Letters

Larvadex and the EPA

Eliot Marshall, in his article "EPA regulators take on the Delaney clause" (News and Comment, 25 May, p. 851), discusses the views of John Moore, assistant administrator of the Environmental Protection Agency's (EPA's) Office of Pesticide and Toxic Substances, in the following way: "Although Moore recognizes that Larvadex is a carcinogen at high doses for male rats, he thinks this finding has little meaning for human health."

Clearly, Larvadex is *not* an animal carcinogen. The EPA's proposal to register this product (*1*) states that "long-term feeding studies in which cyromazine was administered to animals did not demonstrate any evidence of oncogenicity." (Cyromazine is the active ingredient in Larvadex.)

The EPA's concern about oncogenicity in this matter relates to melamine, a metabolite of cyromazine, which did result in tumors of the bladders of some male rats fed daily amounts of melamine more than 20,000 times higher than would ever be present in poultry or eggs. The EPA said in the same proposal that the Food and Drug Administration Cancer Assessment Committee "found that melamine is only indirectly responsible for this occurrence in that stones occurred in the bladder only at high melamine doses and it is the stones, not melamine, that are tumorigenic" (*1*, p. 18121). Despite the committee's conclusion, the EPA is, as a precautionary measure, treating melamine as a carcinogen because of a *remote* possibility that the chemical per se may have caused the bladder neoplasms in the male rats. We believe current research demonstrates that the chemical is not oncogenic.

The article also refers to California as "one of a score of states where Ciba-Geigy has been pushing to have Larvadex registered for use on an emergency basis." Ciba-Geigy does not "push" states to request emergency exemptions for use of its products. This is a matter of company policy known to all of our employees who deal with state agencies. Our involvement has been to provide, when asked, a state with product-related information to help support an emergency request. The actual request procedure is a matter between a state's pesticide lead agency and the EPA. I do not believe that California or any other state could substantiate that Ciba-Geigy has pressured any official to seek an emergency exemption for any of our products.

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Defense R&D

In a recent editorial (25 May, p. 821), William D. Carey warns that the research component of the defense budget—the 6.1 category—is particularly vulnerable when cuts in spending must be apportioned. He asserts this is so both because the development part—the 6.2 dollars—of R&D are so much larger than the research dollars but also because the damage done by cuts in the technology base is not felt until several years have passed.

Because the Army also recognizes the value and the vulnerability of its 6.1 program, it has made protecting its research investment a high priority as we make our case with Congress. While we do not know the final 1985 figures, our progress so far is encouraging to those interested in adequate funds for research. The latest indication is that Congress will fully fund the amount requested in the President's budget ($238.8 million). We have experienced no cuts in the Army's 6.1 program thus far. For comparison, the fiscal year 1984 figure is $217.5 million; thus in 1985 we may see growth of 9.8 percent in the Army's research program.

The proper role for the Army Science Board in this process has been assuring Army management that we have constructed and are operating the best and most credible 6.1 program. This, which is but one continuing activity of the Army Science Board, is what I hope Carey meant when he called for advisory board participation in the budget process.

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Government Research Policy

Jeffrey L. Fox's article about the retirement of Julius Axelrod (News and Comment, 1 June, p. 966) raises a number of important issues in the areas of research policy. A major concern expressed in the article is that scientists at the National Institutes of Health, as well as other federal scientists, do not have the same "rights as academics for consulting." I was somewhat disturbed that this serious concern was cast in terms of "entitlement."

Two related issues are of even more fundamental concern. One is whether the federal salary schedule, constrained at the upper end by congressional salaries, is adequate to attract and retain the best scientific capacity. The evidence of erosion of capacity in the federal science agencies is pervasive. The solution must be sought in a salary structure that is more attractive to both beginning and senior scientists. If Congress persists in undervaluing the services of its own members, other ways must be sought to correct the problem—perhaps through a Federal Scientific Service with an independent salary structure—if the erosion is to be reversed.

A second concern is the degree of complementarity or competition between consulting and research productivity. Consultation by federal as well as other scientists with industry should be encouraged. But an attempt should be made to structure incentives to avoid consulting that is competitive rather than complementary with research, scientific communication, or technology transfer. An adequate salary structure is a necessary complement to the development of a policy on consulting that would encourage collaboration among industry, university, and government scientists while avoiding a situation in which financial benefits would dominate consulting decisions. This is an issue which the academic community continues to struggle with, not always successfully. But such a policy is particularly important in order to avoid any conflict of interest, or even appearance of conflict of interest, between public responsibility and personal benefit.

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Will Deterrence Survive a Nuclear Winter?

