space. Such a ban would raise a number of legal problems, according to arms control experts.

John Rhinelander, the legal adviser to the U.S. delegation that negotiated the ABM treaty and a leading critic of Reagan Administration policies in this area, argues that the Soviet proposal would set "an unverifiable standard." It would exacerbate the problem of what would be permissible, because testing and development of some technologies, such as sensors for early warning satellites, could also be used in ballistic missile defense systems. Similarly, Ashton Carter, a physicist at Harvard University who wrote a critical study of SDI for the Office of Technology Assessment, says "It is totally unclear what limiting to the laboratory really means. I don't think there is any serious way to add restrictions beyond the ABM treaty, because the treaty already bans everything that can be verifiably banned."

Many observers are also left wondering whether Gorbachev's proposal would limit all SDI research—including work on land-based systems, which is currently permitted. In a televised address 2 days after the summit, Gorbachev repeatedly referred to restricting SDI to the laboratory, but the actual proposal he says he gave Reagan in Reykjavik is not all-encompassing. It states: "Testing of all space elements of anti-ballistic defense in space [is] prohibited except research and testing in laboratories."

In fact, a detailed discussion of what Gorbachev had in mind when he proposed limiting SDI research to the laboratory did not take place at Reykjavik, John Poindexter, Reagan's national security adviser, acknowledged at a press briefing. There was also no discussion of the Administration's interpretation of the treaty. According to assistant secretary of defense Richard Perle, "there has been enough discussion of this so that I think it's clear that the Soviets understand that they have to go beyond the ABM treaty if they're going to drive a stake through the heart of the SDI program."

However, there were indications a week after the summit that the Soviets may seek to clarify their proposal. White House spokesman Larry Speakes said on 20 October, "We have had at least some representation from the Soviet Union that they would like to discuss their interpretation of, and our understanding of their paper that they presented at Reykjavik, which talked about laboratory testing. We would be anxious to discuss it with them at Geneva and attempt to clarify [it]."

Whether a clarification of the proposal will provide any basis for a compromise remains to be seen. ■ COLIN NORMAN

San Diego's Tough Stand on Research Fraud

When a faculty member was caught faking data, UCSD asked all of his coauthors to defend their work. The result: 68 medical papers may be invalid

"ALL of us thought very highly of this young man," said radiologist Elliott Lasser, speaking of a junior scientist named Robert Slutsky who wreaked havoc at the medical school of the University of California at San Diego (UCSD) by publishing false data.

Lasser, a senior member of the radiology department, accidentally came upon the Slutsky bombshell in 1985. Following rules established in the wake of fraud cases elsewhere, UCSD created an ad hoc investigative committee in the dean's office, beyond the jurisdiction of the affected department. The group quickly determined that Slutsky had fabricated data in three papers, listed coauthors without their permission, and falsified some of his qualifications on a curriculum vitae.

A second committee, chaired by thoracic surgeon Richard Peters, was appointed to sort through all of Slutsky's publications and to winnow the bad from the good data. On 9 October, after more than a year of painful work, the committee released the bitter residue: 10 additional papers originating in UCSD labs were deemed "fraudulent" because they had relied heavily on suspect work done by Slutsky, and 55 others were of "questionable validity."

In an unusually strong report, the committee noted that it was "unable to persuade Dr. Slutsky through his attorney to acknowledge the fraud." But when the committee asked journals to retract two papers, "Dr. Slutsky, through his attorney, subsequently withdrew 15 published papers, apparently in response to the UCSD retraction letter, but without acknowledging fraud." The group leaves no question about its own view, however, that Slutsky was engaged in "extensive research fraud."

The Peters committee noted that toward the end of his time at UCSD, Slutsky was producing papers at the rate of about one every 10 days. More than anything else, Peters said in a telephone interview, he was shocked by this rate of production, which he said was faster "than most of us could write even if we made it up out of whole cloth." The pace was extraordinary, and "it should have been noted."

Were faculty members perhaps acquiescing in what they knew to be a slapdash approach because it made the lab so productive? Peters said that for some it was "more than acquiescing." The report notes that some senior faculty members "expected that their names be used even though they had provided only facilities for a project, without substantive contribution to, or knowledge of, the validity of the work." This practice, the report said, made "a mockery of authorship of scientific manuscripts, and in this case may have contributed to the perpetration of research fraud."

The Slutsky case appears to be as significant as an earlier and oddly parallel one—the "Darsee affair"—in which a young and ambitious researcher named John Darsee was accused of fabricating data at Emory and Harvard universities. Both cases involve cardiological experiments on dogs, both involve "touching up the numbers" to improve statistical results, and both raise questions about the responsibility of senior faculty members who were supposedly in charge and whose names appeared on questionable papers.

Lasser uncovered the fraud at UCSD in the process of reviewing Slutsky's massive oeuvre when the young scientist was up for a promotion in 1985. As a widely respected former chairman of radiology, Lasser had been asked to write a letter of support. But in reading two of Slutsky's papers side by side, he suspected that the same "control" animals had been used in both without mention of the fact in either. Identical data points appeared in both articles, but the value of the standard deviation in one was given as the standard error in the other. Furthermore, the actual number of animals

cited in each case was different. This suggested at best a sloppy approach to the facts.

Almost immediately after being asked about the statistical discrepancies, Slutsky resigned and left San Diego. A call to his attorney in New York last week prompted no comment.

