Letters

Fenfluramine Studies

While we were not, to our knowledge, approached for comment before Deborah M. Barnes' Research News article "Neurotoxicity creates regulatory dilemma" appeared (6 Jan., p. 29), as the manufacturer of fenfluramine, we are at this time pleased to present more information to the reader.

First, it has been established that fenfluramine, unlike MDMA (3,4-methylenedioxymethamphetamine, or "ecstasy"), does not possess abuse liability and does not produce physical dependence. Although Barnes likens fenfluramine to amphetamine derivatives, fenfluramine's behavioral and pharmacological effects in animals and humans are very different from those of amphetamine-like agents. In therapeutic doses, fenfluramine, unlike most amphetamine derivatives, does not affect norepinephrine and dopamine neurochemistry. As the World Health Organization stated in 1980, "There is evidence that this drug [fenfluramine] does not have amphetamine-like abuse liability nor is there evidence of significant public health and social problems . . ." (*1*).

Second, clinicians have safely prescribed fenfluramine for more than 25 years, in 90 countries, including the United States, to more than 50 million patients, for the treatment of obesity. We have continuously monitored the clinical efficacy of this drug and have carefully scrutinized reports of adverse effects. The most common side effects are sedation and mild gastrointestinal disturbances. Numerous double-blind studies have demonstrated that fenfluramine greatly benefits patients such as obese diabetics, who must reduce their body weight for medical reasons.

Third, 11 years ago, research reports suggested that parenteral administration of high doses of fenfluramine to rats caused destruction of brainstem serotonin cell bodies. After careful investigation, an advisory committee of the U.S. Food and Drug Administration (FDA) concluded, "There are probably no true pathological changes that can be identified in any rat studies" (*2*). The state-of-the-art methods used in the most recent studies alluded to by Barnes confirm the FDA's findings, that is, fenfluramine in tremendously high doses does not result in the loss of serotonin cell bodies.

Finally, current studies on fenfluramine, particularly those cited by Barnes, call for commentary. The kinetics and metabolism of fenfluramine depend on species, dose, and route of administration. For example, a dose of 40 milligrams per kilogram given subcutaneously in rats leads to a drug brain exposure 700-fold greater than does a pharmacologically active dose of 1 milligram per kilogram given orally.

Under these circumstances, we believe the relevance of the current animal studies to patients who are prescribed fenfluramine is highly speculative.

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REFERENCES AND NOTES

1. *Review of Psychoactive Substances for International Control* (World Health Organization, Geneva, 1980).
2. "Report of Peripheral and CNS Drugs Advisory Meeting," 13 June 1978 (Food and Drug Administration, Rockville, MD, 1978).

Climatic Change and Forests

A recent exchange of letters (30 Sept., p. 1736) discussed the possible interactive effects of climatic change on the world's forests. The letter by Roger A Sedjo (p. 1737) cited, but perhaps misinterpreted, my work.

First, it is not my work but rather that of Kauppi and Posch (*2, 3*) which suggests that the growth rates of boreal forests might increase under a doubled CO_2 scenario, and in some areas more than the 75% to 100% cited by Sedjo. The Kauppi-Posch results derive from an empirical regression relation between effective temperature sum and average annual tree growth observed in data from 19 forest districts in Finland. Of course, growth response might differ between managed and unmanaged forests (*4*) and across other parts of the boreal zone.

Recognizing this as a slim basis for extrapolation to the entire boreal zone, I attempted to verify the Kauppi-Posch model for the six regions used in my analysis: eastern and western Canada, Finland, Sweden, Norway, and the U.S.S.R. Kauppi and Posch (*3*) used data on the current climate to estimate current forest growth rates on a grid 5° E-W and 4° N-S covering the Northern Hemisphere between 38°N and 70°N. I aggregated these point estimates of forest growth to average values for the relevant political units and compared these averages with reported information on the actual rates of forest growth (*1*, p. 211 and table 6.2). Deviations between estimated and actual growth ranged from −0.08 cubic meter per hectare per year (−0.9%) in Finland to 0.89 cubic meter per hectare per year (29.6%) in Sweden. In five of the six regions the deviations were positive. This suggests that the Kauppi-Posch model tends to overstate forest growth when extended to large regions outside Finland, but the margin of error is less than the estimated impact of a doubled CO_2 climatic change.

Second, the changes in forest area quoted by Sedjo overstate what might be expected. His analysis assumes that an "expanded global area of boreal forest" of "approximately 6×10^6 square kilometers" represents an area *additional* to the extant forest. This is not the case. As I clearly state (p. 209), my calculations are for *exploitable* forest area, that is, land with forest growth greater than 0.5 cubic meter per hectare per year. In the discussion of the doubled CO_2 scenario, I further indicate, "In no case [that is, in none of the six regions] does the calculated increase in exploitable forest area under climate warming exceed the current total forest area of the country" (*1*, p. 210). Thus the principal effect anticipated by my calculations is an increase in growth, not an increase in forest area as an ecologist would define it.

While I agree with Sedjo that CO_2-induced climatic warming could conceivably produce some homeostatic response within the biosphere, we should be quite humble about our capability to estimate the magnitude of this effect. Humility is, of course, no excuse for ignoring the problem. My examination of the issues, conducted in 1985, was an early attempt to integrate ecological and economic response models to study the effects of climatic change on the world's forest sector. No doubt better data, better scientific knowledge, and better models will give more reliable results.

