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COMMENT

Hydroelectric dams in Brazilian Amazonia: response to Rosa, Schaeffer & dos Santos

In a critique of my paper on 'greenhouse' gas (GHG) emissions from Amazonian reservoirs (Fearnside 1995), Rosa *et al.* (1996) suggest that I have reached overly pessimistic conclusions because the reservoirs on which my calculations are based are unrepresentative and because methods based on the Intergovernmental Panel on Climate Change's (IPCC) global warming potentials (GWPs) are unfair for comparing hydroelectric and fossil fuel options. I rush to defend my analysis.

Balbina

Rosa *et al.* (1996) criticize my estimates for the Balbina Dam because it is worse than other dams. However, I included estimates not only for Balbina but for all existing large dams in Brazilian Amazonia (Fearnside 1995). While it is true that I presented more information for Balbina than for other dams, I was careful to point out that Balbina is the worst case (Fearnside 1995, pp. 8, 15), and the comparisons with fossil fuels that are the focus of Rosa *et al.*'s (1996) criticism were presented for both Balbina and Tucuruí (Fearnside 1995, Tables X and XI). The conclusion section of my paper (Fearnside 1995) presents totals for all existing dams, of which Balbina is only one. Because Balbina is worse than the average future dam is likely to be, extrapolation from the total impact of existing dams does imply an impact of the full 2010 plan that is somewhat worse than it would be in fact. This impact is not nearly as great as it would be had the extrapolation been done from a single case (Balbina) as Rosa *et al.* (1996) suggest.

Belo Monte

Rosa *et al.* (1996) represent hydroelectric dams in Amazonia in a table with three 'hypothetical' types of dams. These types actually correspond to three specific cases, namely Samuel, Tucuruí and Belo Monte (formerly named Kararaô). These are presented with the implication that they represent options amongst which planners might choose as if picking them off a shelf. If minimizing greenhouse gas emissions were a priority, this could be done by simply picking 'Type C' locations for future dams. Unfortunately, Belo Monte (the model for 'Type C') is highly unrepresentative of hydroelectric dams in terms of area flooded per unit of energy produced, even without a substantial exaggeration which (as will be explained below) probably affects the calculations presented by Rosa *et al.* (1996). Of the 79 planned and existing dams, Belo Monte is probably the best from the point of view of energy production per area flooded, while Balbina represents the worst. However, there is one great difference between Balbina and Belo Monte: Balbina exists while Belo Monte does not. As such, Balbina needs to be included as one of the four existing large dams, while it is misleading to grant Belo Monte one-third of the emphasis in a 'type A, B & C' classification. My paper limited its detailed calculations to existing dams.

The most critical question regarding Belo Monte cannot be resolved from the information in Rosa *et al.*'s (1996) Comment, nor can it be so from their previous presentation of this argument (Rosa & Schaeffer 1995), nor, to my knowledge, can it be so either from any publicly available ELETRONORTE publications on the dam. This is the question of whether the 48.18 TWh y^{-1} output of Belo Monte (Rosa & Schaeffer 1995, p. 155) used to calculate the 0.0438 TWh y^{-1} km^{-2} energy density for the dam given by Rosa *et al.* (1996) represents the output with only this one dam, or whether it represents the output of Belo Monte with the flow of the Xingu River regulated by other proposed dams upstream, especially the notorious Babaquara Dam. I strongly suspect that the latter is the case. This would mean that the area of the upstream dams (or some portion thereof) would have to be included in the denominator of the calculation to have a valid value for energy density, and that the 'greenhouse' gas emissions corresponding to this amount of power would be *much* greater than Rosa *et al.*'s (1996) estimates indicate. Belo Monte's modest area (given as 1100 km^2 by Rosa & Schaeffer 1995, p. 155 and as 1225 km^2 by Brazil, ELETRONORTE 1988, p. 7) would be dwarfed by the 6200 km^2 Babaquara Dam; the total area of the six dams planned for the Xingu Basin is a vast 18 000 km^2 (Seva 1990).

The most controversial aspect of Belo Monte (the first dam and the farthest downstream) is that building it is likely to lead to building the other planned dams (or, under ELETRONORTE's current plans for 'redistribution of the fall' of the Xingu River, possibly dams in slightly different locations). When one dam is built, the benefits are increased for additional dams that may be built upstream of the first, because the output of the first dam will be augmented by regulation of the river's flow. Because Amazonian rivers have strong seasonal cycles in streamflow, much water must be released over a dam's spillway without generating power during the high-water season, while, during the low-water period, streamflow is insufficient to run all of the turbines. In the low-water period, water held in an upstream dam will not only generate power at that dam but also at each dam downstream of it.

