

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 Director

/CARTAS AL DIRECTOR/
/LETTERS TO THE EDITOR/
/CARTAS AO, DIRETOR/

BIOMASS OF BRAZILIAN AMAZONIAN FORESTS:

THE NEED FOR GOOD SCIENCE

Scientific debate is a necessary aspect for the progress of science because it helps focus attention on those aspects of scientific understanding that are not clear, poorly understood, or require additional study and consideration. This is clearly the case with the issue of the role of tropical forests in the global carbon cycle. This field is limited by data, an abundance of misconceptions that detract from research progress, and by the lack of sufficient number of trained scientists and facilities to study the issue as it should be. However, scientific debate, like any other scientific enterprise, requires objectivity and fair play if it is going to enhance understanding, sharpen the focus of future studies, or help scientists advance in the field. The recent rebuttal of our article in *Interciencia* (Brown and Lugo 1992) by Fearnside (1992) is a poor example of how to conduct scientific debate and we explain why in this letter.

We submitted an article on above-ground biomass estimates for tropical moist forests of the Brazilian Amazon to *Interciencia* because we believed that such an article was of interest to Latin America where *Interciencia* is the premier journal. We expected a peer review of this article followed by the customary revisions prior to acceptance and eventual publication. Instead of this customary



peer review process, we received a short review of about two paragraphs from Fearnside followed later by a copy of a manuscript by Fearnside (from the editor) that rebutted our manuscript. A letter from the editor of *Interciencia* accepted our paper and announced the intention to run BOTH articles next to each other.

While one is flattered when one's articles attract such interest, we realized that Fearnside's manuscript raised issues that were poorly discussed in our draft manuscript. This motivated us to treat Fearnside's rebuttal as peer review comments and we proceeded to revise our article WITHOUT changing any of our numbers or main points of discussion as a result of his comments. All estimates in our paper are the product of the methodology as discussed. As we improve our methods a constant research activity, we modify estimates. The revised version of the article was sent to *Interciencia*, as well as to Fearnside. *Interciencia* published the revised version of our article and Fearnside's original rebuttal which was expanded with a postscript but otherwise unchanged.

Two unwritten rules of scientific ethics were violated in this incident. First, the violation of privileged information by a reviewer of an article which converted the confidential peer review process into an "ambush" of those who submitted an article in good faith. Second, by not adjusting the rebuttal after our article was modified, Fearnside showed scientific laziness and the journal allowed this to happen. Both instances don't speak well of the parties involved and certainly don't help the progress of science in Latin America. The correct approach should have been a separation of the peer review process from the rebuttal. The rebuttal should have been addressed to a revised manuscript.

None of the above should be interpreted as our withdrawal from a good scientific debate. On the contrary we welcome debate and now clarify some technical matters in Fearnside's comments that are either wrong or poorly expressed and, as a result, detract from

the basic question of the role of tropical forests in the global carbon cycle.

First, Fearnside is wrong in stating in his postscript that we "withdrew our original manuscript." This is nonsense as we just explained. Second, Fearnside's rebuttal includes elaborate discussion on how to estimate whether a tropical forest is a source or a sink of carbon, items that although important are not the subject of our paper. Therefore, the rebuttal confuses issues as opposed to helping clarify how the role of tropical forests in the global carbon cycle can be eventually understood.

To estimate the role of tropical forests in the global carbon cycle, scientists use computer models that account for the storage, rate of uptake, and oxidation of carbon in all tropical forest undergoing change in land use. These models have many problems (some discussed by Fearnside) that we have addressed in numerous papers dealing (specifically with the way carbon cycle models are structured and documented (e.g., Brown *et al.* 1989, 1991, 1992b, Lugo and Brown 1986, 1992). One way to make models more accurate is to provide data bases that can be used at global scales for simulations that encompass more than a century. Such data bases require certain minimum standards of accuracy and consideration to spatial and temporal scale. In the early models for example, ALL tropical forests of the world were represented by two to three categories and biomass data that covered a few hectares. This was in the early 1980's and since then the field has progressed to the point that models now receive geographically referenced inputs of carbon storage and dynamics, simulate carbon dynamics spatially, and consider historical changes in carbon density and change (Brown *et al.* 1992a, Iverson *et al.* 1992, and ongoing research by scientists from several institutions, including us).