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2. P. Kauppi and M. Posch, *Clim. Change* 7, 45 (1985).
3. ——— in *The Impact of Climatic Variations on Agriculture*, vol. 1. *Assessments in Cool, Temperate and Cold Regions,* M. L. Perry, T. R. Carter, N. T. Konijn, Eds. (Kluwer, Dordrecht, 1988), chap. 5.
4. C. S. Binkley and B. C. Larson, in *Forest Decline and Reproduction: Regional and Global Consequences,* L. Kairiukstis, S. Nilsson, A. Straszak, Eds. (Working Paper WP-87-75, International Institute for Applied Systems Analysis, Laxenburg, Austria, 1987), pp. 223-230.

Financial Benefit from Research

As I understand the charges directed toward Scheffer C. G. Tseng (News & Comment, 16 Dec., p. 1497), he is in trouble because he conducted nonfraudulent research dealing with a product in which he had direct financial interest, from which it follows that the results of his work might be biased accordingly. Check me if I am wrong, but I know of no researchers who publish their results who are not in the same situation. Take me, for example. I am an archeologist, employed by a major university, currently working both in North America and in Europe. Given that I am not about to steal and sell artifacts from the sites on which I work, my research has little direct economic value to me. I have yet to dig up a cure for AIDS, and I have never found an ancient clot-dissolving drug. I do, however, publish the results of what I do. Now, we all know what happens to academics who work at major universities and who do not publish. They fail to advance in rank, and they also fail to advance in salary. Even if all the publishing I have done has been done for the sheer love of knowledge, as I am sure must be the case, the truth is that I have gained financially from my productivity. The logic behind the charges directed at Tseng, among many others (for example, Research News, 16 Dec., p. 1505), would suggest that because I will gain financially from my research, a conflict of interest is involved, and the presentation of the results of my work might be warped by my desire to see those results reach a wider world. As an ethical scientist, I would seem to have only two choices given this situation. I can stop doing research, or I can continue doing research but stop publishing. Of course, if all scientists were to stop doing things from which they might benefit financially, the consequences for the country as a whole would be horrendous; but at least we would avoid the behavior engaged in by Tseng, who should clearly be made to suffer for having done something that might do him some personal good.

Oh—please don't let my Dean see this letter. He might like it and give me a raise, and I would not like any of my fellow scientists thinking that I am so unethical that I would gain from something that I wrote that dealt with science.

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Narrow Corridors Stop Falling Soda Machines

I would like to suggest a simple solution to the falling soda machine problem that evidently has not been considered (Random Samples, 6 Jan., p. 32). Although it was probably not with such a hazard in mind, those who make such decisions placed some of our machines in a narrow corridor in which a machine could not fall to the floor because it would be stopped by the facing wall.

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Broad Training for Social Scientists

What would we think of a chemist who calmly assumed that a particular chemical reaction violated the principle of conservation of matter and energy? What would we think of a neurophysiologist who was unfamiliar with basic biochemistry? Not much, of course. The natural sciences are continuous and unified, so that theory in any one field must ultimately be compatible with theory in the others. Training in the sciences therefore begins broadly. So it should be in the social-behavioral sciences.

A behavioral science concept incompatible with evolutionary biology is just as bizarre as a chemical reaction incompatible with basic physics would be. A social science "principle" that is incompatible with known psychology is as wrong as a neurophysiology with impossible biochemistry. Chemists and neurophysiologists do not often make such erors, of course—their training guarantees it. But how would the average social-behavioral scientist know that his or her "theory" makes no sense in terms of an adjacent discipline? Unlike students in the natural sciences, those in the social-behavioral disciplines are not ordinarily required to have a firm grounding in related fields. They should be.

The social-behavioral science tradition of ignorance of related fields was once the path of wisdom. Psychology entered the university at a time when the only evolutionary biology was bad biology; and it was still respectable to explain crime and poverty in terms of family background, race, and "blood," when sociocultural anthropology and sociology were becoming institutionalized. We can only be grateful to a Durkheim, for example, who taught social scientists to look away from psychology and biology. That, however, was around the turn of the century, a century once again about to turn.

While behavioral and social scientists were looking the other way, real progress was being made. Today, for example, a psychologist who wishes to assume that there is such a thing as a general capacity for learning has to reckon with the evolutionary argument that selection is unlikely to favor such a generalized capacity (as opposed to specific cognitive abilities related to specific adaptive problems). A social scientist striving to explain everything in terms of environment and "culture" now must cope with abundant evidence for a very complex, evolved psychology.

The social-behavioral sciences have long sought to become more "scientific" by emulating the emphasis on measurement of the natural sciences. It is time for us to emulate the theoretical continuity of the natural sciences as well.

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Cost of Electricity

People who discuss the issue of investing in more efficient use of electricity versus investing in the construction of more electric generating plants (for example T. M. Besmann, Letters, 13 Jan., p. 243; M. Crawford, Letters, 13 Jan., p. 243) usually omit reference to the human dimension of the costs of new electric generating plants. Let me illustrate. The summer peak demand for electricity in the United States was forecast for 1985 to be 465,000 megawatts (4.651×10^{11} watts) and for 1994 to be 566,000 megawatts (5.66×10^{11} watts) (*1*). This corresponds to a modest annual growth rate of 2.2% and requires the construction of 11,000 megawatts (1.1×10^{10} watts) of generating capacity each year. If conventional fossil fuel plants are built, the capital construction cost of the new plants is around $1.5 per watt. For this rate of growth, this amounts to $68 per year for every one of the 250 million men, women, and children in the United States throughout the period from 1985 to 1994. For even a modest rate of growth of our national electric generating capacity, the human scale of the costs is staggering.

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1. "1985 Reliability Review" (North American Electric Reliability Council, Princeton, NJ, 1985), p. 9.

Erratum: Figure 5 (p. 205) of the report "Observation of individual DNA molecules undergoing gel electrophoresis" by Steven B. Smith *et al.* (13 Jan., p. 203) was printed upside-down.