Brazil's Amazon forest and the global carbon problem: Reply to Lugo and Brown

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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 Amazon (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 problem 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 preoccupation 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 objections 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-

derstated by 3.2%. The two references to 54.69 G tons on page 180 should be changed to 56.44 G tons.

2. Lugo and Brown claim that the table in my paper contains errors that could make its biomass calculation off by 50-100%. However, the points they raise (even if they should all prove valid) could not produce an error of this magnitude. The alleged errors:

a.) Lugo and Brown say that I have exaggerated mangrove biomass, but suggest no alternative value. As noted in the table, a mangrove biomass value from Panama was used; no estimates are available for Amazonia. However, the very small area of mangroves in the Brazilian Amazon means that any overstatement of the biomass per hectare would have minimal effect on the carbon store of the Legal Amazon. The 1000 km^2 of mangroves represent only 0.02% of the area of Brazil's Legal Amazon; even if my purportedly inflated value of 0.025 G tons C in mangroves were cut by half the total change would be a mere 0.021%!

b.) Biomass values from Seiler and Crutzen (1980), derived from Whittaker and Likens (1973), are alleged to be high. Lugo and Brown assert that values from this source are "used extensively" in my table, but these supposedly exaggerated values are only used for three low-biomass forest types whose combined carbon store is 9.24 G tons, or only 15.4% of the total. The 765 m tons ha^{-1} biomass value of Whittaker and Likens for "tropical rainforest" (not used in my paper) is generally conceded to be high, but I do not know of similar criticisms of values for the lower-biomass types. However, should the values in question prove to be double the real biomass for these forest types the result for the Legal Amazon would be altered by only 7.7%. If the biomass values for these forest types are high by some more probable (lower) factor, the impact on the result for Amazonia would be still less.

c.) My table "ignores the many life zones in the region." Undoubtedly the accuracy of the estimate could be increased by using a more refined vegetation classification than the seven categories used in the table, which were based on Braga's (1979) review of Amazonian vegetation types. The paucity of biomass measurements would, for the present, hinder use of a scheme involving many finer categories. One would hope that data will become available in the future for such improved estimates. I see no basis for Lugo and Brown's intimation that using a more refined classification scheme would bolster their case for radically lower carbon stores in Amazonia.

TABLE I

BIOMASS ESTIMATES IN UPLAND DENSE FOREST IN BRAZILIAN AMAZONIA (a)

Above Ground	Total	Location	Reference
247.84	(355.9) (b)	Tucuruí	Cardenas *et al.*, 1982
255.60	(367.1) (b)	Manaus	Klinge and Rodrigues, 1974 (c)
353.4	507.5	Manaus	Klinge, *et al.*, 1975
	354	Jari	Jordan and Russell, 1983 (d)
	155.1	"Tropical American undisturbed productive broadleafed forests"	Brown and Lugo, 1984

(a) Metric tons ha^{-1} dry weight.

(b) Estimated using ratio of above ground to total biomass measured by Klinge *et al.* (1975).

(c) An extension of the direct measurement (Klinge *et al.*, 1975) to 5 nondestructive quadrat and transect forest surveys in the Manaus area (see Fearnside, 1985a: Table I note d). Klinge (personal communication, 1985) now believes that the higher value based solely on direct measurement is the more trustworthy of the two. My having used the lower value therefore biases the outcome toward lesser impact of deforestation.

(d) "Wood" biomass only.

d.) The results in the table do not agree with Brown and Lugo's (1984) biomass estimate based forestry inventories. Indeed the results do not agree. Brown and Lugo's (1984) estimate, based on timber volume inventories, results in a biomass value of only 155.1 m tons ha^{-1} for "tropical American closed undisturbed productive broadleaf forests." Anyone who has actually weighed biomass directly in the Brazilian Amazon has arrived at values more than double this figure (Table I). Timber volume inventories are subject to error because they measure only large trees — above a minimum of 25 cm diameter at breast height (DBH) in the data base used by Brown and Lugo (1984: Fig. 1 caption). Brown and Lugo used a factor of 1.6 to correct the biomass of boles $\geq$ 10 cm DBH to total biomass and a factor of 1.2 to convert "merchantable volume" for trees $\geq$ 25 cm DBH to an estimated value for bole biomass for trees $\geq$ 10 cm DBH. The understory is ignored, but this would affect the estimates by less than 2% (Brown and Lugo, 1984: 1291). While both correction factors appear reasonable, the result for "tropical American closed undisturbed broadleaf forests" is so much lower than values from direct measurements that closer scrutiny is necessary before accepting it as applying to the Brazilian Amazon. Brown and Lugo (1984) give no biomass value for the Brazilian Amazon or for any locality within it.