Because all required data are not to the same standard of accuracy nor available in the same scales of space and time, it is necessary to develop analytical techniques to expand available information to the required scales. Methodology requires constant improvement and scientists must clearly present each data set with sufficient documentation and quality control so that modelers can optimize the value of their simulations. Our paper in *Interciencia* was a data paper ONLY that presented geographic information for aboveground biomass of Amazonian moist forests. We made no effort to simu-

late these data nor discuss the whole carbon cycle, nor to review available information for other forest types in the Amazon Basin.

The focus and scope of our paper is clearly stated at the outset in the title. Moreover, we continue to elaborate and improve estimates as the analytical techniques themselves improve. Therefore, our results are what we call them in the title: "estimates". These estimates are bound to change as we learn more about these forests. What is clear and unlikely to change is the fact that data acquired in small plots consistently bias biomass results to higher values. To the degree that one depends on these kinds of data for large scale estimates of biomass, to the same degree one will produce biased estimates.

Given the above, the comments by Fearnside are hopelessly out of context. He writes on and on about the intricacies of the carbon budget of a stand as if this was the subject of our article or that his insights were new. He makes a big deal about belowground biomass, a subject outside the scope of our paper. We challenge Fearnside or anyone else to make a responsible geographically based estimate of the belowground biomass of all tropical forests. Anyone with knowledge of the literature will recoil from such a challenge because of how little data there are. Fearnside is obsessed with the changes in the biomass estimates as if this was a contest on who's estimate is the most rigid or who agrees with whom. In fact, he states in the postscript that we dropped our original conclusion that average biomass values used by Fearnside (1989) and Houghton *et al.* (1987) are unjustified. This is false. We state and still believe that values > 290 Mg/ha "are justifiable" (p. 17). The point is that biomass estimates are geographically and temporally variable and it is ludicrous to seek one magic number for such a diverse region as the Amazon. The weighted biomass estimates are useful for overview comparisons but are irrelevant to the geographic simulation of values.

Finally, Fearnside's (1992) analysis provides the best illustration of what we mean by his confusion about scales and this whole global issue. He proposes correction factors (Table I, Page 20) to five significant figures. If his correction factors were reported to the number of significant figures warranted by the accuracy and precision of the data, most of his correction factors to aboveground biomass would be 1.0.

We welcome thoughtful and constructive comment and discussion on our paper. More important still is the need for research on the carbon and nutrient dynamics of tropical forests. We believe *Interciencia* is the proper forum for reporting regional research findings and for the open discussion of the implications of research results to policy and scientific activity. We hope, however, that this incident that we describe and the quality of the rebuttal that was offered are discarded as models for such future exchanges and to fulfill the goals of scientific excellence for the region.

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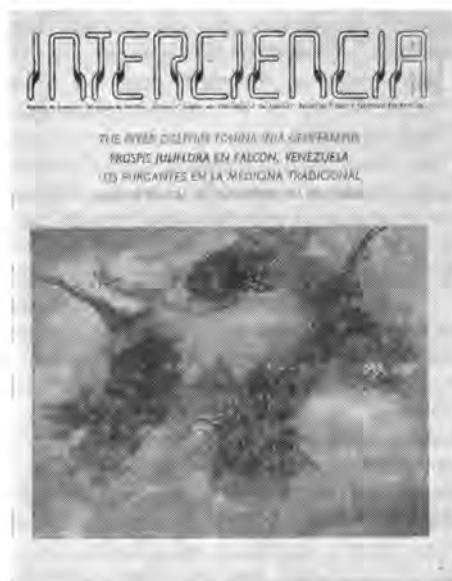
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DISPAROS SIN BLANCO

Con un tanto de sorpresa, por el tono iracundo y la posición dogmática que lo anima, leí en la sección *Cartas al Director* de la revista *Interciencia* un breve artículo del profesor Mario Bunge (Bunge, 1991), de la *Foundations and Philosophy of Science Unit*, Universidad de McGill, Montreal, Canadá. Constituye este artículo una especie de crítica a otro anterior, publicado en la misma revista por J. C. Dávila *et al.*, (1991), de la Universidad de Los Andes, Mérida, Venezuela, titulado "Productividad de la investigación científica venezolana en el área de física según el *Science Citation Index* (1979-1988)".