"We are leaving it up to the journals to decide what to do next," says Paul Friedman, associate dean for academic affairs, speaking of the 68 papers now judged to be questionable. UCSD's approach differed markedly from Harvard's investigation of Darsee, Friedman said. It put all coauthors on notice that they would have to defend their papers. This was a difficult decision, and many objected. But the dean stuck by this policy, even though he says, "There was no obvious place to end the investigation." If coauthors were unable to document findings, or if there were gaps in the trail of data which could not be explained, the papers were classified "questionable." In contrast, Friedman said, Harvard put its institutional weight on the side of the authors.

UCSD's approach took a devastating toll among Slutsky's co-researchers, particularly among the younger fellows who passed through the lab and whose names were included on his papers. An unofficial count reveals that 13 young researchers were stung by coauthorship of "fraudulent" and 25 by "questionable" papers.

According to Peters and Friedman, the university uncovered in its retrospective review that there were early hints of trouble. The fellows suspected that something was amiss but did not speak up uninvited. When questioned, "They were only too happy to say what they knew," according to Friedman. "One of the fellows was a medical student waiting for an internship, and she didn't know what to do when this faculty member said, 'I've put your name on a couple of papers as a favor.' " Those little favors have turned to black marks.

Friedman said that the cardio-radiology lab was a productive center of false research, but that Slutsky alone knew the extent of the falsity. Fellows did experiments under the supervision of Slutsky or the lab chief. They then wrote up the results, "and Slutsky would use those data as the inspiration for a fictitious paper." The paper would include the names of fellows because "they had in fact created the original paper, but things about the subsequent paper were altered." Even though the fellows were unaware of experiments that would justify changes, they still lacked the confidence to challenge Slutsky. Often, it seems, the changes were designed to make results statistically significant, to squeeze several publications out of the same batch of data, or to satisfy criticisms by journal editors who were reluctant to publish the work in its original form.

What about Slutsky's elders? Seven senior faculty members now suffer from his generosity: all are coauthors of questionable, and three of fraudulent, papers. None was stung more severely than the head of the lab, the chief of cardiological radiology, Charles Higgins. (He is now a radiologist at the University of California at San Francisco.) His name appears on three fraudulent and 21 questionable papers.

Peters said it is understandable that someone seeking tenure might churn out a lot of papers. "It's this damned business of counting numbers of papers for promotion, rather than quality." But for the person on the top, Peters said, "It really is ego: I have 150 papers in my bibliography, somebody else has so many." It all comes down to "a false sense of values," and "I don't think any of us are completely innocent of it."

"When someone is changing numbers, it is very hard to know about it," Higgins said.

Two NIH scientists who studied the Darsee affair—Walter Stewart and Ned Feder—use the term "honorific authorship" to describe the custom of including lab directors' names on all papers produced in a given lab. This approach, according to Stewart and Feder, tends to corrode responsibility and weaken the integrity of science. Their report on the Darsee affair and its implications is soon to appear in *Nature*.

At UCSD, the Peters report notes, "high research productivity is encouraged and reinforced" in academic circles. In Slutsky's case, his "role models in cardiology and radiology published a great deal." Higgins, the chief of the lab in which Slutsky last worked, according to one faculty member, has a "mammoth bibliography." It was launched with an impressive burst of publication during his early years as a research fellow. Asked if he had maintained an "honorific authorship" policy at UCSD, Higgins responded that he had not. He said that his entire bibliography included "less than 300" publications, and that he had produced only 32 papers during his 3-year fellowship.

Higgins added that many of the Slutsky papers were written when he, the lab chief, was on sabbatical and could not be as attentive as he would have liked. At other times, Slutsky presented his findings at a weekly data conference, and the results always seemed correct. "When someone is changing numbers, it is very hard to know about it," Higgins said. He stressed that Slutsky was not his trainee but in some sense a peer, in that he transfered into the radiology department from cardiology and nuclear medicine with a faculty appointment. Higgins' main point was that Slutsky was a "very intelligent" person with good ideas, the kind one trusts. "There was a tendency to want him to do well." Many colleagues agreed on this point, describing Slutsky as both hard-working and creative.

Members of the committee said they wrote but then dropped a recommendation for much closer supervision of young researchers. This step, the chairman said, would have stifled creativity because of "one bad apple." Among the recommendations the committee did adopt were the following: (i) that peer review should focus on the quality, not the quantity of a researcher's work, (ii) that each department should develop a means to identify "the type and degree of participation of every faculty author in each published work," (iii) that coauthorship should "reflect scientific involvement and imply responsibility for the work reported," including a responsibility to defend coauthored papers if called upon, and (iv) that the medical school should develop clearer guidelines for supervising trainees and "realistic" standards of productivity.

UCSD was not the only institution taken in. The National Institutes of Health was stung as well. It awarded a grant to Slutsky as principal investigator in 1985 just as the fraud was uncovered. Numerous journals have been embarrassed. Donald Stewart, managing editor of *Radiology*, said that his journal published a note in January retracting four Slutsky articles. But now the journal's staff confronts a terrible "soup," for it must decide what should be done about all the other articles that are neither clearly fraudulent nor clearly valid. Stewart wondered whether any body of research would stand up to a retrospective demand for justification of the kind being raised at UCSD.

"The system fell down at all levels," said one faculty member. It began working again when Lasser made his study of statistical foibles in Slutsky's work and challenged Slutsky to account for them. The pity is that others did not ask these questions sooner. It was a failure of the senior faculty, Peters said, "that we weren't able to find out before this happened and do something to prevent it. That's the real tragedy." ■

ELIOT MARSHALL