A rough calculation can be made of biomass in the Brazilian Amazon employing the methods and most of the data base used by Brown and Lugo. The four volumes of data published by F.A.O. that Brown and Lugo used in the Brazilian portion of their study are available at Manaus (Heinsdijk, 1957, 1958a, b, c). One volume used by Brown and Lugo that was not published by F.A.O. is not available at Manaus (Japiassu *et al*, 1974), and one additional F.A.O. volume not used by Brown and Lugo is available (Glerum, 1960). The results for the 16 localities surveyed in the volumes available at Manaus are presented in Table II. The mean estimate for total biomass is 226.1 m tons ha^{-1}. Using this as the biomass value for upland dense forest in my calculation for the Brazilian Legal Amazon, the total carbon store is 41.42 G tons — a reduction of 31.1% from the value given in my paper.

In converting volumes to biomass values, Brown and Lugo applied a more sophisticated procedure than that used in deriving Table II. They selected a subsample of surveyed hectares within which they computed the mass of individual trees by classing them into species groups and applying a mean wood density for each group. Pires (1978: 613), a botanist who was resident in Belém during the period when the F.A.O. surveys were carried out, has strongly criticized the survey for the methods used in tree identification, which he states can produce error rates as high as 90%. Therefore the more refined density computations used by Brown and Lugo (1984) may well not have produced a result any more accurate than that derived in Table II by applying Brown and Lugo's (1984: 1291) 0.62 mean wood density value for Tropical America directly to the volume figures appearing in the F.A.O. reports, and may well be less reliable for having used only a subsample rather than the full F.A.O. data set.

TABLE II

BIOMASS FROM FOREST VOLUME SURVEYS IN BRAZILIAN AMAZONIA

Locality	Volume (a) (m^3 ha^{-1})	Biomass (b) (m tons ha^{-1})	Reference (c)
Santarém (Slope or "Flanco" forest)	135	160.7	Vol. 1, p. 113
Santarém (plateau or "planalto" forest)	223	265.5	Vol. 1, p. 113
Amapá	162	192.8	Vol. 1, p. 113
West of Portel	314	373.8	Vol. 1, p. 113
Caxuana	271	322.6	Vol. 2, p. 35
Portel	228	271.4	Vol. 2, p. 35
Cametá Oeste	192	228.6	Vol. 2, p. 35
Rio Aripiuns	146	173.8	Vol. 3, p. 13
Maués	169	201.2	Vol. 3, p. 82
Canumã (Canhuma)	164	195.2	Vol. 3, p. 82
South of Belém	210	250.0	Vol. 4, p. 35
Acará	217	258.3	Vol. 4, p. 35
Rio Capim	194	230.9	Vol. 4, p. 35
Piriá	161	191.7	Vol. 5, p. 1
Gurupi	131	155.9	Vol. 5, p. 1
Maracassumé	122	145.2	Vol. 5, p. 1
	$\bar{X}$ = 189.9	226.1	
	SD = 52.6	62.6	
	n = 16	16	

(a) Volume over bark for free boles (stump to first main branch or to 7 cm diameter) of all living trees $\geqq$ 25 cm DBH as reported in F.A.O. surveys.

(b) Biomass calculated from volume using 0.62 average wood density, 1.2 to correct for trees between 10 and 25 cm DBH, and 1.6 to convert bole biomass to total biomass for trees $\leqq$ 10 cm DBH (see text).