El profesor Bunge no critica realmente el artículo de Dávila y col. La molestia del Profesor parece venir, principalmente, de una cita tomada de un trabajo de Heidegger (Heidegger, 1960) con la que Dávila y col. encabezan su publicación. El mismo profesor Bunge reconoce que su crítica "puede parecer excesiva..." En realidad, no sólo es excesiva sino inmotivada y carente de fundamento. Y no es que el exceso venga de que —así lo confiesa el mismo crítico— Dávila no "utilice" en el desarrollo de su trabajo (que en verdad sí lo hace) la frase de Heidegger, sino que el profesor Bunge, según lo muestra en los tres primeros párrafos de su artículo, no se ha tomado la molestia de leer el trabajo de Heidegger sobre la ciencia y la investigación científica. El profesor Bunge, por tanto, ignora en qué sentido usa el filósofo alemán el término *empresa* en la proposición que introduce el artículo de Dávila y col. La cita de Heidegger, que tanta inquina parece haber despertado en el profesor Bunge, reza así: "la ciencia en sí, como investigación, tiene carácter de empresa".



El profesor Bunge, que declara no saber cuál de las acepciones consignadas en el *Diccionario de la Real Academia Española* (1981) es la empleada por Heidegger, las analiza una por una para concluir que la proposición de este autor es "ambigua" y constituye "una trivialidad o una falsedad". La primera acepción del *Diccionario*, según el profesor Bunge, define el término *empresa* como "acción dificultosa que valerosamente se comienza". Si tal fuese la acepción escogida por Heidegger, ni siquiera nuestro profesor de Mc Gill, a quien tanta indignación le produce el nombre del pensador alemán, podría objetar que la ciencia, como investigación, tiene carácter de empresa. El profesor Bunge conjetura, sin razón alguna, acerca de la posibilidad de que Heidegger haya empleado la pa-

labra en la cuarta acepción que da el citado diccionario (Bunge no menciona las otras dos acepciones), por la cual el término *empresa* se toma como "sociedad mercantil o industrial". Pero es el caso de que Heidegger declara expresamente que no usa el término de *empresa* en un sentido peyorativo (véase la sección *Addenda*, Nota Nº 2, página 86 del referido trabajo de Heidegger); y, en verdad, en el texto de dicho trabajo nada hay que pueda interpretarse, aun con malicia, en ese sentido. Ergo, la crítica del profesor Bunge ha sido gratuita y sin base, y simplista y superficial su apreciación del trabajo de Heidegger. Así, pues, ya que es evidente, incluso empleando los mismos argumentos del profesor Bunge, que no puede tildarse de falsa la afirmación de Heidegger, habrá que examinar si ella podrá calificarse de trivial, epíteto que reserva el profesor de la universidad canadiense para el otro aspecto de la alternativa de su planteamiento sobre la frase de Heidegger. A un profesor de filosofía de la ciencia, como lo es el señor Mario Bunge, no se le debería escapar que el pensamiento filosófico muy a menudo se centra en lo que es aparentemente trivial, en lo de todos los días, en aquello con lo que contamos sin más, en lo que miramos sin ver, en lo más banal y cotidiano. ¡Cuánta trivialidad contiene eso de *ser*, cuya mención es profusa en el diario hablar (yo soy..., tú eres..., él es..., ello es...), y, sin embargo, cuánto misterio encierra esa "trivialidad" sobre la que el pensamiento filosófico ha girado desde la Antigüedad hasta nuestros días! ("Reflexión —dice Heidegger— es el valor de convertir en lo más discutible la verdad de los propios axiomas y el ámbito de los propios fines"). Reconoce el filósofo alemán que "esa reflexión no es necesaria para todos, ni todos pueden realizarla o siquiera soportarla". Tal parece que este pensamiento es muy acertado.

Que el profesor Bunge no se ha molestado en consultar a Heidegger queda bien demostrado con el artículo del primero. Tampoco parece haber comprendido la