**De acuerdo con el principio de Interciencia de alentar la discusión libre de opiniones e ideas, dentro de un tono de altura, nuestras páginas están abiertas a las personas e instituciones que deseen expresar puntos de vista aunque no necesariamente coincidan con los que se publican en la revista.**

El Editor

# /CARTAS AL EDITOR/
# /LETTERS TO THE EDITOR/
# /CARTAS AO EDITOR/

## BRAZIL'S AMAZON FOREST AND THE GLOBAL CARBON PROBLEM

The global carbon cycle is a subject of intensive research because current understanding of the cycle is incomplete and it affects our capacity to anticipate the consequences of human impacts. The article by Fearnside in *Interciencia* 10(4): 179-186, 1985 deserves comment because it clouds rather than clarifies important questions about the role of tropical forests (and the Amazon region) in the global carbon cycle.

The article is an extensive review of old literature (60% of citations are from 1980 or earlier in a field that is moving at a torrid pace) based mostly on unsubstantiated opinion, linear extrapolation of complex non-linear phenomena (p. 181), and fails to add new ideas to the issue. Instead, the article is based on a contrived scenario (the complete conversion of the Amazonian forest to agriculture or pasture over an unspecified but assumed to be imminent time period) presented to illustrate how such an event "adds to the substantial list of probable negative biological and human impacts from large scale deforestation" (p. 184).

The following is a list of factual mistakes:

1 — On pages 180 and 184 the author uses 60.09 Gt as the *aboveground* biomass of Amazonia. Table I, which substantiates this value, reports that value as *total* (above and belowground) biomass (a 24% error).

2 — The Table itself has problems that magnify the biomass of Amazonia (a critical point of contention in the simulation of carbon models). For example, for mangroves the value for the riverine mangroves of Panama (the largest mangrove biomass reported for this hemisphere) is extrapolated to all the region's mangroves; biomass values reported by Seiler and Crutzen are used extensively, unfortunately, these authors did not measure biomass, they quoted Whittaker and Likens whose values have been shown to be high (Brown and Lugo 1982, 1984) and no longer used by those working in the global carbon problem; ignores the many life zones in the region even though life zones have been shown to have discrete carbon storage; and assumes that all the region's forests have the high biomass values reported in the Table when in fact the extensive volume data for the region shows the opposite (Brown and Lugo, 1984). This Table may have anywhere from 50 to 100% error in its biomass estimate.

3 — The area of secondary forest is assumed to be small in the region (p. 181) in spite of the report of Lanly (1982) to the contrary.

4 — Soils are assumed to lose carbon irreversibly once a forest is converted to pasture. Our extensive studies for the U.S. Department of Energy's Carbon Dioxide Program show the opposite, i.e., pasture soils accumulate carbon and loss of soil carbon after conversion occurs for a short time interval (decades) under intensive agricultural use of the land.

5 — Currently accepted rates of carbon release by changes in land use in the tropics are lower than quoted on p. 184. Loucks for example, completely revised his estimate in recent publications and so has Woodwell *et al.*

The following assumptions in the article show a bias to the preconceived idea that the global cycle will be affected by the contrived scenario of destruction.

1 — It is assumed that there will be little recovery of forest after its conversion to pasture. If the recovery occurs, it would be to 50% of original biomass. No data are presented to substantiate these assumptions, nor is the reader informed of what amount of area in the Amazon may show recovery after the forest is cut.

2 — It is assumed that mature natural forests have no role in the global carbon cycle (a verbatum repetition of assumptions commonly used in carbon models) but no data or arguments are given to substantiate the assumption. If the so called primary forest was to have a small carbon accumulation (25 g carbon/$m^2$ yr), the global carbon cycle would balance. This illustrates how precarious these assumptions are.

3 — "Delayed effects" will eventually cause *all* carbon in the Amazon vegetation to become airborne. Again, no new data are given. This assumption basically says that all vegetation in the Amazun (4.8 million $km^2$) will be converted to carbon dioxide and not replaced. Is this possible?

The tendency in the article is to eliminate all possibility of any carbon sink to operate in the Amazon region while maximizing the effect of carbon sources. When sinks are mentioned, their effect is never incorporated in the calculation of the total effect of the annihilation of the Amazon Basin. Uncertainties in the analysis are termed "small" (p. 181) and this is highlighted by Journal editors.