(c) Vol. 1 = Heinsdijk, 1957; Vol. 2 = Heinsdijk, 1958c; Vol. 3 = Heinsdijk, 1958a; Vol. 4 = Heinsdijk, 1958b; Vol. 5 = Glerum, 1960.

Brown and Lugo's (1984) value of 155.1 m tons ha^{-1} is lower than the volume-derived estimates for all but one of the 16 localities in Brazilian Amazonia presented in Table II, making it highly unlikely that a value this low applies to the Brazilian Amazon. Brown and Lugo used areas of forest types derived from maps of meteorological data, which is an improvement over the simple mean of sampled localities given in Table II. It is difficult to imagine, however, that correction for forest type areas would result in a difference of this magnitude. The inclusion of areas outside of the Brazilian Amazon in Brown and Lugo's estimate is a more likely explanation. In any case, even if their Tropical America value were used in place of my 361.5 m tons ha^{-1} for upland dense forests, the carbon release from the Legal Amazon would be 31.64 G tons (a 47.3% reduction). As pointed out in my paper, "climatically significant amounts of carbon would be released by clearing the region's forests, even if the much lower timber-volume-based value were to prove correct" (p. 182).

Where, then, do we stand with respect to a best estimate for dense forest biomass in the Brazilian Amazon? High variance in biomass over short distances means that reliance on a few high-quality estimates from destructive sampling risks error from inadequate coverage of the region. Despite the approximations involved in using volume data from forestry surveys of large trees, the use of these data sets to estimate biomass (pioneered by Brown and Lugo) is a promising approach. Since the localities of the estimates in Tables I and II do not overlap, probably the best available estimate at present would be a mean for the 16 localities from combining the two tables (using Klinge *et al.*, 1975 for the Manaus value in Table I). The resulting 250.8 m tons ha^{-1} mean for dense forest biomass represents 34.57 G tons of carbon (using 0.45 for carbon content), and would bring the total carbon load for the Brazilian Amazon to 44.83 G tons — a 25.4% reduction from the estimate given in my paper. Using Brown and Lugo's (1982, 1984) value of 0.50 for carbon content, the carbon total for the Brazilian Amazon would be 50.38 G tons, or 16.2% lower than the estimate in my paper.

Future improvements on this estimate are likely to result from the analysis (now in progress) of volume and destructive sampling data from the same location. Data of this type have been collected in an area near Manaus under study by INPA and World Wildlife Fund-US. Preliminary analysis of the destructive sampling portion of the study confirms the high biomass estimates of other studies in the Manaus area. The biomass data, combined with Judy Rankin's survey of over 30,000 trees $\geqq$ 10 cm DBH (all with botanical collections), should provide the key to improved interpretation of forestry surveys throughout the Amazon.

3.) Lugo and Brown imply that ignoring secondary forests significantly lowers the estimate, and suggest that Lanly's (1982) report indicated large areas of secondary vegetation. As stated in my table, the values apply to "natural" vegetation. Since a part of the region has already been converted to other vegetation forms, including secondary forest, carbon releases would be slightly lower for conversion to cattle pasture starting from present land uses. Reliable values for the area of secondary forest are difficult to obtain since only the youngest stands can be detected on LANDSAT satellite imagery (Fearnside, 1982). I do not know the basis of the official communications used as the information base for Lanly's (1982) report. His tabulations for "Tropical America" (lumped for 23 countries present forest areas for primary and secondary forests in "closed" and "open" formations (Lanly, 1982: 50). The areas of secondary forest reported correspond to 13.8% for closed forests and 22.1% for open forests; these values are higher than I would expect for the Brazilian Amazon. This may be due, in part, to the Brazilian Amazon being less densely occupied than the tropical forests in most of the other countries. Even if one accepts the percentages of secondary forest areas reported by Lanly for Tropical America as applying to Brazilian Amazonia, the effect on my estimate of carbon stocks is not great. Considering the *cerrado* as open and the remaining types as closed (with the exception of humid savanna, which is not forest), the total carbon stock would be lowered to 53.7 G tons (a decrease of 10.6%) if the average secondary forest is assumed to have 25% of the biomass of primary forest. Unfortunately, no data are available on the age or biomass distributions of secondary forests.

