Letters

Mature Accelerators

Mark Crawford's article "Racing after the Z particle" (News & Comment, 26 Aug., p. 1031) caught my attention and deserves comment. The author contrasts Europe's LEP (large electron-position) accelerator with the Stanford Linear Accelerator Center's SLC (Stanford Linear Collider) and uses the following zinger: "Unlike SLC, Europe's LEP is a classic synchrotron storage ring built with new hardware and it is unlikely to encounter the kinds of delays that have afflicted researchers at Stanford." For your readers' information LEP is based on a series of machines that inject into it, the oldest of which is the PS, a 25-billion-electron-volt (GeV) machine constructed between 1956 and 1960 and delivering beam to their SPS, the 400-GeV machine (operating for protons) completed in 1976, which in turn injects into the "new hardware." An essential difference between the European way and the U.S. way is that Europe supports their machines in a style to which we would love to become accustomed. There is nothing wrong with mature accelerators if they are given the "TLC" they require. Like fine wine they may even work better. Examples of still productive and very reliable workhorses that are 20 years old and older are the Brookhaven AGS (1959), the Cornell Collider Injector (1965), the DESY (Hamburg) accelerators, and the circa-1970 Fermilab machines injecting into the new TEVATRON. Since Fermilab is considering whether or not to use its machines as injector to the (when-and-if) Superconducting Super Collider, all of this is intensively relevant.

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The Global Carbon Cycle

I applaud the attempt by R. P. Detwiler and Charles A. S. Hall (Articles, 1 Jan., p. 42) to reduce the uncertainty in estimates of the release of CO_2 from terrestrial ecosystems to the atmosphere; however, I do not find evidence that they have done so. Nor do I find that they have used data not already used in an almost identical analysis published in 1985 (*1*). Furthermore, even if the terms of the global carbon budget appear to balance in 1980, large discrepancies at other dates still remain.

Detwiler and Hall say their estimates of flux are lower than many previous ones because they included ecosystem recovery processes and because they used lower estimates of tropical forest biomass and slightly lower estimates of rates of land clearing than earlier studies. The implication of the first reason they give is that previous studies did not include recovery processes. In fact, every analysis cited by Detwiler and Hall did include them, although only the two more recent analyses cited (*1, 2*) included the recovery processes of shifting cultivation.

These two recent analyses (*1, 2*) also used the same sources of data for estimates of biomass (*3*) and clearing rates (*1, 4*) that Detwiler and Hall used and were almost identical in other aspects as well. It is, therefore, puzzling that the flux estimates of Detwiler and Hall are lower than those of Detwiler *et al.* (*2*) and Houghton et al. (*1*). A full documentation of the reasons for the difference would require detailed comparison of data and models. On the other hand, the values of biomass Detwiler and Hall used are lower than those used by Houghton *et al.* (*1*) despite the fact that both studies used the same sources of data. The most likely explanation would appear to be the methods of aggregation used to calculate means for various world regions or vegetation types. Thus, while the means used by Detwiler and Hall define one range of uncertainty in the estimated release of CO_2 from terrestrial ecosystems, the authors provide no evidence that the higher values obtained by Houghton *et al.* are less likely. The new range they report is a low subset of the possible range.

Another aspect of the analysis by Detwiler and Hall that deserves comment is their implication that if the accumulations and releases of carbon in various reservoirs can be made to balance in 1980, the global budget is balanced. On the contrary, several authors have pointed out the importance of past releases of carbon or past atmospheric CO_2 concentrations to the current balance of the carbon cycle (*5, 6*). For example, one reason why deconvolutions of ratios of ^{13}C to ^{12}C in tree rings gave a positive biotic release in 1980 (table 3 of Detwiler and Hall) is because the calculated release has been decreasing over the last decades. There is no evidence from records of land use that the release of carbon from terrestrial ecosystems, globally, was larger earlier in the 20th century than in 1980 (*7*). The rate of deforestation worldwide is greater now than it has ever been in the recorded past. Despite the conclusions offered by Detwiler and Hall, estimates of the biotic flux based on land-use data continue to remain incompatible with those based on past atmospheric CO_2 concentrations as measured in air bubbles trapped in glacial ice (*6, 8*).

