Letters

Earth's Early Atmosphere

J. F. Kasting and T. P. Ackerman (Reports, 12 Dec., p. 1383) present results of yet another computer-generated model for the evolution of the earth's atmosphere. The work indicates that, given certain starting assumptions, a dense CO_2 atmosphere could have coexisted with the oceans early in earth history. They conclude (i) that this result can be accomplished "without violating any known constraints on the planet's subsequent evolution" and (ii) that it precludes "the possibility of an oxygenic prebiotic atmosphere caused by photodissociation of water vapor followed by escape of hydrogen to space." These conclusions deserve comment.

There are at least two constraints that appear to have been left out of the model and presumably violated: (i) the development of a Precambrian ocean isotopically lighter in hydrogen than the mantle source rock from which it was presumed to have outgassed and (ii) the development and maintenance of some sort of atmospheric protection against the higher-than-present fluxes of solar ultraviolet radiation (necessary to mediate the origin of nucleic acids and allow a continuity in the evolution of life, especially photosynthetic life). The former demands a loss of photodissociated ocean water hydrogen to space. The latter can be best accomplished by means of a minimal ozone screen, something which the former could provide with the oxygen left over after hydrogen loss.

Changes in the ozone screen have been important in evaluating the radiation effects of nuclear winter or bolide impact scenarios on the oceanic plankton and to our own survival. The radiation factor should be at least as important in studies of the early earth—even more so in view of the higher ultraviolet fluxes involved. Computer-generated models could be designed explicitly to require that whatever photochemical changes take place in the early atmosphere over time, they be accomplished in such a way that the potentially lethal ultraviolet flux at the earth's surface is maintained at or below some value which would permit life to originate, proliferate, and diversify. This boundary condition may be considered on a par with such traditionally incorporated model requirements as ocean water temperatures above freezing but below boiling or initial silicate weathering rates rapid enough that carbonates can conveniently remove sufficient CO_2 in the time required to prevent a runaway greenhouse. If in the end all such computer models prove unstable, then the starting assumptions may need to be reconsidered. For example, early outgassing might be combined with the input of volatiles from cometary sources.

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Response: Towe raises two objections to our model of a dense, CO_2 early atmosphere. The first is that the deuterium-to-hydrogen ratio in the present ocean is higher than in typical mantle materials (*1*). This is taken to imply the loss of large quantities of water, presumably by photodissociation in the atmosphere followed by escape of hydrogen to space. Preferential escape of the lighter isotope could account for the observed deuterium enrichment. According to Towe our model does not explain this phenomenon because the primitive stratosphere is predicted to be dry and, hence, the hydrogen escape rate from this process should have been low. However, we have not precluded the possibility that substantial quantities of hydrogen were lost by other mechanisms. One plausible example would be photostimulated oxidation of iron in the oceans (*2*) followed by escape of H_2. Furthermore, we specifically limited our model to the time period after the accretionary process had slowed down, that is, subsequent to the first 100 million years or so of Earth's history. During the accretion period itself a steam atmosphere is expected (*3*), and hydrogen should have been lost rapidly by the mechanism that Towe envisions.

The second point that Towe raises—the perceived requirement that the primitive earth possessed an ozone screen to protect early organisms from solar ultraviolet (UV) radiation—is a matter of opinion rather than of fact. For that matter it is an opinion that is not, to our knowledge, widely held among paleobotanists. An alternative viewpoint is presented in the discussion of the early evolution of life by Schopf *et al.* (*4*). Prokaryotic organisms are known to be more resistant to UV radiation than are eukaryotes. This observation is consistent with their having evolved in an earlier, higher UV environment. The proliferation and diversification of oceanic plankton between 2.5 and 1.5 billion years ago may have been related to a decrease in biologically harmful UV radiation during that time. Before the establishment of an ozone screen organisms may have protected themselves from irradiation by living under a protective layer of water, or soil, or the bodies of other organisms. Thus, along with many other workers, we believe that life could have evolved under a high UV flux. The absence of O_2 (and hence ozone) from the early atmosphere is furthermore often considered to have been essential in order for life to have originated in the first place (*5*). This latter requirement may be more fundamental than the one that Towe has suggested, in which case our model is in good accord with evolutionary constraints.

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Novelty of "Supply-Side Ecology"

It is heartening to learn from Roger Lewin's 3 October article (Research News, p. 25) that intertidal ecologists and theoreticians are recognizing the importance of recruitment processes. Lewin writes that "[i]nterest in the potentially broad impact of the supply of new members to community dynamics has been developing . . . for at least half a dozen years. . . ." Without disputing the main points of the article, I feel compelled to point out that, even outside fishery science (where recruitment studies are the stock and trade), studies of larval abundance and settlement have been a mainstream endeavor ever since delay of metamorphosis was discovered over a half-century ago (*1, 2*) and T. C. Nelson (*3*) began correlating recruitment levels of oysters with abundances of larvae and their predators. Following the early thinking of Thorson (*4*), Wilson (*2*) and others, many British, Scandinavian, and Russian ecologists have consistently discussed the role of larval supply since the 1950s. Relative newcomers to the hoary field have boldly suggested that larval supply may influence populations of intertidal barnacles, even though literally dozens of papers have already been published on barnacle recruitment, including some classics of empirical fieldwork (*5*). A major assumption of one of the new models is that "[t]he rate of settlement per unit of unoccupied space is assumed to be determined by factors outside of the local system"

