was only 0.15 year. One of the worst aspects of the old Mirex was that its half-life was estimated to be 12 years. The EPA staff, noting that Mississippi's experiments with Ferriamicide are only 3 years old, concluded as follows:

> Mirex even in this formulation is quite persistent, and . . . it appears to degrade by a two-stage process. The halflife of the first stage is about two months but the second halflife is estimated to be at least three years. In fact, a long halflife is intuitively obvious from the [Mississippi] statement that 20 percent of the Mirex remains after three years. The HED review concluded that as much as 40 percent, or 94 percent including degradates, may remain after three years. . . . HED does not believe that the rapid field degradation of Mirex (in Ferriamicide) has been demonstrated.

In addition to being persistent, Mirex is a proved carcinogen in rats and mice. It degrades into—among other substances—Kepone, which is neurotoxic in humans and a proved carcinogen in rats and mice. Because of its stability and resistance to metabolic elimination, it is passed upward through the food chain and concentrated particularly in fish and dairy products. An EPA survey of human tissue in 1976 found that 23 percent of all the samples taken from Mirex-treated areas contained Mirex. Given this record, it is surprising that Mississippi sought, and that EPA agreed to allow, a form of Mirex back on the market. According to Ueltschey, the decision came about in the following way.

After Allied dropped the business and while Mirex was being phased out, Mississippi made about $2.5 million on the sale of Mirex. As required by state law, the income was applied to developing a new, biodegradable ant poison. The state announced after a short period of experimentation that it had such a product: Ferriamicide. The EPA granted an emergency use permit under Section 18 of the Federal Insecticide Fungicide and Rodenticide Act (FIFRA), allowing the use of Ferriamicide for 6 months ending on 30 June 1979. The permit was not used because new Canadian data indicated the chief breakdown product of the new formula was more toxic than Mirex. Mississippi challenged the accuracy of the data, and in October 1981, EPA's scientific advisory board notified the state that the breakdown product was in fact no more toxic than Mirex.

Eager to get on with production, Mississippi in December asked EPA for a "conditional" registration permit to allow the use of Ferriamicide in Mississippi and eight other ant-infested states. The permit was meant to tide the state over until it had enough toxicological

Sharing Credit for the Nobel

"Yes I was very surprised and especially so that I'm getting the prize alone." That was the sentiment of Kenneth G. Wilson of Cornell University when the Associated Press called early on the morning of 18 October to congratulate him on winning the Nobel Prize in Physics.

In an interview with *Science*, Wilson, 46, one of the world's younger laureates, explained his views on dividing credit and giving awards in an era increasingly marked by teamwork. On several previous occasions, the award of the Nobel Prize has been controversial because it has been argued that the prize should have been shared by others who made major contributions. An unusual aspect of this year's award is that the issue of shared credit has been forcefully addressed by the recipient.

What led to Wilson's astonishment was that he fully expected two other scientists to share the prize, since all three in 1980 had shared a taste of scientific glory when they received Israel's Wolf Prize. Given for work in agriculture, mathematics, chemistry, physics, and medicine, the Wolf Prizes carry a cash award of $100,000 each. Wilson shared the physics prize in 1980 with Leo P. Kadanoff of the University of Chicago and Michael E. Fisher of Cornell University. "I was very happy with that," says Wilson, "and it was my sense that the community was happy with it as well."

Wilson, rather than questioning the wisdom of the Nobel committee in singling him out, directed his remarks to the general question of allocating credit. "The unraveling of work and awards is a very serious problem, especially in experimental physics but even in theoretical physics. When you have theories like quantum chromodynamics, which in many ways evolved through a world collaborative effort, dividing up the credit is a difficult problem at best." And the problem in some branches of physics is likely to get worse. For example, CERN, the European center for high energy physics near Geneva, is in the process of building an accelerator known as LEP, a behemoth that will stretch for 16 miles under the French-Swiss border and have teams of 250 scientists taking data from each detector. A dilemma the Nobel committee may one day face is who will get credit for the discoveries.

And even with his theoretical work in phase transitions, Wilson says sharing a Nobel among three workers would not do justice to the spadework of many. "It is a general problem with scientific awards. And certainly, in my nobel lecture, I will be mentioning more people than Leo and Michael."

In the long history of the physics prize, the Nobel Foundation has made a few controversial omissions. The 1923 prize, for example, went to Robert A. Millikan for measuring the charge on an electron. Yet it omitted Harvey Fletcher, a graduate student who performed many of the experiments and, according to the June 1982 *Physics Today*, suggested the critical idea of suspending oil drops between charged plates.

Deserving individuals have clearly been left out in the cold, but the democratic process can go too far, Wilson believes. At some point individuals must be singled out. "The most powerful results must get their do, even relative to other important results. This is especially the case with the Nobel Prize, which has the unique character of being recognized by both the scientific and general community. It's extremely important the community at large sees the very best science has to offer."

Moreover, Wilson takes exception to an editorial that appeared in the *New York Times* on 15 October in which a hypothetical Nobelist renounced the prize. "My discovery," said the newspaper's fictitious laureate, "is a small development of the work of 15 colleagues. Most of my experiments have been carried out by my loyal and unassuming graduate students."

Wilson argues that although points in the editorial are relevant, it missed an important consideration. "Obviously there are difficulties of all the kinds they said, but it is necessary to recognize what the Nobel committee has done despite those difficulties. An atmosphere of trust surrounds the Nobel Prize. Nobody else has come close to that, to achieving the recognition that the award holds among scientists and the general public. There's a very important form of communication that would be lost to the world if you went along with that editorial."—**WILLIAM J. BROAD**

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