Neither the statement by Detwiler and Hall that the global carbon cycle may now be balanced nor the statement that the range of the biotic release has been reduced is justified. On the contrary, because of the lower estimate they report, the range for the net flux in 1980 appears to have been increased from a new low of 0.4×10^{15} grams of carbon to the previously calculated high of 2.5×10^{15} grams of carbon (*1*). Even this range includes the releases of carbon only from the outright clearing of forests and not from degradation going on within tropical forests (*4*). Thus, the high estimate of 1.6×10^{15} grams given by Detwiler and Hall may underestimate the net flux of carbon due to changes in land use in the tropics by 1×10^{15} grams of carbon or more.

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The suggestion by Detwiler and Hall that the global carbon budget can be "balanced" on the basis of estimates of rates of deforestation alone is based on the assumption that there is no other net change under way in biotically controlled reservoirs of carbon, such as forests and soils. These pools are two to three times the amount in the atmosphere. They are maintained by gross fluxes of 90 or more gigatons (GT) of carbon annually through gross photosynthesis and total respiration (*1*). Small changes in these fluxes in the range of 1 to 2% would affect the carbon "balance" appreciably. It is reasonable to assume that such changes are under way. The approximately 0.5°C warming of the earth over the past century can be assumed to have increased rates of respiration of soils in high latitudes by 5 to 15%, possibly more. Whether there is a compensating stimulation of gross photosynthesis globally is not known, as discussed by Detwiler and Hall, but there is no obvious mechanism of equal magnitude. These considerations emphasize that the total release of carbon into the atmosphere globally is not known and that current attempts to "balance the equation" mean little (*2*).

Knowledge of details of the global carbon cycle is important. Uncertainty as to those details, however, does not prevent steps toward stabilization of the greenhouse gas content of the atmosphere. Such steps will be necessary and will yield valuable new information about the rate of absorption of carbon into the oceans. Whatever the current total release of CO_2 into the atmosphere, the current imbalance in the atmosphere is about 3 GT of carbon per year, an amount that produces a net annual increase in the atmosphere of about 1.5 parts per million by volume. Removal of 3.0 GT of carbon from current releases would stabilize the atmospheric burden of CO_2 instantaneously, if temporarily. Further adjustments in the releases would be appropriate in successive years in response to the new information that would be available about the rate of absorption of CO_2 by the oceans.

The removal of 3.0 GT of carbon from current releases is possible without an extraordinary disruption of current activities, although it would require an unusual global effort. The current release from fossil fuels is more than 5.0 GT. That from deforestation is commonly thought to be in the range of 1 to 3 GT. Reforestation of about 2×10^6 square kilometers will store about 1 GT annually for each year for 40 to 50 years. Other possibilities exist, such as the burial of CO_2 in solid form in the oceanic depths. These possibilities have been explored recently in detail in conferences that also examined, but rejected, the possibility of allowing the imbalance to continue and the earth to warm (*3*).

Steps to reduce emissions are possible. They are appropriate now. Effective steps hinge heavily on a clarified understanding of the contribution of deforestation to the atmospheric burden of CO_2. Detwiler and Hall raise an important topic.

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Detwiler and Hall conclude their article "Tropical forests and the global carbon cycle" with the observation that, "Human populations and economic growth and their accompanying requirements for land, timber, and other resources almost inevitably mean more forests will be cleared. The net carbon released by these activities is an important component of the global carbon cycle and, as a consequence, of possible future climate changes."

This prognosis, while in itself sensible, does not take note of some potentially compensating factors. For example, few would dispute the proposition that while the area of tropical forests is declining, the area of temperate climate forests is remaining constant, if not expanding modestly (*1*). Furthermore, most global climate models, although they tend to disagree considerably with respect to regional climate changes that may follow a global greenhouse warming, do generally agree that temperature increases will be particularly pronounced at the higher latitudes, at least in winter (*2*). The aggregate effects of a global warming on forest area and biomass are conjectural and cannot be known with certainty. In many regions, higher temperatures could increase net photosynthesis, thereby increasing forest growth. However, higher temperatures in warmer climate zones could increase respiration rates, thereby reducing net photosynthesis and the fixation of carbon. Depending on how the lottery of temperature and precipitation change plays out in the world's forested regions, plausible outcomes of the greenhouse effect could be either an increase or decrease in the land area and the biomass of the world's forests. If "fertilization effects" associated with higher ambient levels of CO_2 occur in the open air as they do in controlled environment facilities, the propects of an increase in forest-generated carbon fixation are heightened.

