

CARTAS AL EDITOR
LETTERS TO THE EDITOR
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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

BIOMASS OF BRAZIL'S AMAZONIAN FORESTS: REPLY TO BROWN AND LUGO REVISITED

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Brown and Lugo (1992a) attack the editors of Interciencia and myself over the handling of their paper on forest biomass (Brown and Lugo, 1992b) and its rebuttal by me (Fearnside, 1992). They allege a lack of "fair play" and complain of an "ambush." I rush to defend both my rebuttal and the journal editors.

Exchanges between myself and Brown and Lugo have now become something of an Interciencia tradition. All of them contain a substantial amount of information on a topic of intense worldwide interest. Like its predecessors, the most recent exchange plays a useful role; it is not a "poor example of how to conduct scientific debate" or something that doesn't "help the progress of science in Latin America." On the contrary, the explicit point-by-point rebuttal of much of the information in Brown and Lugo paper allows readers to compare the evidence directly. The simultaneous publication is devastatingly efficient: readers need wait no longer than it takes to flip to the rebuttal to see what is wrong with the estimate and to have a better alternative.

Contrary to Brown and Lugo's allegation, there is nothing unethical about my rebutting their paper in the same issue of the journal. Many journal editors even encourage such rebuttals. It is quite healthy for a reviewer who disagrees with an author's interpretation to suggest that the manuscript in question should be accepted provided that a rebuttal is also considered. What would be unethical is taking advantage of one's position as a reviewer to try to block other points of view from appearing in print.



I think that the editors of *Interciencia* can be credited with great tolerance in their willingness to entertain last minute revisions through a flurry of faxes when the journal was already in press. Were I the editor, I think that I would have responded to the revision by asking Brown and Lugo to reply in a separate round. I suspect that this was the editors' original intention when they sent my rebuttal to Brown and Lugo. Certainly *Interciencia* would not have incurred the expense of typesetting the original text and my rebuttal in galley proofs if the editors had been expecting Brown and Lugo to replace their already-accepted paper with an extensively rewritten version. Most journals charge authors about US\$5 per line of text altered in the galley proof stage. In my case, I confined myself to adding a postscript.

Brown and Lugo state they interpreted my rebuttal as a set of reviewer's comments, and decided to revise their manuscript to clarify "issues that were poorly discussed." This is an unusual interpretation, given that they received the advance copy of the rebuttal accompanied by a letter from the editors accepting their article manuscript as it stood and informing them that the rebuttal manuscript had also been accepted. I would say that the editors were being very generous in accepting at face value Brown and Lugo's explanation of having thought that the rebuttal was intended as a set of reviewer's comments. Reviewer's comments don't usually come in the form of thirty-page peer-reviewed manuscripts already accepted for publication. As Brown and Lugo's letter mentions (p. 201), they had already received their reviewers' comments separately some time previously. I was one of the reviewers, and deduce that approximately three months elapsed between the time they received the reviewers' comments and the time they received my rebuttal (during which time they apparently did not think it necessary to revise their manuscript). I find it improbable that Brown and Lugo did not understand that their reply to my rebuttal should take the form of a separate round rather than a revision of their manuscript and a corresponding co-evolution of the rebuttal

- all while in press and, by the time they submitted their revision, scheduled for publication within a matter of days. One indication that they really knew that the rebuttal was not reviewer's comments is their extraordinary confession of having revised their article "WITHOUT changing any of our numbers of main points of discussion as a result of his [Fearnside's] comments." When I receive reviewers' comments I generally try my best to incorporate the advice given into my papers.

Brown and Lugo accuse me of "scientific laziness," claiming that I should have rewritten my rebuttal to address only their revised manuscript. Laziness was not the issue. Rather, it was important that both the original and modified versions of the manuscript be rebutted. The original version was widely circulated in manuscript form among researchers concerned with climate change, presumably by Brown and Lugo themselves. As a result, the original version of the manuscript has been repeatedly cited in the literature as "submitted" or "in press," and is *still* being so cited (in fact, I reviewed one manuscript within the last month that used numbers from the original version). In addition, Brown and Lugo have published the original version elsewhere in a conference proceedings (Brown and Lugo, 1992c).

Brown and Lugo's letter states (one might even say boasts) that they used my rebuttal only to reformulate the superficial packaging of their -article without altering any numbers, calculations or conclusions. As pointed out in the postscript to my rebuttal, their biomass estimates based on the RADAMBRASIL and FAO data sets increased by 40% and 11 % respectively, and several of the principal conclusions were dropped, including their inference that logging had caused a massive depletion of forest volume and that biomass estimates as high as those of Fearnside (1990) and Houghton *et al.* (1987) are "not justifiable." Given that they have ruled out my rebuttal as a source of input (and that their revised numbers are not what would be obtained if it had been used), the reasons for these and other substantial changes still remain unexplained. The appearance of the original version in published form (Brown and Lugo, 1992c) in May 1992 now makes it possible for any interested person to verify this.

