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# GREENHOUSE GAS EMISSIONS FROM HYDROELECTRIC DAMS: REPLY TO ROSA ET AL.

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Ever since my first estimate indicating high emissions of greenhouse gases from Amazonian dams (Fearnside, 1995), Luis Pinguelli Rosa and coworkers have effectively made a career of trying to prove me wrong. However, the longer this debate goes on and the more information that becomes available, the greater the impacts are found to be. The latest attack (Rosa et al., 2005) serves to illustrate a series of misconceptions regarding the science and brings out some of the political context that surrounds the subject in Brazil.

First, a variety of claims in the Rosa et al. (2005) letter are technically incorrect, and would mislead readers not familiar with the details of the previous rounds of this debate. Rosa et al. (2005) claim that I assume that the $CH_4$ concentration is "uniform in the reservoir and constant for many years." Neither assumption is made. The calculation in question (for Tucurui) only applies this concentration to the depth of the turbines—not as a concentration that is uniform throughout the reservoir. In fact, it is conservative in that the value is based on a measurement at 30 m depth, and is assumed to be the same at the 34.6-m depth at the turbine intakes, even though methane concentrations are well known to increase steadily with depth anywhere below the thermocline. Neither is it assumed to be constant for many years, nor to be unduly "extended through extrapolation." The "extrapolation" referred to is only for one year, from 1989 (the year of Tundisi's measurement) to 1990 [not 1991]. The year 1990 was used for the estimate because this is the standard year for the initial national inventories of greenhouse gases under the United Nations Framework Convention on Climate Change. The proportions of water passed through the spillway versus the turbines are from 1991, the year closest to 1990 for which data were available.

The graph (Graph 1) reproduced from Galy-Lacaux et al. (1999) shows the annual cycle of methane concentration variation at the Petit Saut reservoir in French Guiana, together with the expected general decline over time as the reservoir ages. The March 1989 measurement from Tucuruí (rainy season at age four years) corresponds to the fourth trough on the saw-tooth like graph. The decrease from year four to year five is approximately 0.5 mg/L (about 12.5%)—hardly enough to explain the supposed nineteen-fold exaggeration in methane emissions.

The value at age four years in the Galy-Lacaux et al. (1999) graph is approximately 50% lower than the corresponding number measured at Tucuruí (4 versus 6 mg/L). However, there is a good reason for this: the Galy-Lacaux et al. (1999) graph is for average methane concentrations in the full water column of the reservoir, whereas the concentrations mentioned from my paper refer to the necessarily higher concentration at the depth of the turbine intakes. Since their 1999 publication, the Petit-Saut research group has switched to my method of calculating turbine emissions from the concentration at the turbine depth, rather than the average for the water column (Delmas et al., 2004; Richard et al., 2004).

The Henry's Law constant mentioned in my editorial comment comes from a standard chemical reference (Geventman, 1999). Unit conversions follow Plambeck (1995) and are not believed to be in error. Rosa et al.'s (2005) conclusion from the alleged error in the Henry's Law constant is remarkable, as the supposed error is in the wrong direction for the inference they draw. If the equilibrium concentration of methane in water at one atmosphere pressure (i.e., in the tailrace) is *lower* than I calculated, then one should conclude that *more* (not less) $CH_4$ is released when water that is super-saturated with methane emerges from the turbines. Even if the value for the Henry's Law constant were the value suggested by Rosa et al. (2005), the estimates of $CH_4$ release presented in my editorial comment would remain the same. Both the discussion of Henry's Law and of soft drinks in my editorial comment were illustrations of the principles involved in methane release. The actual calculation of the amount released, however, was based instead on published measurements of $CH_4$ concentrations above and below real dams. Rather than a theoretical calculation from temperature and pressure changes, the bottom line, which Rosa et al. (2005) think "absurd", is based on a more direct line of evidence.