Another reason to think that an increase in forest area is possible stems from evidence showing that real forests can advance quite rapidly in response to small increases in temperature at their current altitudinal and poleward latitudinal limits. Shugart *et al.* (*1*), on the basis of the migration of European fossil pollen taxa, show (table 10.1, p. 484) rates of migration of some 22 species to range from about 50 to 2000 meters per year. The high-latitude regions, many now occupied by tundra, may also be particularly easily invaded by forests in the sense that competing human uses would be minimal. On the other hand, Shugart *et al.* also conclude that large reductions in the aerial extent of boreal forests could accompany a poleward shift in their boundaries. This apparent inconsistency stems from the possibility that larger areas would be lost at the southern margins to competition with other tree and nontree species.

Although the boreal forest of the Northern Hemisphere is likely to suffer long-term decline at its southern limits due to the invasion of other plant communities, much of this decline could be replaced by cool, moist, temperate forests such as those that currently inhabit much of New England, the Great Lakes states, and eastern Canada. While this speculation is at variance with the modeling of Emanuel *et al.* (*3*), that study ignored the increases in mean global precipitation levels that likely would accompany a global warming.

A recent study by the International Institute for Applied Systems Analysis (IIASA) (*4*) estimated that the effect of global warming alone, independent of any "fertilization" effects, could be a 75 to 100% increase in the growth rate of the northern boreal forests. The same study speculated that the warming could increase the total area of boreal forest by about two-thirds and that the total inventory of all conifer forests, roughly one-half of the world's forest biomass, could increase by 29% by the year 2030.

Woodwell (*5*) has calculated that 2×10^6 to 4×10^6 square kilometers of new rapidly developing forest would be enough to withdraw the annual increment of approximately 1×10^{15} to 2×10^{15} grams of carbon now accumulating in the atmosphere at current rates of emission. This capability would continue for, say, 40 years or until the forest reaches maturity. Calculations based on Cooper's model (*6*) and other sources suggest that an area two or more times as large may actually by required to fix this much carbon. The IIASA figure for the expanded global area of boreal forest as the result of increased warming is approximately 6×10^6 square kilometers, which is at least in the order of magnitude of the other estimates of new forest area required to stabilize atmospheric CO_2 concentration.

While none of the estimates can be viewed as definitive, the forces working on the global climate system can surely operate in more than a single direction, and the effects of compensating factors may be significant. One cannot rule out the possibility that at least the first increments of the anticipated global warming could result in some degree of expansion of the earth's forest into regions not now forested. New forests, whether planted or inadvertent, might provide some mitigation of the anticipated buildup of CO_2 in the global atmosphere.

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Response: Houghton and Woodwell write that we made several erroneous assertions and reached conclusions that "mean little" and are not "justified." We believe their criticisms are unwarranted. We did not say in our article (*1*) that *all* previous assessments of the CO_2 release from tropical forests had completely ignored forest recovery. But, as Houghton admits, "only the two more recent analyses cited . . . included the recovery processes of shifting cultivation." As we have stated repeatedly, distinguishing between shifting and permanent cultivation is crucial to an accurate determination of the net carbon release from tropical forests (*2, 4*). One reason the 1985 estimate by Houghton and his colleagues (*5*) is much lower than the ones they published in 1983 (*6, 7*) is that they had overestimated the percentage of permanent clearing in their earlier studies. Their later studies assigned some of that clearing to shifting cultivation (*5*, p. 620).

We did not state or intend to imply that balancing the carbon budget in 1980 allows one to ignore historical changes in the carbon cycle. In our article we did not attempt to balance the overall carbon budget for the last century, in large part because we concluded that if there was still so much residual uncertainty in 1980, any statement about the budget for earlier years would be speculation, as estimates of land use change and biomass for the years before 1980 are even less reliable. We do note that if Takahashi's estimate (see reference 66 in our article) of oceanic uptake for 1980 is correct, it suggests that the oceans also took up more CO_2 in the past. If the oceans have been a somewhat larger sink throughout the industrial revolution than previously believed, the carbon budget can accommodate larger releases of CO_2 during the same period.