Brown and Lugo criticize me for writing "on and on" about how their numbers would be used (o misused) in greenhouse calculations. Far from being "hopelessly out of context," it is precisely because of the importance of Amazonian forest biomass to global warming that the scientific community is so interested in this topic. The figures presented by Brown and Lugo have, in fact, been misinterpreted exactly as I predicted (*e.g.* Southworth *et al.*, 1991). A recent conversation with the head of the Intergovernmental Panel on Climate Change (IPCC) revealed that even he had been misled. As I pointed out in my rebuttal, very few people hearing the term "biomass" think that this refers to the aboveground portion of live trees over 10 cm in diameter excluding palms (in addition to excluding vines, litter, dead trees, roots, etc.). Understating forest biomass leads to underestimating the greenhouse impact of deforestation, and the urgency of doing something to slow forest loss.

Brown and Lugo imply that I am engaged in the "ludicrous" enterprise of seeking "one magic number" for biomass,

whereas they are interested in geographic variation. I should point out that, in addition to disaggregating the result by forest type, my rebuttal presents the biomass data in the same RADAM BRASIL volume "grid cell" format used by Brown and Lugo, as well as by state - the geographical unit for which Brazil's deforestation data have been available. The state-by-state breakdown is the key to being able to use the deforestation and biomass estimates together to generate more reliable greenhouse emission calculations (e.g., Fearnside, 1991).

Brown and Lugo charge that I suffer from "confusion about scales and this whole global issue." The feature of my rebuttal that purportedly illustrates this is the list of correction factors proposed for adjusting Brown and Lugo's estimates to account for omitted components. The various items, which individually may be small, together represent a very substantial omission requiring an 83% upward adjustment in the carbon pools implied by Brown and Lugo's estimate. This is hardly an illustration of confusion on my part.

With regard to the question of significant figures, it is important to remember that the numbers about which Brown and Lugo complain are a table of multipliers that are intermediate portions of a calculation. When performing a series of multiplications, roundoff errors accumulate if roundings are applied before reaching the final result. Once the result is reached, then the level of confidence should be expressed through rounding it in the conventional way. It is revealing that Brown and Lugo present no counter argument to any of the correction factors I applied.

An important philosophical difference is brought out by Brown and Lugo's discussion of the varying levels of accuracy and precision of information. Their reaction to this inherent problem is to leave out any information that they judge to have an inadequate data base. One example is underground biomass, about which Brown and Lugo accuse me of making "a big deal." While data are few on below-ground biomass, it nevertheless represents an important stock of carbon. As I stated in my rebuttal, omitting uncertain information does not make the final result more reliable, it only makes it less realistic.

In my view, there is a best current estimate for all items at all times. We must use all of the relevant information, and then work to improve upon this base. The greenhouse effect is a very serious problem and requires immediate and massive action throughout the world. One cannot do as Brown and Lugo imply should be done: pretend that uncertain numbers are irrelevant and should be left out of the calculations. Use made of Brown and Lugo's estimate has already resulted in some substantial underestimates of the greenhouse impact of Amazonian deforestation.

It should be remembered that almost all of the points I raised in the rebuttal remain unanswered by Brown and Lugo. Subsequent expansion of the data set has increased my estimate for the average biomass for Amazonian forests, based on 2,892 ha of data distributed throughout the region (mostly gleaned from published forest volume surveys). The best current estimates (August 1992) for average total biomass dry weight (including below-ground and dead material) are 394 metric tons/ha for all forests present in the Brazilian Legal Amazon and 372 metric tons/ha for the forests cleared in 1990. The increase is mainly due to better information on the forests in the southern part of the region - where most of the deforestation is currently taking place (and for which Brown and Lugo's paper contains no data).

One thing that we can agree upon is the need for good science. As part of doing good science, it is essential to make use of the best data available rather than resisting any information coming from what Brown and Lugo apparently consider to be an unwelcome quarter. This resistance is clear in Brown and Lugo's extraordinary statement that they had not modified any of their numbers in deference to my criticisms. The estimates presented in my rebuttal are not based on more and better-distributed data, but also contain a number of key elements needed for their interpretation that are lacking in Brown and Lugo's paper. Remaining open to new information is a first rule of good science. This, after all, is what exchanges of views such as this are all about.

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