4.) Lugo and Brown's suggestion that soils converted to pastures gain rather than lose carbon is nothing short of incredible, considering the weight of evidence to the contrary. In addition to the data from Falesi's (1976) study used in my paper, other studies finding carbon decline under pasture in the Brazilian Amazon include Bennema (1975), Dantas (1979), Hecht (1983) and Sombroek (1966). Workers in other tropical countries have found similar trends (for a review of literature on pasture soils see Fearnside, 1980a; for effects by soil type see Buringh, 1984: 97).

One process would act to accumulate some carbon in soils under pastures on a time scale of centuries. This is the deposition of inert charcoal from repeated burning of the pasture or of secondary forests between intermittent use of the land as pasture. On the time scale of a few decades for which impacts are discussed in my paper, however, the amount of carbon deposited as charcoal would be minimal in comparison with the massive releases from removal of the forest.

5.) Lugo and Brown state that currently-accepted annual rates of carbon release from tropical deforestation are lower than those quoted (p. 184). They allude, without citation, to updates by Loucks and by Woodwell *et al.* I am not familiar with an update by Loucks. Woodwell *et al.* have lowered their estimates, but to my knowledge have not published a new figure for the impact of tropical deforestation, more recent values being given only for the entire terrestrial biosphere. They revised their earlier terrestrial biosphere estimate of 1.5 - 13 G tons (Woodwell *et al.*, 1978) to 1.8 - 4.7 G tons Woodwell *et al.*, 1983). I am happy to report that since publication of my article, Woodwell (1985) has given a revised estimate of the approximate contribution of tropical deforestation as 1 — 3 G tons year^{-1}, down from the Woodwell *et al* (1978) range of 1 - 7 tons year^{-1}. At the point in my paper where the contributions from deforestation are discussed (p. 181), it is the Woodwell *et al.* (1983) article that is cited, rather than the more dramatic releases that the same group reported in 1978.

The context in which I mentioned annual rates or carbon release from tropical deforestation made it inappropriate to include a digression to explain the Woodwell *et al.* (1983) revisions of values for the terrestrial biosphere. Although annual carbon release from tropical deforestation is not a value entering into my calculations of the impact converting the Legal Amazon to cattle pasture, I listed the published values for annual release in order to provide a scale for comparison with potential releases from accelerated clearing in Amazonia. The Woodwell *et al.* (1978) value of 1 - 7 G tons was clearly identified as a high estimate, and it's mention was immediately followed by descriptions of two types of criticism indicating that it is too high: Broecker *et al.* (1979) on ocean sinks and Seiler and Crutzen (1980) on charcoal sinks — hardly indication of a biased presentation in favor of high releases.

Now to Lugo and Brown's numbered points allegedly indicating my "bias":

1.) Lugo and Brown say that bias is indicated by assuming no recovery of forest after conversion to pasture or, if recovery occurs, 50% of original biomass. The modifying influence of forest regrowth in abandoned pasture is discussed in my paper on pages 180 and 181. The value of 50% was not presented as an expected value for biomass of secondary forest, but as an illustration of how even a recovery to this level would result in climatically significant carbon releases. While no survey exists of the ages or biomasses of secondary forests cut following abandonment of pasture, the cases I have observed near Altamira, Pará have been much lower than this figure.

The Lugo and Brown's emphasis on recovery of secondary forest is consistent with their publications arguing that such regeneration could largely negate the effects of deforestation (Brown, 1980; Brown and Lugo, 1982; Lugo and Brown, 1981, 1982). As explained in my article (p. 181 and note 4), the recovery rates for secondary forest in shifting cultivation (used in Lugo and Brown's arguments) are much more rapid than the recovery rates in degraded pastures. Since it is pasture that replaces the bulk of the forest now being cleared in the Brazilian Amazon (Fearnside, 1983), these arguments are misleading.