(*6*, p. 54). These external factors, which still have not been studied by intertidal modelers, are the very processes that larval ecologists and zooplankton ecologists have been trying to quantify for years. Although the logistical problems of working with microscopic larval stages in the field have often limited investigations to methods with less power of inference than might be desired, the large body of literature produced in these studies deserves to be credited. Jonathan Roughgarden and his colleagues, like fellow ecologists Joseph Connell and Anthony Underwood, are making important contributions, not only with data, but by directing the attention of intertidal experimentalists to the plankton. Nevertheless, the trendy new field of "supply-side ecology" seems more than anything else to be a novel way of introducing existing ideas to an audience that has been slow to acknowledge them in the past.

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U.S. Management and Productivity

M. N. Baily appears to miss the point of the argument that he presents under the heading of "Management failures" (Articles, 24 Oct., p. 443). The issue is not whether "both old-style and new-style managers made their share of mistakes." Of course they both did. Baily states that the predominant management attitude taught M.B.A. graduates in the 1970s was "to achieve quick results before they move on to the next industry" (the product is irrelevant—only the bottom line counts). On the whole, corporate managers made few mistakes as they assiduously followed this philosophy to its logical conclusion.

But when management lacks commitment to product or to quality, it transmits that attitude to its employees. If the only corporate priority is to maximize current profit with the least investment, can (indeed, should) labor's attitude be any different? Both groups are behaving "rationally" within their perception of optimum strategy in a free-market economy. Given the monomaniac preoccupation with short-term results, the plateau in productivity is not surprising.

Unfortunately, economists and society are only now beginning to realize who the hindmost turned out to be, and that the devil has indeed taken them. The Pogo principle has struck again.

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Baily's review of trends in U.S. productivity growth leads him to endorse the counterintuitive idea that technological innovation has been declining in the U.S. economy in recent years. This seems a strange argument, as the development of the computer on a chip in the early 1970s has led to extraordinary innovations in both products and production processes.

Perhaps the problem is that the data Baily draws on have been inadequately sensitive to the economic impacts of computerization. He does not report, for example, that those who compile the data on gross national product (GNP)—from which his figures are drawn—did not have, until quite recently, a price index for computers. Since their prices have been falling sharply, the lack of a price index led to a significant understatement of GNP growth. When government economists introduced a price index for computers at the end of 1985, they found that GNP in 1984, measured in 1972 dollars, was $100 billion higher than previously thought (*1*, pp. 16–17). This increased the annual growth rate of total GNP from 2.7% to 3.6%. (To be sure, when the statisticians shifted to 1982 prices, this $100 billion gain disappeared almost completely. This extraordinary sensitivity of the data to a technical change such as a shift in the base year, however, is simply another indication of the problems in the existing measurement techniques.) Moreover, there are many other electronics-based products for which adequate price indexes have been lacking or where existing techniques confuse technologically generated price declines with reductions in output.

At the same time, there are many sectors of the economy in which increased use of computers is not reflected in increased output because of other measurement problems (*2*). For government, banking, health care, retail trade, and other sectors, there are only indirect measures for changes in constant dollar output. This means that any computer-based productivity advances in these sectors are unlikely to show up in the GNP data.

In short, the existing accounting scheme has cumulative defects. The contributions of electronics to increased productivity are measured adequately neither at the point of production nor in many of the industries in which the technologies are actually used. Before we wring our hands about lack of technological innovation in the United States, we would do well to make some innovations in the 40-year-old accounting schemes used to measure output and productivity.

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Response: Block states that the idea of declining innovation in the 1970s is counterintuitive, given the innovations in computer technology that took place. He also states that the contributions of the computer to productivity are not being picked up in the industries that use them. I agree with both points and, indeed, made them in my article. My agreement is qualified by a skepticism about the actual contributions computers are making to productivity.

I am familiar with the problem of the government's price index for computers. I did not discuss it in my article because index number theory and practice are somewhat arcane for the general reader, and this problem turned out to make little difference when the numbers were revised. When the old (1972 base) index numbers were used, output was understated because the decline in computer prices was not captured. Offsetting this, however, was the fact that, by retaining the 1972 base year too long, the contribution of computers and other fast-growing industries to overall productivity growth was dramatically overstated. This is because a base-weighted index overstates the output share of products whose relative prices are falling. Improvements in the computer price index and the shift to a 1982 base year were both long overdue. However, when the changes were finally made the revised output data continued to show a dramatic productivity slowdown.

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Erratum: In Eliot Marshall's article "End game for the N Reactor?" (News & Comment, 2 Jan., p. 17), the environmental group demanding an impact statement on the plant's restart is the Natural Resources Defense Council. The present manager of the N Reactor is UNC Nuclear Industries, Inc., not Rockwell International, which runs the PUREX plant at the same site. Both companies will be replaced by Westinghouse this year.