(*1*). Several panel members who disagreed with my views in 1987 and 1988 were the same persons who advised the California Department of Food and Agriculture (CDFA) on earlier medfly programs and had declared the state free of medflies. For them to have agreed that medflies were not eradicated would have required their concession that their earlier recommendations had been premature. It is unlikely that they would have made such a concession even if they agreed with me because it would have immediately raised questions about the millions of dollars previously spent on medfly eradication by the CDFA and the U.S. Department of Agriculture (USDA). This quandary stemmed partly from the procedure in all California eradication programs for the same persons who advise on operational aspects of the program to also judge the program's ultimate effectiveness and partly from the inevitable political and economic pressures to declare eradication quickly so that quarantine restrictions could be lifted.

Voss incorrectly characterizes the other panel members' current views of my position—not all of them have said that they believe the medfly has been eradicated in California. He correctly notes that one of the conclusions of the University of California (UC) "Blue Ribbon" Committee to investigate the medfly situation in California was that the medfly was not endemic (*2*). However, he does not say that this conclusion was based on a semantic point. The committee's report stated, "Endemic is defined as native or indigenous to an area. The Committee felt that the use of the word endemic was imprecise" (*2*, p. 3). Voss distorts the spirit and intent of that report by quoting part of their statement out of context. I view as highly significant the UC Blue Ribbon committee's major conclusion that the 1989–90 medfly outbreak was linked to the 1988 outbreak, and possibly to the 1987 outbreak, because in both of these years the CDFA declared the medfly eradicated.

Voss implies that the number of medfly interceptions in California airports is low because the USDA inspectors are not looking for them; that the five interceptions were almost coincidental—the result of larvae "crawling out of fruit." This implication is erroneous, as I fully documented in my article. Both Voss and Saul imply that presence of medfly hosts along entry pathways "explains" the recurrent medfly outbreaks. Fruit presence is not an explanation. Rather it is a precondition for the presence of medfly larvae which, in turn, is a precondition for medfly introduction, then for medfly colonization, and so forth. Indeed, successful biological invasions require a multitude of preconditional steps (*3*). Most key pests and their hosts are present at some time along major pathways because of the scope and intensity of world travel (*4*). Therefore to accept the presence of fruit including medfly-infested fruit along an entry pathway as an explanation for repeated, widespread medfly outbreaks in the Los Angeles Basin is to accept a partial and inadequate explanation for an extremely complicated process. Multimillion-dollar eradication programs designed to protect the multibillion-dollar agricultural economy of California must surely be based on answers to questions far more profound than whether fruit is present along entry pathways.

A step toward addressing these more basic questions was taken when the CDFA recently funded a team of geneticists to determine the original geographic source (or sources) of medflies captured in the state. This group recently completed an analysis of mitochondrial DNA variation in 20 medfly populations from the New World, including 10 feral populations from four Hawaiian islands. They concluded that medfly samples collected from Calfornia in 1989 and 1991 were not derived from Hawaiian flies (*5*). These preliminary results provide direct evidence that contradicts Saul's statement that Hawaii is the likely source of medfly infestations in California. While the data are inconclusive regarding the likely origin of California flies, I believe the genetics approach that this group is pioneering will yield important insights into the nature and origin of the California medfly invasion as well as the characteristics of medfly global spread. This type of fundamental investigation must serve as the foundation on which future exclusion and eradication policy is based.

I appreciate the grave concerns of agricultural administrators such as Voss about establishment of the medfly in California. However, the pest cannot be wished away or legislated out of existence. I stand firmly behind the conclusion in my article that the medfly is established in California. The capture of 26 more medflies in Los Angeles County last fall shortly after my article was published and of one early this year in Orange County further substantiates my position (*6*). The majority of the medflies captured in Los Angeles County were within a few blocks of captures made in previous years. The developing medfly crisis in the state must be dealt with directly, immediately, and decisively (*7*). This cannot happen if agriculture policy-makers continue to insist that the medfly problem in California is under control.

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REFERENCES

1. Minutes of conference call, 28 September 1989, in G. Loughner, Executive Secretary, California Medfly Science Advisory Panel, Memo, 4 October 1989.
2. "Blue Ribbon" Committee on the Establishment of the Mediterranean Fruit Fly in California, R. A. Van Steenwyk, Chairman, "Initial report" (University of California, Oakland, CA, 1 August 1990).
3. R. L. Kluge, H. G. Zimmerman, C. J. Culliers, G. B. Harding, in *The Ecology and Management of Biological Invasions in Southern Africa*, I. A. Macdonald, F. J. Kruger, A. A. Ferrar, Eds. (Oxford Univ. Press, Cape Town, South Africa, 1986), pp. 295–303.
4. G. J. Vermeij, *Science* **253**, 1099 (1991).
5. W. S. Sheppard, G. J. Steck, B. A. McPheron, in preparation.
6. "Summary of 1991–92 Mediterranean fruit fly finds" (Detection Advisory, California Department of Food and Agriculture, Sacramento, CA, 6 January 1992).
7. J. R. Carey, *Calif. Agric.* **46** (no. 1), 13 (1992).

Transmittal of Hepatitis C

A statement in Michelle Hoffman's News & Comment article "Hepatitis A shows promise" that hepatitis C is transmitted largely through blood transfusions (13 Dec., p. 1581) is incorrect. Recent data have demonstrated that only approximately 6% of cases of hepatitis C infection may be attributable to blood transfusion (*1*). Approximately 50% of patients do have a defined parenteral exposure, but the vast majority of cases result from drug use. Interestingly, 40 to 50% of patients have no identifiable source for the infection.

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REFERENCES

1. M. J. Alter *et al.*, *J. Am. Med. Assoc.* **264**, 2231 (1990).

Erratum: In Richard Stone's News & Comment article " 'Third wave': Roiling the waters" (15 Nov., p. 930), it should have been made clear that Graham W. Gibbs did not make a formal presentation at the conference "The third wave of asbestos disease: Exposure to asbestos in place. Public health control," nor did he write a "forward" to proceedings of the meeting.

Erratum: In Joseph Palca's article "A $9-billion budget for NIH" (News & Comment, 8 Nov., p. 791), the subheading of the graph "NIH's (probable) final budget" should have been "Dollars in millions," not "Dollars in thousands."

Erratum: In the report "Hydroxyl radical photo-production in the sea and its potential impact on marine processes" by K. Mopper and Xianliang Zhou (2 Nov. 1990, p. 661), the range of values represented by the x-axis in Fig. 1A should have been 0 to 7.0×10^{-18} M, not 0 to 2.0×10^{-18} M.