2.) Lugo and Brown state that bias is shown by my assumption that mature natural forests have no role in the carbon cycle, although my critics acknowledge that this assumption is "commonly used in carbon models." One might amend "commonly" to "universally." Lugo and Brown raise the possibility of forests accumulating carbon, and mention a hypothetical value of 25 g C/m^2/year. To my knowledge no evidence exists for mature forests growing indefinitely in biomass if they were, they would be very big by now. However, if the mature forests are, in fact, accumulating carbon, then removing those forests will have an even greater deleterious impact on the global carbon problem than the one proposed in my paper.

3.) Lugo and Brown say that I have assumed that delayed effects will eventually cause "all carbon in the Amazon vegetation to become airborne." However, the calculations in my paper are for conversion to cattle pasture (which contains some carbon), not a reduction to zero carbon in Amazonia.

I presented an update of my carbon calculations at a recent conference on biogeochemistry of Amazonia (Fearnside, 1985b). In it I replaced the pasture biomass value from Hecht's (1982) work at Paragominas, Pará (used in my paper) with a value from my work at Ouro Preto do Oeste, Rondônia. I consider the value from Rondônia to be more reliable because it includes monitoring of dry weight biomass over a full annual cycle at two sites. The average pasture biomass is significantly higher (10.67 m tons dry weight ha^{-1} as opposed to 0.95 m tons ha^{-1}), but the total carbon release declines by only 3.4% to 59.71 G tons.

Lugo and Brown's allegations of bias are surprising considering the pains that I took to use conservative values throughout the analysis. For example, had I been anxious to use only values that supported a case for high biomass, I could easily have used the Klinge *et al.*

1975 estimate of 507.5 m tons for biomass at Manaus, rather than diluting this with the lower Klinge and Rodrigues (1974) estimate (see Table I), or with the still lower Tucuruí estimate.

The carbonization factor is another area where a conservative value was chosen. As stated in my paper (p. 180), the 11.9% weighted carbonization factor derived from Goudriaan and Ketner (1984) used in the calculations was suspected to be high. Our measurements at INPA in a burn near Manaus have since confirmed this, yielding a value approximately one-third that of the one used. Impact of deforestation would therefore be greater than that shown by the calculations on page 180 in my paper.

Yet another assumption minimizing the impact reported is that of a fixed remaining or cumulative airborne fraction (p. 181), which means that each G ton of carbon released has less impact on global temperatures than would be the case were the calculations to include the expected increase in the fraction remaining from 66 $\pm$ 12% to a value over 80% (Keeling and Bacastow, 1977; see p. 181). Disruption of annual flux of carbon between the biosphere and the atmosphere (p. 182) is another area where impact could be greater than my calculations would indicate.

My calculation also minimizes the impact of deforestation on carbon releases by ignoring releases of soil carbon from below 20 cm depth (p. 182). Brown and Lugo (1982: 183) estimated carbon stocks to one meter depth based on 20 cm depth samples using the relationships that the top 20 cm contain 45% of the soil carbon in a one meter profile. Had I considered soil to one meter using Brown and Lugo's procedure, the soil contribution would have more than doubled to 4.35 G tons, raising the total release to 64.23 G tons (a 3.9% increase)

I also used a low value for the carbon content of forest biomass (0.45), while Brown and Lugo themselves have calculated a value of 0.51 (1982: 174) and used a value of 0.50 in their calculations (1982, 1984). Had I used 0.50 as the value for carbon content, the results would have been higher by 11.1% — with no danger of cries of "bias" from Lugo and Brown!

In addition to the many ways my article minimizes carbon releases, caution was exercised throughout in presenting the sometimes controversial ways in which the environmental impacts of these releases would be felt. For example, in discussing polar ice melting and sea level changes (pp 179-180) much more "alarmist" scenarios could easily have been chosen had I been bent on the sensationalism-at-all-costs implied by my critics.

Lugo and Brown say that I have given inadequate attention to carbon sinks in Amazonia. Two such sinks discussed earlier could absorb a small part of the carbon released: growth of secondary forest (pp. 180-181) and formation of charcoal (p. 181). A third factor that some have claimed could absorb carbon is the vegetation's response to higher levels of atmospheric CO_2. CO_2 "fertilization" would supposedly permit both enhanced growth of existing forests and the spread of forests into presently non-forested areas (Idso, 1984). Reasons to doubt that higher CO_2 levels would result in net increases in carbon uptake include the fact that forest growth is not limited by low CO_2 but rather by such factors as nutrients, water and sunlight, and that climate changes from deforestation (whether from CO_2 or other causes) would reduce tree growth (lower precipitation) and accelerate decomposition (higher temperature) (see review by Liss and Crane, 1983: 33).

