gens from a wide variety of classes were mutagens in the test. Furthermore, Lijinsky's study does show that the *Salmonella* mutagenicity assay is an excellent test for the detection of carcinogenic polycyclic hydrocarbons: 88 percent of the carcinogenic hydrocarbons analyzed (14 out of 16) were mutagenic in *Salmonella*. This agrees with a larger study on polycyclics by Coombs *et al.* (*14*), in which 38 out of 38 carcinogenic polycyclics were mutagenic in the test.

Lijinsky obtained the "55 percent correlation" estimate by combining his results on the 16 carcinogens with the 8 "noncarcinogens," of which 8 out of 8 were mutagens. Unfortunately, Lijinsky accepts the results of limited or inadequate cancer tests as definitive evidence that a chemical is a "noncarcinogen." But the word "noncarcinogen" has meaning only in the context of an analysis of the *power of the test*. We have discussed elsewhere (*15*) the problems with using Lijinsky's insensitive cancer tests for determining whether or not a chemical is a "noncarcinogen." His analysis of carcinogenicity was based entirely on skin painting studies, which are considerably less sensitive than the standard NCI feeding studies and do not meet NCI criteria for an adequate test. It remains problematical whether his skin painting studies would have detected very many of the known human carcinogens at the doses used, although the *Salmonella* test detects almost all of these (*13*).

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5. Gehring's estimate, based on an extrapolation from rodent data, is that the risk of liver angiosarcoma in *humans* is about 10^{-8} from exposure for over half a lifetime to 1 ppm of vinyl chloride. This contrasts with a risk of liver angiosarcomas in *rats* of greater than 10^{-3} (and mammary carcinomas of greater than 10^{-2}) from exposure for over half a lifetime to 1 ppm (*2*); in extrapolating to humans we estimate the risk of cancer might be in the range of 10^{-2} to 10^{-1}.
6. P. G. Watanabe, J. A. Zempel, D. G. Pegg, P. J. Gehring, *Toxicol. Appl. Pharmacol.* **44**, 571 (1978); P. J. Gehring, P. G. Watanabe, C. N. Park, *ibid.*, p. 581. It should also be noted that, if Maugh's figure 1, which suggests a threshold, is replotted with dose on a linear scale, the data fit equally well a straight line passing through the origin, suggesting no threshold.
7. "Report on the carcinogenesis bioassay of chloroform" (National Cancer Institute, Bethesda, Md., 1 March 1976); "Individual animal pathology table for chloroform" (National Cancer Institute, Bethesda, Md., 24 September 1976).
8. F. J. C. Roe, unpublished preliminary report of long-term tests of chloroform in rats, mice, and dogs. In four strains of mice, tumors developed in tissue which showed no other pathological damage, and tumors did not develop in tissue which showed pathological damage: ICI-Swiss mice had kidney tumors but no kidney damage; CBA and CF/1 mice had kidney damage but no kidney tumors; and ICI-Swiss mice had liver damage but no liver tumors.
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15. B. N. Ames and L. Haroun, in preparation. For example, Lijinsky (*11*) lists benzo[*e*]pyrene as one of the eight "noncarcinogens." The *Salmonella* test shows that benzo[*e*]pyrene is a mutagen, but it is only 1/360 as potent as benzo[*a*]pyrene. Other short-term tests using animal cells also show that benzo[*e*]pyrene is a mutagen. Before Lijinsky can conclude that these tests are unreliable because benzo[*e*]pyrene is not a carcinogen, he should show that the power of the cancer test is such that it could detect a chemical 1/360 as potent as benzo[*a*]pyrene.

In his review (Research News, 6 Oct. 1978, p. 40) of some of the arguments concerning the existence of threshold doses in carcinogenesis, Thomas Maugh reproduces a graph [from David B. Clayson (*1*, p. 164)] as the basis for some people's argument that high dose-low dose extrapolation of animal data (and hence extrapolation from animal to man) is a nearly impossible task. The graph reproduced is incomplete in that the dose scale—log dose—that was in the original article is left out.

That graph implies a biological model—the logarithm of the percent of animals developing a tumor is linearly related to the logarithm of the dose. Not one of the extrapolation techniques that I know (Mantel-Bryan, Crump *et al.*, Albert-Altschuler, one-hit, multi-hit, log-logistic, and others) uses this model. I know no theory of carcinogenesis that is consistent with such a model. Gently put, the model is inappropriate. Finally, the confidence intervals as drawn can come only from an experiment without a low response at some low dose, that is, an experiment without a control group.

If the point is that an inappropriate biological model combined with a defective experiment is likely to give nonuseful answers, I agree wholeheartedly. If the point is that that graph provides a serious argument against extrapolation using a reasonable biological model from data gathered in a well-conducted experiment, then serious questions arise in my mind about its pertinence and their logic.

By way of analogy, I find that a picture of a centipede is not proof (or even evidence) that a horse is not a quadruped.

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There is a continuing national debate over determining the risks of small amounts of toxic substances by extrapolations of massive exposure studies. The legend of figure 1 of Maugh's 6 October 1978 article on chemical carcinogens states "Most scientists, however, now think that the actual response is indicated by the smooth curve passing through zero dose and zero response." Dose is plotted on a logarithmic scale. In figure 2 response is plotted on a logarithmic scale. How do "most" scientists plan to extrapolate these logarithmic scales to zero?

All models of dose versus response in which the curve increases monotonically *must* include the points (0,0) and (∞, 100 percent), but this does not help to distinguish among them. In probit theory the relationship between log dose and susceptibility is a normal curve. Response is the integral of this normal curve which is "S shaped" but can give the impression of a straight line over narrow dose ranges. Probit analysis requires at least two real points to independently evaluate potency and the standard deviation of a diverse population to this effect.

