LETTERS

Dangerous Trends

A recent editorial (14 Sept., p. 1087) calls for universities to look carefully at their admissions counseling policies, especially in the sciences, since "neither individual students nor their potential counselors can accurately gauge aptitudes, talents, drive, and judgment at the time of entry into the university." The editorial goes on to advise that students of differing abilities be urged to take science courses of varying degrees of rigor. We scientists must also carefully scrutinize some of the conditions that shape student attitudes toward science, and some of the dangerous results of our misreading these conditions.

The same media which have at times "hyped" science, raising career expectations beyond reason, have at other times contributed to society's deeply felt anxieties about science and scientists. The result is a generation of students fearful of science and convinced that they are incapable of comprehending it. Low enrollments in science programs attest to the widespread phenomenon of "science anxiety." This fear, and an accompanying avoidance of science by students in secondary schools, make it difficult for college counselors to accurately assess aptitudes and talents. The disproportionately low numbers of female and minority scientists also suggest strongly that forces other than "aptitude" or "talent" determine who studies science.

One result of our erroneous interpretation of anxiety and avoidance as lack of aptitude has been the introduction of courses that capitulate to the myth that many people can't do science at all. Scientists can only communicate the power and beauty that come with understanding nature by giving their students the technical skills to solve science problems, not by simply lecturing to them about science. The student who completes a science course that requires no scientific skills knows that he or she has been shortchanged.

The student who, on the other hand, is required to *do* science, even in a course for nonmajors, learns to appreciate the creativity of the professional scientist. Having mastered some of the techniques of science, the student gains confidence that he or she can comprehend the important science-based political questions of today. Such a person is unwilling to leave things to the "experts," but rather learns to cast a critical eye on the claims of the various sides of issues such as pollution, reactor safety, and energy. This embodies the Jeffersonian ideal of the informed citizenry as the cornerstone of democracy.

It is up to us, scientists and science teachers, to produce this informed citizenry. We can only do so by recognizing that our society sees science as the preserve of a gifted few (mostly white and male), that stereotypes of scientists are almost always negative, and that potential scientists are thus turned away from science by the fear that they cannot be like us, or by the fear that they can. If we teach nonscientists to do science, then we can give the lie to these stereotypes, so that the preserve of the few can become the province of the many. In the end science wins, and so does society.

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Viroid Discovery

In his review (7 Sept., p. 992) of my book *Viroids and Viroid Diseases* (*1*), Zaitlin states that viroids were discovered by two groups; namely "by Diener and his colleagues . . . and, independently . . . by a group led by J. S. Semancik." The published record demonstrates, however, that the work of Semancik's group with the citrus exocortis viroid (CEV) consistently lagged about 1 year behind the work with the potato spindle tuber viroid (PSTV). Although the detailed methodologies used in the two laboratories differed somewhat, the CEV work cannot be considered as having led to an independent discovery of viroids, because Semancik's group, at each stage of the work, was acquainted with the earlier PSTV results. Rather the CEV work represents a confirmation, with a different disease agent, of previously published results with PSTV.

Recognition of the profound disparity between viroids and conventional viruses required convincing evidence that (i) the pathogen exists in vivo as an unencapsulated nucleic acid; (ii) viruslike particles are not detectable in infected tissue; (iii) the pathogen is a nucleic acid of low molecular weight; (iv) the infectious nucleic acid replicates autonomously, without assistance from a helper virus; and (v) the infectious nucleic acid consists of one molecular species only (*1*).

With PSTV, evidence regarding (i) and (ii) was reported in October 1967 (*2*); with CEV, not until October 1968 (*3*). With PSTV, extensive evidence supporting (iii) was published in 1971 (*4, 5*). In contrast, Semancik and Weathers, in a paper published in February 1972 (*6*), still considered low molecular weight of CEV as only one of two possibilities to explain their results. Only in a later paper (*7*) did they definitely consider CEV to be an RNA of low molecular weight and acknowledge a "class relationship [of CEV] with the potato spindle tuber 'viroid' " —without, however, citing the decisive PSTV paper (*4*) in which the viroid concept had been established.

With PSTV, evidence supporting (iv) was published in the same 1971 paper (*4*) and in a follow-up paper in 1972 (*8*). Zaitlin correctly points out that these small disease agents could have been akin to defective or satellite viruses that require a helper virus for their replication. Yet, the two PSTV papers (*4, 8*) still are the only ones addressing this question. Apparently, other workers, including Semancik, considered our evidence for autonomous replication of PSTV compelling and tacitly assumed that the PSTV findings would apply to other small infectious RNA's as well. Thus, with regard to (iv), the viroid nature of these pathogens (including CEV) has been asserted on the basis of analogy with PSTV, and not as a result of independent work. Finally, some evidence supporting (v) was published for PSTV in 1972 (*9*), and for CEV in 1973 (*10*).

It is regrettable that Zaitlin's statement does not reflect the published record correctly, particularly in view of the following recent statement by R. F. Marsh, T. G. Malone, W. D. Lancaster, R. P. Hanson, and J. S. Semancik (*11*): "Diener's discovery of plant viroids demonstrated for the first time that small nucleic acids could produce disease by themselves. . . ."

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3. J. S. Semancik and L. G. Weathers, *Virology* **36**, 326 (1968). Reference is made in this paper to the similar results reported with PSTV in 1967 (*2*).
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5. _____ and D. R. Smith, *ibid.* **46**, 498 (1971).
6. J. S. Semancik and L. G. Weathers, *ibid.* **47**, 456 (1972).
7. _____, *Nature (London) New Biol.* **237**, 242 (1972).
8. T. O. Diener, D. R. Smith, M. J. O'Brien, *Virology* **48**, 844 (1972).
9. T. O. Diener, *Adv. Virus Res.* **17**, 295 (1972).
10. J. S. Semancik, T. J. Morris, L. G. Weathers, *Virology* **53**, 448 (1973); although a number of workers have later made original and important contributions to our knowledge of viroids (such as more refined size and structure determinations), this was not required for the original establishment of the viroid concept.
11. R. F. Marsh *et al.*, in *Persistent Viruses*, J. G. Stevens, G. J. Todaro, C. F. Fox, Eds. (Academic Press, New York, 1978), pp. 581–590.