al model originally put forward by Cairns *et al.* (*7*). Thus, the debate has lost some of the excitement caused by the provocative initial suggestion made by Cairns *et al.* in 1988 (*7*, p. 142) that "cells may have mechanisms for choosing which mutations will occur."

The alternative explanations that we presented (*1*), and which Foster disputes, are merely hypotheses; but they are amenable to careful experimental analysis. Regardless of the correctness of any particular hypothesis, this debate has focused well-deserved attention on molecular mechanisms of mutation, the physiology and ecology of starving bacteria, and the evolutionary causes and consequences of mutation.

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The Cost of Energy Efficiency

We write in response to Amory B. Lovins, Carl Blumstein and Jeffrey Harris, and Peter M. Miller (Letters, 20 Aug., pp. 969–971) about our Policy Forum "What does utility-subsidized energy efficiency really cost?" (16 Apr., p. 281). Lovins argues that, because of differences in technologies and accounting conventions, it is "meaningless" to compare the costs of energy conservation (as reported by utilities) with the aggregate cost projections developed by organizations such as the Rocky Mountain Institute (RMI), which he founded. We disagree.

The RMI conservation "supply curve" has been widely displayed in policy circles and in the media as a guide to the need for, and likely cost of, energy conservation policies. The actual performance of conservation programs is the best basis for determining whether current policies are delivering the promised benefits. What Lovins says are "state-of-the-art" technologies may not be used in these programs because those who

develop, implement, and pay for them may not be impressed by such technologies.

We have developed a set of economic and accounting principles to measure the costs of utility conservation programs. These principles appear to be widely accepted. Our original study (*1*) applied these principles to data from a group of utilities so that the costs of their programs could be examined on a consistent economic basis. Where this was impossible we explicitly discussed (*1*) the biases that would likely result.

We do not agree that we have "mischaracterized" Lovins' findings or those of others. The derivations of RMI and Electric Power Research Institute (EPRI) curves are discussed and compared in great detail in our original study (*1*). The much briefer Policy Forum failed only to note that, unlike EPRI, RMI apparently does not exclude from its calculations the effects of naturally occurring conservation. Also, contrary to Lovins' assertion, the EPRI curve is based on the assumption that "the most efficient electric technologies known today . . . attain complete market saturation in 2000" (*2*). This "saturation" appears to be the same concept as Lovins' "asymptote."

RMI has developed cost and performance data for many electric devices, some commercial and some experimental, each under specific usage conditions. To develop a conservation "supply curve" for the nation, RMI aggregates these data across all applicable electric end-uses. The results are based on many assumptions about the generality of usage conditions, marketing, installation, monitoring costs, and the potential market for each device and not, as Lovins would have it, only on empirical data.

Lovins suggests that our data are invalid, inscrutable, and not reproducible. These assertions are unfounded. Our data (*1*) were derived from published reports from major utilities, many of these regarded as leaders in conservation (for example, Central Maine Power, Massachusetts Electric, and Pacific Gas & Electric) (*3*). We see no reason to assume, as does Lovins, that the reported costs may be systematically too high. We also make clear (*1*) how we determined cost per kilowatt-hour (kWh) saved. While these calculations are tedious, they are certainly reproducible.

Lovins argues that administrative costs should be small in good programs, but his only evidence is for a single utility program in 1984. Our study relied on more recent data for multiple utilities which indicate that these costs are often quite large.

Lovins also comments on our interpretation of measured as opposed to engineering estimates of savings (footnote 2 of his letter). Nadel and Keating (*4*) did not lead to an opposite conclusion from ours. They found, for example, that the median ratio of measured to projected savings was 63%. Our conclusions are also supported by the experience of other utility programs (*5*). Lovins rejects the use of control groups and appropriate supporting statistical analysis to verify energy savings (his footnote 2). However, without proper control groups, along with adjustments for "free riders," it is difficult to account for the many factors besides conservation that may influence energy consumption. The uncontrolled studies Lovins relies on do not provide accurate estimates of energy savings.

Blumstein and Harris argue that our cost-per-kWh figures may have been too high because data from low-income residential programs (possibly undertaken for reasons of income distribution rather than cost-effectiveness) may have been included. We found, however, that such programs "do not appear to be systematically more expensive than other residential programs" (*1*, p. 55). Moreover, it is not obvious that spending money on inefficient energy conservation programs, whose costs are then recovered through higher electricity rates, is a particularly good way to redistribute income to the poor.

Blumstein and Harris also suggest that we did not consider the potential benefits of "market transformation." If market transformation is a primary goal, most utility programs are poorly designed because they rely on a central planning philosophy that treats conservation as a utility resource rather than as a market-driven, customer resource. We did not say that conservation programs should be "deemphasized"; rather, programs and their evaluation should be improved so that they really help to remove market barriers and to facilitate wise energy conservation choices by consumers.

Miller argues that our comparison suffers from an "apples and oranges" fallacy because the RMI and EPRI aggregate projections are based on the installation of a range of technologies in average climate conditions, while utility programs use different technologies in specific climates. We recognize the possible differences in technology; in the absence of comprehensive evaluations of a large number of programs from across the nation, such differences are unavoidable. The programs that are most likely to be climate sensitive are those related to electric heating and cooling. While heating and cooling programs are probably underrepresented in our sample (*1*), the EPRI study (*2*) indicates that conservation opportunities in these areas are modest and carry relatively high engineering costs.

We agree with Blumstein and Harris that better data should be developed and we hope that this will be done by the Database on Energy Efficiency Programs at Lawrence Berkeley Laboratory, noted by Miller, and

by similar projects. Using such data properly to evaluate and improve utility energy conservation programs is, however, at least as big a challenge as collecting it.

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References and Notes

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2. Electric Power Research Institute, *End-Use Energy Efficiency* (EPRI, Palo Alto, CA, undated), p. 7.
3. More than half of the utilities in our sample have programs included on the list of "the best" utility programs by T. Flanigan and J. Weintraub [*Electr. J.* **4** (no. 6), 53 (1993)].
4. S. M. Nadel and K. M. Keating, in *Proceedings of the 1991 International Energy Program Evaluation Conference*, Chicago, 21 to 23 August 1991 (Evanston, IL, 1991), pp. 24–33.
5. P. L. Joskow and D. B. Marron, *Electr. J.* **6** (no. 6), 14 (1993) (see footnotes 6 and 8).

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NIDR Report

The ScienceScope item "NIH sinks its teeth into dental debate" (20 Aug., p. 975) implies that Ruth Kirschstein, acting NIH director, supports the content of the Blue Ribbon Panel report on the National Institute of Dental Research (NIDR) intramural program, particularly the suggestion about a narrowing of the focus of the intramural program. Kirschstein has never made a comment on the content of that report. She *has* said that she supports the efforts of the NIDR leadership and its National Advisory Council to review its intramural program and the basic methodology they used, that is, consideration by a group of outside experts.

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Corrections and Clarifications

In the article "Evidence found for a possible 'aggression gene' " by Virginia Morell (Research News, 18 June, p. 1722), the collaborative teams of Peter Seeburg at the University of Heidelberg, Germany; Jean Shih at the University of Southern California; and Creed W. Abell at the University of Texas, Austin, should also have been credited with cloning the human monoamine oxidase (MAO) A and B genes. Seeburg and his colleagues published their research in the July 1988 issue of the *Proceedings of the National Academy of Sciences*. Xandra Breakefield and her colleagues reported their cloning of the MAO A gene in the October 1988 issue of the *Journal of Neurochemistry*. Although Breakefield's group also cloned the MAO B gene, they did not publish this research.