Rosa et al. (2005) suggest that the argument for a rapid release of methane when water emerges from the turbines of a hydroelectric dam is somehow invalidated by the difference between the "few seconds" mentioned in my paper as the time necessary for release of gas bubbles from a bottle of Coca Cola and the half hour they refer to for the last bubbles to emerge from a leisurely consumed bottle of Brazil's politically correct soft drink--Guaraná. Unfortunately, whether the release occurs in a matter of 30 seconds or 30 minutes makes little difference for this important conclusion. In either case, the methane from the turbines is released to the atmosphere before there is time for bacteria in the river to degrade it to $CO_2$ while the methane is still dissolved in the water.

Rosa et. al. (2004) claim that there is "no indication of any sudden methane emissions into the atmosphere" in the tailrace because, if there were a sudden release, there "would be almost nil methane concentrations" in the tailrace water. A drop to a concentration of zero is not necessary for there to be a very significant release in the tailrace. The assumed 55.5% midpoint for release (immediate and downstream) from turbined water, based on Petit Saut, is sufficient to have a substantial atmospheric impact. Release of large quantities of methane when pressure is suddenly reduced as water passes through the turbines is the explanation believed to account for the drop in methane concentration measured at Petit Saut (Galy-Lacaux et al., 1997, 1999). A strong confirmation of this conclusion, without the complicating factor of Petit-Saut's aerating device, is provided by results from the Balbina Dam in the Brazilian state of Amazonas. At Balbina, monthly measurements over a six-month period indicate that the average release of exported methane immediately at the turbines (through bubbling in the first 50 m of the river below the dam) is 42.4%, and that 13.6% of the remaining methane is released in the river downstream of the dam (A. Kemenes, personal communication, 2005). Of the total methane exported through the turbines, the release therefore averages 65.5%--a value higher than the 55.5% midpoint of the range used in my calculation for Tucuruí (Fearnside, 2002a). The higher turbine flow (and resulting turbulence) at Tucuruí as compared to Balbina means that the percentage emitted in the tailrace would be greater at Tucuruí, making the calculation I published for Tucuruí's turbine emissions even more conservative. In the case of methane in the water from the spillway at Tucuruí, this is best considered to be completely released because the water is pulverized into tiny droplets as it falls 58 m to a reinforced-concrete dissipation basin, including a "ski-jump" device that launches the water skyward half-way down its decent (see Fearnside, 2004a).

Rosa et al. (2005) have apparently misread the description of the $CO_2$ portion of my calculation. They state that I add the "$CO_2$ emissions not only from the hydroelectric dam tailrace, but also from the water surface". Neither of these $CO_2$ sources is included in my calculation; the only $CO_2$ included in the calculation is that from above-water decay of dead trees. The release of dissolved $CO_2$ either at the reservoir surface or in the tailrace is not considered precisely because a portion of this emission is from carbon that has been fixed by photosynthesis in the reservoir itself (for example by algae, drawdown vegetation and macrophytes) and is therefore not a net contributor to global warming.

Rosa et al. (2005) have confused the numbers in my calculation of the spillway emission, suggesting that the methane concentration at this depth should be 3 instead of 7.5 mg/L. The methane concentration used was neither of these values, instead having an annual average of 4 mg/L (see Figure 1 in Fearnside, 2004a). The 7.5 mg/L value refers to concentration at the turbine depth, rather than the shallower spillway depth.

