reluctant to disclose the particulars of that case.)

Ingelfinger stresses that he feels competitive only with the medical press. Contrary to what many persons assume, he has no quarrel with what is presented in daily lay newspapers. Nevertheless, this distinction blurs in the minds of both reporters and scientists who tend to react to the Ingelfinger rule as if it applied to any form of news coverage whatever. Many investigators behave as though the ground rules for publication in the *New England Journal of Medicine* applied to other journals as well. Ingelfinger wishes it did.

Thelma Heatwole, who runs the press operation at a number of meetings, including FASEB in the spring and the American Association for the Advancement of Science in the winter, observes that when she asks speakers for texts of their papers she is frequently confronted with their fears about violating the Ingelfinger rule even though they may have no intention of submitting a paper to the *New England Journal of Medicine*.

There are two motives behind Ingelfinger's attitude toward the press. First, the gastroenterologist turned editor has printer's ink in his blood and, quite simply, does not want to be scooped. "Why," he asks, "should people want to read my journal if they know that most of what is in it is likely to have appeared already in *Medical World News* or *Medical Tribune?*" Disputing the common counter-argument that the

New England Journal of Medicine is archival, a journal of record, Ingelfinger says its function is educational—to carry the latest in medical knowledge to its 130,000 subscribers—and likens it to *Harper's* and the *Atlantic Monthly* in that it is a general, rather than a specialty, magazine in its field. Its archival role, he maintains, is secondary.

Ingelfinger's second point in explanation of his position on dual and prior publication has its roots in his attitude about what the public needs to know, and when, about peer review. "I ask you," he demanded of the participants at the Hershey meeting, "why a university man must tell you what he is doing in research until he is done. What is the rush?"

Ingelfinger believes that there is precious little going on that must be reported in depth by the news media in advance of scientific publication. He responds to reporters' contentions that they need full texts and data in order to report accurately by saying, "Although they pride themselves on reporting accurately, there is no assurance that what they report is accurate in the first place." In this regard, Ingelfinger is frequently accused by reporters of wanting to censor science news. He says he merely thinks it worthwhile to have a man's work reviewed by his peers before it is broadcast to the public. It is one of the most difficult issues about which to reach any consensus.

In approaching the question one must confront the motives underlying the attitudes of those concerned. There is the competitive side of the problem. Reporters do not want to wait for months until the *New England Journal of Medicine* comes out after they've heard something at a meeting, any more than Ingelfinger wants to be second into print. And there is the growing willingness—even desire—of many scientists to talk to the press on the belief that publicity will help them when their grants come up for approval. Researchers who even 2 or 3 years ago demurred when approached by newsmen have changed their minds and no longer look upon a conversation with a reporter as a breach of scientific decorum. Indeed, the notion that the public has a right to know about what is going on in the laboratory even before a project reaches completion is gaining momentum. This attitude was carefully spelled out after an FASEB conference 2 years ago, but was buried in a lengthy report in the *Federation Proceedings*, May-June 1971.

Although Ingelfinger now concedes that he may be willing to reconsider his policy, he has been fighting for the last couple of years to convince the editors of other journals to adopt his policy and, indeed, is trying to get the 300-member Council of Biology Editors

Rainmaking: Stockholm Stand Watered Down for Military

During the sometimes stormy international environmental meeting at Stockholm, there were disagreements not only among different countries, but within national delegations too. It has been learned that, during a meeting of the U.S. delegation there, a Department of Defense (DOD) official admitted that possible military use of weather modification was a key consideration in an official U.S. attempt to dilute a recommendation on climatic changes.

At Stockholm, Recommendation 218 of the work of Committee III was, like all hundred-or-so planks, discussed among the U.S. delegates before being taken up as official business. This plank required all governments to "carefully evaluate the likelihood and magnitude of climatic effects and disseminate their findings . . ." and to "consult fully other interested states when activities carrying a risk of such effects are being contemplated or implemented."

The language of the recommendation sounds innocuous enough; however, the United States planned—successfully—to water it down even more. The U.S. position paper argued for the insertion of two phrases ("to the maximum extent feasible" and "wherever practicable") in the recommendation.

According to the U.S. official paper, the reason for the insertion was realism: ". . . The mechanisms by which man's activities might affect the climate are to a great extent imperfectly known," it said.

But according to sources present at an informal meeting of the U.S. delegation on 5 June, William Ruckelshaus, administrator of the Environmental Protection Agency (EPA), questioned the need for the United States to dilute this language. Ruckelshaus, it is said, persistently questioned the position papers' reasoning on this point. His comments apparently led into a discussion of weather modification, in which Robert M. White, administrator of the National Oceanographic and Atmospheric Administration, and Lieutenant Colonel John Nolan, a DOD representative participated.

White allegedly asked Nolan if military uses of weather modification might pose a situation in which the United States would not notify other countries of effects on their climate. Nolan replied to the effect that that was exactly the case.

Subsequently, the conference as a whole voted to add one of the two U.S.-suggested phrases to the language of Recommendation 218.

The within-group discussions of the U.S. delegation to Stockholm are confidential. Most of those contacted would neither confirm nor deny the details of this incident. However, some were willing to confirm certain aspects and to say that a thorough discussion of weather modification had taken place. Nolan, when asked about these accounts, replied that they seemed to him "a dry hole." The DOD position, he said, was that "Stockholm was not a place to get involved in defense issues."

About a dozen observers were present during the alleged conversation, as well as other notables such as Senator Howard H. Baker, Jr., of Tennessee; two State Department officials, Lowell Dowd and Donald King; Fitzhugh Green, assistant administrator of EPA; and David Keaney of the Senate Foreign Relations Committee staff. The Nolan admission adds more weight to rumors and shreds of evidence that the DOD has pursued weather modification activities in the course of the Indochina war (*Science*, 16 June). At the very least, it seem clear that DOD considers these tactics potentially valuable militarily.—D.S.

(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