The physical location of the proposed Belo Monte is a dam-builder's dream, with a 94 m drop and an average flow of 8600 $m^3 s^{-1}$ (Brazil, ELETRONORTE 1988, p. 3). The problem with tapping it is institutional: Brazil's electrical authorities may declaim as they might that only the first dam is at stake, but such claims are not likely to have any effect on building the other dams when their time arrives in the construction schedule. The history of broken promises (to use a euphemism) in the case of filling Balbina provides a directly parallel example (Fearnside 1989). In the case of Babaquara, ELETRONORTE has not even promised not to build the dam, but only to remove it from the '2010 Plan'; moreover, the 'redistribution of the fall' leaves open the option of flooding the same areas with other dams with different names.

Tucuruí

Rosa *et al.* (1996) make calculations for Tucuruí (their 'Type B') which show that this dam is better than fossil fuels. My analysis (Fearnside 1995) also showed Tucuruí to be better than fossil fuels, although I show higher emissions than do Rosa *et al.* (1996). My paper states clearly, and re-emphasizes in the summary, that in 1990 Tucuruí produced only 40% as much 'greenhouse' impact as would generating the same power from fossil fuels. Tucuruí and Balbina are compared for different years so that the results reflect the same elapsed time after filling; the great advantage of Tucuruí over Balbina is apparent (Fearnside 1995, Table X).

Rosa *et al.* (1996) refer to their analysis as 'even more rigorous' than mine. I find this comparison unconvincing. In the first place, my analysis brings together much more data from measurements in Amazonia (Fearnside 1995) than does theirs. The biomass of vegetation in the reservoirs is one of the critical parameters affecting emissions. Rosa *et al.* (1996) got their value from Setzer and Pereira (1991), who, in turn, had obtained it from one of my early estimates for biomass of forests throughout the region (Fearnside 1987). My hydroelectric calculations (Fearnside 1995), however, use data from measurements made at all dams except the smallest (Curuá-Una), which occupies only 1.3% of the forest area flooded in Amazonia by 1990.

Rosa *et al.* (1996) assume without explanation that 30% of decomposition is anaerobic (i.e., to CH_4). Although my estimate also contains a number of assumptions, it takes great pains to calculate the vertical distribution of the biomass, the areas of reservoirs at different depths, and the amount of biomass exposed to anaerobic decomposition at different times (Fearnside 1995). Rosa *et al.* (1996) consider all biomass to have a half-life of seven years; this is also an unsupported assumption, and one that is contradicted by the virtually intact biomass present in the anoxic zone of Balbina over seven years after filling (Fearnside 1995). The 70% that, under Rosa *et al.*'s (1996) assumptions, presumably decompose aerobically to CO_2, are simply ignored: comparisons are only of dam-derived CH_4 to fossil fuel CO_2. They need to compare total emissions of both gases. The comparisons of dams with fossil fuels (Rosa *et al.* 1996, Table I), which ignore most of the carbon released on the reservoir side of the balance, are grossly misleading.

Rosa *et al.* (1996) compare my values for carbon above minimum water level in 1990 in Tucuruí with total reservoir emission for that year, and conclude that all carbon emissions would be released in an unrealistically short period of 2.25 years. However, several errors invalidate their calculation. First, rather than carbon, the 6.41 million t value refers to biomass (Fearnside 1995, Table VII). Only half of the biomass dry weight is carbon, thereby doubling the time. Even more important, however, is that the 2.25 year value calculated by Rosa *et al.* (1996) misrepresents how decay occurs. Decay is not a straight line decrease to zero, as implied by Rosa *et al.*'s (1996) calculation, but rather a form of exponential decline, falling quickly at first and then levelling off. Rosa *et al.* (1996) mention the exponential nature of decay parenthetically after their calculation, but the erroneous impression is left that somehow their calculation casts doubt on the validity of mine.

The form of the decline in remaining biomass is important to understanding why high rates of emission are to be expected in the first years after filling a reservoir. Because of the many tree species with differing resistance to decay, the decline is not a simple exponential described by a single decay constant. I handled this problem by dividing time into four periods and applying a different exponential decay rate in each period (Fearnside 1995, Table VI). After the initial rapid decline in biomass, the amount remaining at any given time up to the end of the time horizon is greater than it would be were a single rate applied. In fact, it should be even more so than the calculations indicate due to some highly resistant species. For example, in Gatun Lake (created by the Panama Canal), some trees were still standing over 70 years after flooding (Bultman & Southwell 1976). In 1990, however, the Tucuruí Reservoir had only been filled for six years, and decomposition was still rapid. In my model (Fearnside 1995), the decomposition rate falls to less than half that rate from the seventh year onwards.