A fourth sink is the erosion of some of the carbon in the soil and litter. Deposition of eroded material in marine sediments would indeed prevent part of the carbon from reaching the atmosphere. Richey *et al.* (1980: 1350) have studied organic carbon transport and oxidation in the Amazon River, and conclude that it discharges about 0.05 G tons year^{-1} into the ocean (0.1 G tons total of transported + oxidized carbon, with 50% of the total being oxidized in the river). The study indicates that about 60% is contributed by tributaries below Iquitos, Peru — implying that the contribution from the Brazilian Amazon is about 0.03 G tons year^{-1}. Of the river's carbon from soil erosion, most comes from the Andes rather than the Brazilian Amazon. Most of the carbon reaching the ocean is in dissolved rather than particulate form. For example, in the measurement at the lowest sampling station for which complete data are available (high water measurement at Tapajós, 768 km above the Amazon's mouth), only 18% of the carbon was particulate. It should be noted that this is based on surface samples, and inclusion of the deeper layers and bedload would raise the percentage of solid material (NB*:* Richey *et al.* adjusted the values for annual transport given earlier to approximate the total load). Much of the dissolved carbon would not be deposited in ocean sediments, and would therefore remain exposed to oxidation. The current annual contribution to ocean sinks from erosion in the Brazilian Amazon is an as yet unquantified fraction of the approximately 0.03 G ton total transported out of the region. In any case, these present day values are small relative to potential releases from forest clearing. However, erosion could increase greatly with large scale deforestation. Substantial erosion rates have been measured under annual crops (Fearnside, 1980b), and recent measurements (in preparation) indicate lower but still significant erosion rates under cattle pasture. In the near future we hope to have better information on the magnitude of erosion as a carbon sink (as well as its impact on agricultural sustainability).

Lugo and Brown assert that my article brings "no new information" to the carbon debate. To summarize the new information for them very briefly: 62 gigatons!

Lugo and Brown present as their example of something that "confuses the issue through misinformation" my not specifying when Amazonia would, in their words, "ejaculate" its load of carbon into the atmosphere. No one knows the answer to this question. As became clear in the earlier exchange of letters between myself and Lugo and Brown (*Interciencia,* 7(6): 361-362) regarding my paper on deforestation rates (Fearnside, 1982), Lugo and Brown have difficulty in distinguishing hypothetical scenarios built on projection of current trends from predictions about future events. They continue in that tradition here. To illustrate the potential magnitude of carbon releases from Amazonia (p. 184) I offered the example of deforestation occurring over a period of 62 years (to make it equal to the number of gigatons of carbon). Using 62 years as the time period rather than the much shorter intervals implied by recent deforestation trends (Fearnside, 1982, 1985c) is, once again, an example of my deliberately erring on the side of conservatism — hardly consistent with Lugo and Brown's charges of bias.

Lugo and Brown brand the article as "clearly alarmist." Reflection on the gravity of the points raised in my paper should provide ample cause for alarm. The term "alarmist," however, has become a shibboleth of persons and groups intent on deflecting public concern from environmental problems in general. Lugo and Brown use the term in this pejorative sense, implying baseless sensationalism. Readers of *Interciencia* are urged to consider carefully the points raised in my paper rather than being swayed by Lugo and Brown's attempt to pigeon-hole it as "alarmist."

Lugo and Brown conclude their criticism by asserting that my paper "sets science back." To the contrary, it interprets in terms relevant to policy a mass of highly diverse and scattered information. The criticisms mounted by Lugo and Brown are useful, as they allow me the opportunity to correct minor faults, to combat mistaken interpretations, and to show the robustness of my conclusions. Such exchanges are, in part, the stuff of which scientific progress is made.

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