It is ironic that so much energy is wasted on a debate over what amounts to the proper choice of coordinate axes.

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Mexican Oil Reserves

All of us owe a debt to William D. Metz for his commentary "Mexico: The premier oil discovery in the Western Hemisphere" (News and Comment, 22 Dec. 1978, p. 1261). Recognition in the United States of the Mexican accomplishment is long overdue, and Metz makes his point in a most effective man-

ner. However, for the record, some points need clarification or correction.

I am a former consultant to Petróleos Mexicanos (Pemex) and have been involved with the Chiapas-Tabasco (Reforma)–Gulf of Campeche discoveries since 1972. The data from these discoveries have been released to Francisco Viniegra Osorio, former exploration manager of Pemex (now retired) and the discoverer of Mexico's new giant fields. They will be published in an article by Viniegra to appear during 1979 in the *Journal of Petroleum Geology*. Viniegra and I will present a summary of the Pemex discoveries at the forthcoming American Association of Petroleum Geologists annual meeting in Houston, 1 to 4 April 1979.

On 14 September 1978, Jorgé Díaz Serrano (president of Pemex) and I were among those who presented a review of the new discoveries on "The MacNeil/Lehrer Report." We reported that, in the approximately 35 structures then drilled, 57 billion barrels of oil had been found—20 billion *proved* and 37 billion *probable*. The 200-billion-barrel estimate cited in Metz's article is a projected (that is, *potential* or *speculative*) volume for the drilled *and undrilled* structures together—35 drilled and 100 to 150 undrilled. It is undesirable at this time to project reserve volumes beyond actual knowledge. In addition, on the basis of reservoir performances and production records, Viniegra and I have since downgraded the 57-billion-barrel figure to 47 billion barrels—still a giant reserve by any standard.

I also wish to correct the statement that Pemex's Ixchel-1 well was a "gusher." As of 22 December 1978, the date of this letter, Ixchel-1 has *not* been drilled, despite recurrent reports in trade journals that it has been. My statement that the well has not been drilled is based on letters received today from (i) the Pemex office in Mexico City and (ii) my coauthor Viniegra, who joins me in asking that the official record be set straight.

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On 31 December, Pemex doubled its figure for proved reserves and raised its figure for probable reserves, so that the total for both categories is now 84 billion barrels—up considerably from the 57 billion figure quoted by Meyerhoff. The status of drilling in the Ixchel area is an important indicator of the oil potential in far-offshore areas that have been little explored. Successful drilling in the Ixchel area was first reported on 5 July

Congressional Science Fellowships

The American Association for the Advancement of Science (AAAS), which coordinates the Congressional Science and Engineering Fellow Program, is pleased to announce that the American Society of Zoologists (ASZ) and the Biophysical Society (BS) [in conjunction with the American Society for Photobiology (ASP)] have joined 17 other professional societies seeking applications for a Congressional Science Fellow for 1979–1980. The AAAS will also select and sponsor one Congressional Science Fellow; persons in all fields of science and engineering are invited to apply.

Fellows spend 1 year working as special legislative assistants on the staffs of various members of Congress or congressional committees. Stipends for the AAAS, ASZ, and BS/ASP Fellowships are approximately $18,000 per annum plus a nominal sum for travel and relocation expenses. The $18,000 stipend is in addition to any other source of support available to the Fellow, such as sabbatical salary.

Applicants should write directly to the appropriate society listed below. Deadlines for the three Fellowships range from mid-March to March 31. Information about the entire program and the addresses of all 21 participating societies are available from AAAS.

Dr. E. L. Hess, The Biophysical Society, 9650 Rockville Pike, Bethesda, Maryland 20014.

Dr. Robert Higgins, Department of Invertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C. 20560.

Mr. Charles A. Mosher, Congressional Science Fellow Program, AAAS, 8th Floor, 1776 Massachusetts Ave., NW, Washington, D.C. 20036.

1978 in the *Wall Street Journal*. At a press breakfast the next day in Mexico City, Pemex president Diaz Serrano discussed the new well, going over its spelling and location several times with reporters. According to Charles Green of the Associated Press, who attended the breakfast and based an AP story on it, Diaz Serrano left no doubt the Ixchel well had been drilled and said that it "indicates a considerable platform" of oil. Whether Pemex designated that well Ixchel-1 could not be determined. More detail was given in the September 1978 newsletter of the international oil reporting service Petroconsultants, S.A., in Geneva. Specifying a discovery well with the surname Ixchel drilled by May 1978, Petroconsultants issued a field record giving the well's location (92°22′ west; 21°47′ north), geological features, gravity, and sulfur content. Petroconsultants president J. Dixon told *Science* he had no doubt at all that oil had been discovered at the site—that a field record was authoritative. Not only does Petroconsultants stand by its original report, but the firm reported another discovery in late 1978 made in the same vicinity.

—WILLIAM D. METZ

Editorial Writers

Concerning Rochelle Semmel Albin's letter (19 Jan., p. 228) on the selection of *Science* editorial writers, I hope that the uses to which the editorial page is put will continue to be based on (i) the appropriateness of the subject and (ii) the credentials of the author to handle the subject in a competent and readable manner. This does not lend itself to quotas based on the sex of the author.

I prefer to believe that it is only coincidence that the 19 January editorial is signed by Lucy W. Sells.

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As an occasional author of *Science* editorials, I would like to share my experience with Rochelle Semmel Albin; it may sound too simple, but nothing more was involved than writing an editorial and sending it to the editor. While I have no idea how many editorials are received, I suggest that Albin give the same procedure a try—as I assume was the case with the editorial by Lucy W. Sells, published in the same issue of *Science*.

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