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7. I am indebted to Norman Rosenberg for assistance with aspects of the discussion on climate.

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, and we stand by our analysis and our conclusion that it has reduced the likely range of the CO_2 release from tropical forests. Over the past decade, our research has confirmed our preliminary estimate that the destruction of tropical forests released probably less than 2 GT of carbon in 1980 (*2, 3, 12*). During this period, the estimates by Woodwell, Houghton, and their colleagues of the release from the tropics in 1980 have decreased from 1 to 7 GT to 0.9 to 2.5 GT (*5–7, 13*). Both groups have contributed to reducing the uncertainty. Nevertheless, it appears that part of the reduction in their estimate results from two ideas we incorporated into our work first: the importance of distinguishing between temporary and permanent clearing and the likelihood that early estimates of tropical forest biomass were too high. The second idea was a consequence of our collaboration with Brown and Lugo, whose work on tropical forest biomass has reduced much of the uncertainty about the size and extent of tropical forests.

The issues concerning temperate and boreal forests that Sedjo has raised are interesting. As our focus was the role of tropical forests in the carbon cycle, we did not address these issues in our article beyond citing several studies that discussed them (*1*, references 18, 19, and 53). Predicting the consequences of climate change is even more risky than trying to balance the carbon budget, but also, perhaps, more important.

Finally, our original conclusion (*1*, p. 46) bears repeating:

> Thus, there is some possibility, *how large we cannot say*, that the global carbon budget can be balanced without postulating another sink *if* the actual oceanic uptake is closer to Takahashi's estimate than to those of the other geochemists. If the other geochemists are correct, however, we must find a sink that can accommodate not only 0.1 to 1.1 GT of fossil-fuel carbon in 1980 but also 0.3 to 1.7 GT of carbon from forests [emphasis added].

This, to our minds, is neither an assertion that the carbon budget *is* balanced, nor an argument for accepting continued uncertainty, forest destruction, or increasing levels of atmospheric CO_2.

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Being the lead author of the two most frequently cited papers (*1, 2*) on the topic of biomass of tropical forests, I would like to add my comments to those of Houghton and Woodwell and the response by Detwiler and Hall.

A major point of discussion between Detwiler and Hall and Houghton rests on the values of tropical forest biomass based on direct sampling. The source of data used for these estimates is (*1*). In this paper we grouped biomass of tropical forests into six types on the basis of the life zone system that relates to climate but not to geographical region. Houghton and his colleagues (*3*) regrouped these data into nine types on the basis of climate and continent. Because I am familiar with the data base, I was able to divide the data up into the same groups as those of Houghton *et al.*, and I obtained the following results for the carbon contents of tropical forests, in tons of carbon per hectare (using 0.45 to convert biomass to carbon, as does Houghton).

Forest type	America	Africa	Asia
Moist	155	187	160
Seasonal	none	178	105
Dry	27	63	27

Houghton *et al.* (*3*) obtained the following results from the same data base.

Forest type	America	Africa	Asia
Moist	176	210	250
Seasonal	158	160	150
Dry	27	90	60

It is clear that we do not obtain the same results. I report no data for seasonal forests in America, and the mean values for moist forests in all three areas that I obtain are more like those Houghton *et al.* used for seasonal forests. I have not been able to obtain any of the high values that Houghton *et al.* reported. In fact the highest value that I reported in (*1*) was 242 tons of carbon per hectare for a moist forest in Africa.

I believe that values of the carbon content of tropical forests used by Detwiler and Hall in their model are more defensible than those of Houghton *et al.* because Detwiler and Hall conferred with me many times to ensure that they had interpreted the data correctly. Thus it is not surprising that Houghton *et al.* and Detwiler and Hall disagree on the upper end of the range of the carbon flux to the atmosphere from tropical deforestation. Use of the high but unsubstantiated numbers given above by Houghton *et al.* in their models would account for most of the discrepancies between the two groups.

Of more importance to resolving the role of tropical forests in the global carbon cycle is the need for accurate and precise data on the carbon content of the forests actually being cleared. New approaches to this problem are now being initiated by Hall, Houghton, Woodwell, and me, working as a group, and it is hoped that significant progress will be made in resolving these issues on tropical forest biomass.

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Cataract Removal

Robert Pool, in his article "Trapping with optical tweezers" (Research News, 26 Aug., p. 1042) summarizes some of the uses of lasers. However, lasers are not used to "burn off cataracts in eye surgery." This is a common misconception of the lay public. Lasers can be used to create a posterior capsulotomy after cataract surgery. However, they cannot be used to remove a cataract, which is a significant chunk of tissue.

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