LETTERS

Acid Rain Funding

The proposed funding in fiscal year 1984 for acid deposition research at the Environmental Protection Agency (EPA) is incorrectly reported in Marjorie Sun's News and Comment article of 11 February (p. 749). The correct figure is $14 million, not $24 million. Furthermore, the $40 million that Sun states was the EPA funding for acid rain research in fiscal year 1982 is actually the cumulative amount spent by EPA up through fiscal year 1982 on acid rain research. No major cuts have been proposed in total funding since the national program began in fiscal year 1982.

Proposed funding in fiscal year 1984 for research under the National Acid Precipitation Assessment Program is $27.6 million. Through the actions of the Administration and Congress, funding for this 12-agency federal effort has risen from $18.3 million in fiscal year 1982 to $22.3 million in fiscal year 1983.

This national program has an integrated planning and budget process that is unique among federal research programs. The coordinated interagency research effort proposed for fiscal year 1984 includes the following major agency participation: EPA, $14 million; National Oceanic and Atmospheric Administration, $3.1 million; Department of Agriculture, $2.8 million; Department of Energy, $3.3 million; and Department of the Interior, $4.4 million. In addition, the National Science Foundation (NSF) sponsors about $1.5 million in research related to acid rain. The NSF projects are coordinated with the national program but are not included in its "core" budget because they are basic research and do not specifically address critical policy questions.

CHRIS BERNABO
Interagency Task Force on Acid Precipitation, 722 Jackson Place, NW, Washington, D.C. 20006

Evolution: A Cyclical Argument?

Thomas J. M. Schopf and Antoni Hoffman (Letters, 4 Feb., p. 438) and Stephen Jay Gould (Letters, 4 Feb., p. 439) argue endlessly about the mode of evolution, apparently because they think they are offering alternative explanations of a set of facts. Gould goes so far in his letter to *Science* as to mention long geologic sections and a "database." Unfortunately, gradualism is logically unprovable, so a choice between that mode and punctuated equilibrium always must devolve to a matter of personal preference.

Given a fossil in the lower part of a formation and another in the upper part, one may infer that the lower one is an ancestor of the upper one. Say the formation is rather thin, so the two fossils are only narrowly separated; one can say that it is highly likely that the earlier form gave rise to the later form. One can say this, but it always is an inference: it cannot be proved. Imagine that the fossils are tiny microfossils, separated only by millimeters, and that one is slightly different from the other. It can be said that one gave rise to the other and the relationship is clearly one of gradual change, but this remains an inference. It also can be claimed that the two evolved separately, far apart, and represent either separate migrations or washings-in to the place where they are found. There is no way out: that A gave rise to B always must be inferred.

Historically, the punctuation model comes up with cyclical regularity under various names (for example, "saltation," "allopatric speciation") and a furor ensues. Then the futility of the argument becomes apparent to most who examine it and it tends to go away, while gradualism seems to retain its hold on most minds. Perhaps it is now time to end the current cycle, recognizing that the question is philosophically intractable and therefore is a pseudoquestion.

RICHARD E. GRANT
Department of Paleobiology, National Museum of Natural History, Washington, D.C. 20560

Understanding Cancer

I believe the current rush to accept cellular oncogenes as the origin of human cancer (Research News, 19 Feb. 1982, p. 955) (*1*) is at best premature. I have discussed previously some of the problems inherent in a simple genetic interpretation of cancer (*2*), and others (Letters, 15 Oct., p. 214; 10 Dec., p. 1069) have pointed out flaws in experimental design which raise serious questions of interpretation of the results that engendered the present excitement. Further detailed criticism is unlikely to have much effect. It should be pointed out, however, that explanations for the origin of cancer have been varied and plentiful in this century. A limited list would include early theories of chromosomal alterations, virus infection, high glycolytic rates, and damaged grana (mitochondria), enzyme deletion, and reduced immunological surveillance. Each of these was carried to the fore by developments in a corresponding area of basic biology or biochemistry, and each time many were convinced that a final answer had been found. In retrospect, the supporting evidence always was strong, but it later turned out to be inadequate to establish causality. I believe we have confused advances in molecular biology and its attendant technology with deepened understanding of the nature of malignancy. Current unqualified acceptance of oncogenes rests on two risky assumptions: (i) that the malignant character of cells is analogous to a conventional hereditary trait of somatic cells, and (ii) that the transmission of such character in a line of cells is only possible if there is a change in the sequence of nucleotides in DNA, whether it is brought about through gene mutation or chromosomal transposition.

With reference to (i), the malignant transformation of cells involves a large variety of cellular characteristics. Recent evidence shows that the population of a tumor is extremely heterogeneous with regard to some of its most important characteristics, including metastasis, which practically defines malignancy (*3*). In the latter case, there appears to be a continuous distribution of metastases. Most mutations affect unit characters and are discontinuous. Even those which are pleiotropic determine only a few traits and do so discontinuously. Cancer involves a loss, to a lesser or greater extent, of most of the differentiated characteristics of cells rather than a discrete change in particular properties.

With reference to (ii), the most common cause of hereditary change in somatic cells of metazoa is differentiation, which—aside from lymphoid cells—does not require a change in DNA sequence. Unfortunately, we are far from understanding the chain of causality in differentiation. It is not unlikely that interference with this epigenetic type of process is at the root of malignancy. Whether differentiation is ultimately described in reductionist or holistic terms, a deeper understanding of malignancy is likely to depend on it. I have heard it said that people prefer an explanation that is probably wrong to no explanation at all. But that is a problem of human fallibility that the scientist must guard against, particularly in the cancer field, where so much is at stake.

HARRY RUBIN
Department of Molecular Biology, University of California, Berkeley 94720