Woodwell implies that our suggestion that the carbon budget can be balanced will discourage further study of, or attempts to control, tropical deforestation. Lest anyone else reach such a conclusion, we state again that we wholeheartedly support additional research on tropical forests and efforts to reduce their wanton destruction. Our conclusion that the carbon budget for 1980 might be in balance is not an endorsement of laissez-faire climate policy. As Woodwell points out, society can decide to alter the fluxes of carbon so as to achieve a different balance point.

The most serious criticisms made by Woodwell and Houghton concern the differences between our estimates of the carbon release from tropical forests and theirs. Houghton finds these differences "puzzling," as we use the "same sources of data for estimates of biomass . . . and clearing rates." In fact, the major reasons for the differences between our estimates and theirs are (i) we used estimates of clearing rates not used by Houghton and his colleagues; (ii) we used different data to determine the carbon released from soils; (iii) we used different data for simulating the fate of cleared vegetation; and (iv) we used the biomass data reported by Brown and Lugo (*8, 9*) differently from the way they did. The fourth reason is by far the most important.

Our three lowest estimates of the carbon release from tropical forests in 1980 [0.42 to 0.67 gigaton (GT) per year] (*1*, table 2) are the results of simulations that use estimates of clearing derived from the work of Seiler and Crutzen (*10*) and from work by the Food and Agriculture Organization (FAO) of the United Nations (*11*). The FAO estimate is, as described in our article, the sum of individual simulations for 76 tropical countries. These three estimates establish the lower end of our range. Houghton and his colleagues (*5*) did not use the data of Seiler and Crutzen and did not use the FAO data to simulate each country individually. The FAO data for many countries revealed that much of the clearing in 1980 took place in forest types with relatively less biomass than those in the data used by Houghton and his co-workers.

The data we used to estimate the release from tropical soils come from work by one of us (R.P.D.) (*2*). These values result in a much smaller release from soils than the values used by Houghton and his colleagues (*5*, p. 618). We believe that the estimates we used are more appropriate for the reasons set out in detail in (*2*, pp. 69–78): land use change in the tropics generally does not reduce soil organic matter below about 40 centimeters of depth, and it reduces organic matter above 40 centimeters less than had been previously assumed.

The parameters we used to simulate the fate of cleared vegetation are derived from an admittedly limited amount of information (*1*, p. 44; *4*, pp. 341–342). Houghton and his colleagues assigned cleared vegetation to three pools that decay at rates of 100%, 10%, or 1% per year (*5*, p. 619; *6*, p. 239). They do not state the fractions of cleared vegetation they assigned to each pool in their 1985 study (*5*), but if they used the same fractions reported in an earlier article (*6*), their assumptions about the fate of cleared vegetation would result in a faster and somewhat larger release, all other things being equal, than ours would. We set out our reasons for using the parameters we did in an earlier article (*4*, pp. 341–342) and believe that they are at least as appropriate as those used by Houghton and his colleagues. This is probably the least important reason for the differences in our respective estimates of the release from tropical forests.

The 1985 analysis by Houghton and his colleagues (*5*) and ours (*1*) both rely on two articles by Brown and Lugo (*8, 9*) for estimates of tropical forest biomass. But as Houghton and Woodwell point out, common sources did not result in identical estimates. We could not reproduce the biomass estimates used in their analysis from the information found in the work of Brown and Lugo. We asked Brown for assistance. Her reply accompanies this response.

Houghton and Woodwell also argue that degradation of tropical forests is unaccounted for in our estimates. Our simulation based on the FAO data explicitly includes carbon released due to degradation, as the FAO reports contain estimates of the rate of forest degradation in many countries. As to our other simulations, the assertion concerning degradation by Houghton and Woodwell is true only to the extent that degraded forests are not subsequently cleared. As our higher estimates of forest biomass represent the carbon content of relatively undisturbed forests (*8*, p. 163), the carbon released upon clearing in these simulations equals the amount that would have been released even if degradation had reduced their biomass somewhat before they were completely cleared. Of course, the timing of the release differs, but not to a degree that is significant in a global view of the carbon budget.

These four differences in methodology and data account for the discrepancies between our estimates of the carbon released by tropical forest clearing and those of Houghton and his colleagues. It would be foolish to argue that there is no uncertainty in our estimates. We pointed out many of these uncertainties in the article (*1*, pp. 44–45). Yet there are valid reasons for the differences between our estimates and theirs,