Rosa *et al.* (1996) use the numbers in my paper to calculate that a coal-fired plant would produce 14 times more GHG impact over a 100-year period, while the equivalent value for natural gas would be six times (NB: neither of these fuels is a real option in Eastern Amazonia where Tucuruí is located). Rosa *et al.*'s (1996) calculation of the emissions of Tucuruí over 100 years, done by multiplying the rate of emission in 1990 (Fearnside 1995, Table XI) by 100, is not valid for two reasons. On the minus side, it ignores the much greater emissions in the first six years after filling (prior to 1990), while on the plus side it ignores declining emissions after that date (Fearnside 1995, Fig. 5). However, if the comparison were done correctly by simulating emissions over a 100-year horizon and summing them, the result would also show Tucuruí (but not Balbina) to have a lesser greenhouse impact than fossil fuel.

Rosa *et al.*'s (1996) suggestion of a 100-year comparison raises the question of how such a comparison should be made. I would suggest that a simple unweighted summing does not reflect society's best interests, and that concentration of impact in the early years in the case of hydroelectric dams would turn the results *against* hydroelectricity if emissions (or their impacts) were weighted for the time when they occur (i.e. through discounting or an alternative time preference indicator). This problem was not broached in my paper (Fearnside 1995), which restrained itself to 1990 comparisons; 1990 is the base year for which countries of the world are currently undertaking national emissions inventories under the Framework Convention on Climate Change.

Sound reasons exist for some form of time preference weighting for global warming impacts, rather than the zero-discount scheme employed by Rosa *et al.* (1996). Buildup of GHG in the atmosphere initiates a stream of impacts (including increases in human death rates), not just single-event impacts. If this stream of impacts begins later rather than sooner, the savings between the sooner and the later time, for example, of human lives, represents a permanent savings, even though the same individuals may die the next year. The logic is directly parallel to the accepted practice of considering avoided fossil fuel emissions as permanent savings, even though the same barrel of oil may be burned the next year. Applying even a very small discounting would greatly increase the impact of the large initial pulse of hydroelectric-dam emissions, relative to the evenly distributed emissions from fossil fuel.

Two factors need to be included to make a fair comparison of hydroelectric impacts with those of fossil fuel generation that would worsen the impact of hydro. One is correction for losses of energy in long-distance transmission, which is not a factor for fossil fuel energy generated at the site where it will be used. The other is the fact that large hydroelectric dams typically take up to a decade or more after closing the dam for all of their generators to be installed, during which time biomass is decaying in the reservoir with high emissions of GHG. If a time preference mechanism (such as discounting or an alternative) is applied, this will weigh heavily against hydro as compared to calculations made using the full configuration of the power station (such as Rosa *et al.*'s (1996) calculation for Belo Monte).

Rosa *et al.* (1996) find that my Tucuruí results are 'qualitatively different' from theirs. As far as I can determine, the reason for this is that they have simply omitted CO_2 emissions from the results and used only methane (despite their statement in the Comment that a comparison is made using only CO_2); their previous presentation of the type A, B and C classification (Rosa & Schaeffer 1995, p. 155 Table 2, footnote f) states clearly that only methane is included. While my estimate of methane emissions is significantly lower than their assumed 30%, CO_2 would represent a substantial part of GHG impact, had Rosa and Schaeffer (1995) considered it, even under their assumption of less carbon being emitted in this form.

Global Warming Potentials

Rosa *et al.* (1996) question the IPCC's GWP methodology, and suggest that their own method of comparing the impacts of different emissions would make hydroelectric generation compare better with respect to fossil fuels than does the IPCC method I used. Since the IPCC is an international body seeking to represent consensus on scientific issues related to climate change, criticizing my use of these 'official' methods is a bit out of place. However, I too have questioned the IPCC's GWP scheme (Fearnside 1992) – but my own alternative (not employed in Fearnside 1995) has the effect opposite to that of Rosa *et al.*'s (1996) method. That is, my method makes hydroelectric compare less favourably with fossil fuels. By giving equal weight to events 100 years in the future (in the scenario emphasized by the IPCC), IPCC GWPs understate the impact of emissions from reservoirs. This is both because of the unequal distribution of emissions over time (concentrated in the first years) and because of the emission of CH_4, a gas whose short atmospheric life makes it appear less important when equal weight is given to periods far in the future.

Rosa *et al.* (1996) criticize the work of Lashof and Ahuja (1990) for considering emissions from a single pulse, rather than a stream of emissions over time. A formulation that provides for a stream of emissions is indeed needed. However, I would point out that, by including effects of time preference, the method of Lashof and Ahuja (1990) is more advanced than either the IPCC or the Rosa *et al.* (1996) methods.

Rosa *et al.* (1996) apply their own formulae to make a comparison with my results. They mention 'CH_4 emissions not considered by Fearnside'. I hasten to point out that CH_4 emissions were, in fact, included in my results. For converting CH_4 to CO_2 equivalents, I used IPCC values at the time of publication, and did not enter into controversies surrounding GWPs other than to mention that GWPs for CH_4 were likely to be revised upward (Fearnside 1995). This is a change that has since occurred, thereby increasing the impact of reservoirs relative to fossil fuels.

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