Herbert A. Simon (Editorial, 24 Feb., p. 775) notes that the public has received the "nuclear winter" findings (23 Dec. 1983, p. 1283; p. 1293) as "just one more chapter," possibly the final, in the story of Armageddon. He correctly points out that the findings on nuclear winter differ from other research results on the consequences of nuclear war not only in severity but in strategic and policy implications (*1*). His conclusion that a scientific confirmation of the nuclear winter findings would render nuclear weapons suicidal, however, and that "the futility of mutual deterrence [would be] complete," ignores the devastating consequences of a nuclear strike below the nuclear winter threshold.

Even if we assume that the results of the nuclear winter scenario of lowest explosive yield, 100 megatons, were replicated in upcoming collaborations (Letters, 13 Apr., p. 110) and, even if we were to attach a 99 percent confidence interval of ± 5 megatons to the threshold point estimate (given that other assumptions of the model were met), each side would still be capable of launching a nuclear attack of more than 90 megatons without committing suicide. With the new generation of American Pershing II and ground-launched cruise missiles averaging between 10 and 50 kilotons per warhead, the United States would be able to launch thousands of nuclear weapons. The Soviet Union, with its SS-20 missiles and corresponding developments, would also be able to launch several hundred, if not thousands, of weapons. To put this into perspective, the Cuban Missile Crisis, which involved enough missiles to kill 80 million Americans (*2*), arose from concern over 42 medium-range ballistic missiles and 24 to 32 intermediate-range ballistic missiles.

Thus, even if the nuclear winter findings were confirmed, the motivation for each side to maintain a deterrent would likely continue. What would change is not deterrence, but the deterrent itself. The deterrent for optimal security would move from the most powerful nuclear force available to a nuclear force positioned in such a way that it would take more than the nuclear winter threshold amount of weapons (plus a margin of error) to destroy the target's retaliatory capacity. Any nuclear weapons beyond that amount would not only be militarily superfluous, they would, as Simon indicates, contribute a nonzero probability of accident or miscalculation.

This probability is far from negligible. In fact, William Perry, former under secretary of defense for research and engineering and member of the President's Commission on Strategic Forces, has asserted that "the most realistic danger posed by nuclear weapons is the risk of nuclear war by accident or miscalculation" (*3*, p. 18). The worldwide network of nuclear warheads is a system with 50,000 "moving parts," each component of which includes still more parts in its own system and linkage to the network. In view of these numbers, it is hardly surprising that there have been hundreds of American false alarms depicting an imminent Soviet attack and at least 32 Broken Arrows, or major accidents involving nuclear weapons (*4*). No comparable figures are available for Soviet nuclear accidents.

One of the most effective ways to reduce the chances of a nuclear war is, of course, to reduce the number of operative nuclear weapons, and here rests the central policy implication of a nuclear winter threshold. What the findings on nuclear winter contribute is an assurance that, at present levels of armament, a reduction of our nuclear forces will not lead to a reduction in our nuclear deterrent. The United States nuclear arsenal stands at more than 10,000 megatons today, and the Soviet arsenal, at somewhat more than that. If a nuclear winter threshold of 100 to 200 megatons were confirmed, this would mean that the nuclear disarmament equivalent of more than 90 percent of all existing explosive yield could be safely undertaken without even addressing the issues of deterrence and verification (as cheating would be to no advantage).

While the confirmation of a nuclear winter threshold may well replace nuclear deterrence as we have come to know it in the 1980's, it will certainly not supplant nuclear deterrence in general; confusion of the two is but a testament to how much fat can be trimmed before hitting the bone of the problem.

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Immortality

In the article "Gene therapy method shows promise" (Research News, 30 Mar., p. 1376) Gina Kolata states that "Cells of the bone marrow . . . contain stem cells that are immortal . . . and they essentially divide indefinitely." This notion, if true, has profound implications in many biological disciplines, not the least of which are gerontology, developmental biology, and evolution. Contrary to the quoted statement, there is no unequivocal proof of the immortality of any normal vertebrate somatic cell population studied in vivo or in vitro (*1*). In fact, the literature is replete with reports of the replicative finitude of normal bone marrow cells and other normal hematopoietic stem cells. A few examples are given here (*2*).

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The choice of the word "immortal" was unfortunate, but I hope that most readers will have understood what was meant. After all, it takes a long time to show that any cells are truly immortal.

—GINA KOLATA

Erratum: In the article "Inherently safe reactors and a second nuclear era" by Alvin M. Weinberg and Irving Spiewak (29 June, p. 1398), figures 1 and 2 were interchanged. The captions are correct.

Erratum: In the credit for the photograph on page 1086 of the issue of 8 June accompanying the Research News article "Crystal anisotropy directs solidification" by Arthur L. Robinson (p. 1085), Kurt Nassau's affiliation is incorrectly given as Western Electric. Nassau is at AT&T Bell Laboratories, Murray Hill, New Jersey.