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.

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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.

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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.

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Physics Nobel Prize

There is one shortcoming in the otherwise beautifully composed article by Philip W. Anderson (19 Nov., p. 763) on the 1982 Nobel Prize in Physics. Anderson writes: "Experimental observations of singular behavior at critical points (such as deviations of critical fluctuations or 'critical opalescence' from the naïve Ornstein-Zernicke [sic] form predicted in the 1930's) multiplied as the years went on."

Ornstein and Zernike actually developed their prescient theory of correlated density fluctuations as early as 1914 (*1*). They intended to correct a major problem with the (naïve?) Einstein-Smoluchowski treatment of critical opalescence, which led to a diverging, angle-independent scattering intensity at the critical point. Ornstein and Zernike demonstrated that near a critical point density fluctuations in adjacent volume elements become correlated; this, in turn, leads to a decrease in scattering intensity as the scattering angle increases from 0° to 180°.

The physical picture behind the Ornstein-Zernike scattering equation is that, although all density fluctuations become more likely as the critical point is approached, only the long-wavelength ones, responsible for the forward scattering, can grow without bound; the shorter wavelength fluctuations carry an extra cost in free energy proportional to the square of the density gradient. The concept that a density inhomogeneity bears a free energy cost proportional to the square of the density gradient was introduced by van der Waals in 1893 in his theory of surface tension (*2*); the employment (albeit implicit) of the same device in the description of supercritical density fluctuations is a demonstration of genius at work. The theory of Ornstein and Zernike has been so successful that, unlike van der Waals' mean field theory, it has survived until the present day as the correct representation of critical light scattering at low angles. It is embedded in the so-called Landau-Ginzburg-Wilson Hamiltonian, which is, after all, just the limiting critical mean field free energy density as given by van der Waals with the square gradient term added. Although the results for the structure factor as derived from the renormalization-group theory applied to the Landau-Ginzburg-Wilson Hamiltonian differ essentially from the original results of Ornstein and Zernike for fluids, it is by such a minute amount that no more than the *sign* of the departure has been established by the best light-scattering experiments available.

Thus the sentence quoted suffers from two defects:

1) The usage of the word "naïve" does not do justice to the fundamental role of the Ornstein-Zernike theory in the renormalization group approach; and

2) The "[e]xperimental observations of singular behavior at critical points" that "multiplied as the years went on" did not include definitive observations of departures from Ornstein-Zernike scattering.

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In Anderson's penetrating review of the scientific contributions of Kenneth G. Wilson which led to his 1982 Nobel Prize in Physics, there are two errors in a footnote on page 764 that I would like to correct. First, he attributes the introduction of the concept of the so-called renormalization group in quantum field theory to Gell-Mann and Goldberger. The Gell-Mann part is correct; but the Goldberger is not, unfortunately, as I wish I had been involved in this profound work. Gell-Mann's collaborator was Francis E. Low. The second error is the statement that the technique was developed to analyze the infrared (low frequency) divergence in quantum electrodynamics: in fact, it was used to study the ultraviolet (high frequency) divergence behavior of the theory.

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Information Technology

With regard to Philip H. Abelson's thoughtful editorial "Leadership in computer technology" (7 Jan., p. 11), let me add my concern in the more general area of information technology. Both Germany and Japan have national information policies and goals. The United States, on the other hand, has neither a policy nor a single government institution dedicated to advancing information technology per se. Foreign investments in information technology are in great evidence. In the area of optical disk technology for mass storage of computer data, Phillips–Eindhoven (Holland), Phillips–North America (Holland), Toshiba (Japan), and Thompson CSF (France) are some of the main contenders.

In the area of optical video disks, which offer much to information technology for both graphics as well as for the "publication" of digital data or information, the real strength lies in Japan, Holland, and France. It is interesting to note that, not only is there an obviously large share of the home entertainment market, including video disk and audio disk technology, going to Japan, but the once-American firms of Magnavox and Sylvania are now owned by Phillips (Holland).

In another area, two information enterprises founded in the United States, Bibliographic Retrieval Services (BRS) and Predicasts, are now owned by interests in Holland. BRS is one of the three largest general on-line information retrieval services in the United States. Predicasts supports and provides on-line access to the largest private (nongovernment) database on business products and activities in the world.

There are numerous other examples, but the general picture should be clear—if not obvious. We have good reason to be concerned about losing our lead, not only in computer science but also in applied information technology and services. Without more resolve and dedication on our part to do those things that have to be done on a national scale—planning, research and development, education, innovative applications, and cooperative programs—we stand to lose not only the lead but our ability to compete as a nation. We need to move from concern to action, and quickly.

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Correction

The AAAS Annual Meeting will be held in Detroit on 26–31 May 1983. The inclusive dates were given incorrectly as 21–31 May in the heading of the Preliminary Program (25 Feb., p. 948). The large body of water shown to the south of Detroit on the cover of the issue of 25 February is Lake Erie. The cover legend stated incorrectly that it was Lake Huron.