This article is clearly alarmist and while it offers no new information, it accomplishes two things: 1 — it does not improve our understanding of the role of tropical forests in the carbon cycle prob-

lem and 2 — it confuses the issue through misinformation. It is unfortunate, for example, that the author never lets the reader know over what period of time the Amazon Basin will ejaculate 60 Gt of carbon to the atmosphere. Without this important piece of information it is impossible to make a serious evaluation of the global role of the region (for example, humans currently add about 6 Gt/ from fossil fuel combustion). The author does imply on p. 184 that his earlier article in *Interciencia* 7(2): 82-88 may provide this critical time interval. We call the attention of *Interciencia* readers to our commentary on this article in *Interciencia* 7(6): 361-362.

On page 182 the author mentions the many "academic controversies" surrounding the points discussed in the article. He is correct. However, we must add that academicians and scientists in general owe the public and the rest of the scientific community their best effort to avoid extending controversies through bias and strawmanship. We believe that articles like this one set science back in its quest to resolve the human problems in the tropics.

LITERATURE CITED

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*EDITOR'S NOTE*

*Following* Interciencia's editorial policies, *Fearnside's manuscript was duly refereed. Totally opposed appraisals and recommendations were solved through the additional opinion of a renowned expert, who recommended its publication. It appeared in Vol. 10 Nº 4. The Editors have judged as interesting and illustrative of the journal's attitude to publish the comments submitted thereafter by Lugo and Brown, together with a rebuttal by the author of the article.*

## BRAZIL'S AMAZON FOREST AND THE GLOBAL CARBON PROBLEM: REPLY TO LUGO AND BROWN

Lugo and Brown (1986) label me as an "alarmist" who engages in "bias and strawmanship" in order to argue to a preconceived conclusion in my paper "Brazil's Amazon Forest and the Global Carbon Problem" (Fearnside, 1985a). Their remarks illustrate a number of the logical fallacies and factual errors that abound in the global carbon debate (as well as some new errors that they have inaugurated here). I welcome the opportunity to respond to their comments.

The possibility that a large part of Brazil's Amazon region might be converted to cattle pasture is scarcely a "contrived scenario." Precisely this transformation is now happening very quickly (Fearnside, 1983). Making calculations of what environmental impacts would ensue from a hypothetical complete conversion is entirely justified as a means of providing decision-makers with the information necessary for them to judge whether taking action to contain deforestation would be worth the substantial financial and political costs of achieving that goal.

Lugo and Brown attempt to dismiss my paper as a "review of old literature" based on 60% of the citations being from 1980 or earlier. If *not* citing articles more than four years old is a new standard by which scholarship is judged, it is one of which I readily confess to be unaware. I would suggest that a better approach might be to see if my paper failed to cite any significant contributions, old or new. One indication that my coverage of the field was reasonably thorough is Lugo and Brown's failure to provide citations for any such omissions. The only work cited by Lugo and Brown that is not cited in my paper is Lanly's (1982) world-wide compilation for F.A.O. of official statistics on forest areas, which would have been inappropriate to use in lieu of original sources from Brazil.

Lugo and Brown's preoccupatioo with citation dates may stem from disappointment that their own recent estimate of forest biomass (Brown and Lugo, 1984) was not used as the basis for my calculations. As explained in my paper (p. 182), the Brown and Lugo estimate was not used because there is reason to believe that the values presented in that paper seriously underestimated forest biomass. I will return to this in discussing their numbered criticisms of my paper.

I owe Brown and Lugo and apology for the serious mangling of the citation to their 1984 work as it appears in my paper's bibliography. In place of the first line of the Brown and Lugo (1984) citation the typesetter duplicated the first line of the citation below it, so it appeared listed as "Buschbacker, 1983." I failed to discover the substitution on the galley proofs, and the editors subsequently amended the two "Buschbacker" listings to "1983a" and "1983b."

Now to the first series of numbered objeetions raised by Lugo and Brown:

1. Lugo and Brown point out an inconsistency between the table and the text with regard to aboveground and total biomass. The second of the two references to "above ground" biomass on page 180, and the tree references on page 184 are indeed incorrect, and should be changed to read "total" biomass. The table is correct, as are the calculations with the exception of the following modification (which increases rather than decreases the amount of carbon ultimately released). On page 180 the fraction converted to charcoal is incorrectly applied to the total biomass (60.09 G tons), rather than to the smaller above ground value (45.41 G tons). The amount of carbon stored as charcoal is thereby exaggerated, and the long term impact of deforestation on carbon release to the atmosphere un-