The digression on global warming potentials (GWPs) and alternative indices of equivalence among greenhouse gases partially explains how Rosa et al. (2005) managed to compute such a low impact for hydroelectric emissions. Several hats are in the ring for alternatives to the GWP formulation currently used by the Intergovernmental Panel on Climate Change (IPCC) and the climate convention, including both my own alternative (Fearnside, 1997, 2002b) and that of Rosa and Schaeffer (1995). Rosa et al. (2005) use the Rosa and Schaeffer (1995) alternative to compute their value for the impact of Tucuruí; as they state, the impact of methane in this calculation is equivalent to a GWP of only 7 (one ton of $CH_4$ having the impact of seven tons of $CO_2$). As compared to the impact indicated by internationally accepted values for the GWP of methane, the impact computed using a GWP of 7 is only one-third or less. In my case, rather than use my own alternative, I used the GWP of 21 for methane from the IPCC's Second Assessment Report that has been adopted by the Kyoto Protocol for the first commitment period. If I had a bias towards exaggerating the impact of hydroelectric dams, as Rosa et al. (2005) strongly imply, I would have used instead the GWP of 23 for methane from the IPCC's Third Assessment Report, giving an impact 9.5% higher while still using an internationally accepted value for this key parameter.

Rosa et al. (2005) do not provide any information on their estimate that produced a value for Tucuruí 19 times lower than mine. This information would be necessary to identify where the remainder of difference lies, beyond the three-fold difference explained by their choice of GWP. However, assuming that they are referring to the same work presented in their editorial comment (Rosa et al., 2004), the main difference is in simply ignoring all emissions from the turbines and spillway, as well as the above-water decay of the trees left standing in the reservoir. In order to compare apples to apples, they also must compare emissions in the same year (in this case, 1990).

I am accused of the sin of not having mentioned a proposal from Pinguelli Rosa's research group to measure tailrace methane concentrations below Tucuruí. Unfortunately, having a proposal is not the same thing as having completed the measurements and published the results. The research group can rest assured that their results will not be "ignored" once they come into being. The implication that these results will show that a five-year-old dam (Tucuruí in 1990) emits little methane has no basis that I can see. Tailrace measurements at Petit Saut (Delmas et al., 2004; Richard et al., 2004) suggest otherwise.

Rosa et al. (2005) devote a paragraph to the need for deducting pre-dam emissions in order to arrive at a net result for the impact of a dam. The implication is that I have exaggerated net emissions of Tucuruí by omitting such a deduction. To the contrary, my calculations for Tucuruí include just such a deduction (see Fearnside, 2002a). Rosa et al.'s (2005) statement that the pre-dam emissions have "massive greenhouse gases potential" in the case of the Belo Monte Dam does not agree with my calculations for that dam (Fearnside, 2005).

As for the charge that "Fearnside takes advantage of having English as mother language and of his American origin, to publish too much abroad …", I would point out that I have also published extensively in Portuguese over the course of my 29 years in Brazil, including explanation of hydroelectric emissions (e.g., Fearnside, 2004b). In addition to English-language publications, readers can find Portuguese-language publications and unpublished translations at http://philip.inpa.gov.br.

With regard to the "political insinuations about a statement by José Miguez," insinuations were hardly necessary since the statement in question was explicit and unambiguous, and readers can draw their own conclusions from the quote itself. Tellingly, Rosa et al. (2005) offer no alternative interpretation as to what Miguez might have meant by his statement. The statement is truly extraordinary coming from the person responsible for overseeing Brazil's national inventory of greenhouse gas emissions, explaining why findings of high emissions from hydroelectric dams represent a political danger to Brazil in international negotiations, and that the subject was therefore entrusted to ELETROBRÁS [that is, to L.P. Rosa]. His words were:

> *"We* [the MCT climate sector] *talked with Prof. Pinguelli* [Rosa] *and I asked the help of ELETROBRÁS* [on the subject of greenhouse gas emissions from dams]*; actually, it was ELETROBRÁS that coordinated this work* [*i.e.*, the work reported in Rosa et al., 2004] *exactly because of this, because this subject was becoming political. It has a very great impact at the World level; we are going to suffer pressure from the developed countries because of this subject. And, this subject was little known. It is mistreated. It is mistreated and continues to be mistreated by Philip Fearnside himself, and we have to be very careful. The debate that is taking place now in the press shows this clearly; that is to say, you can take any one-sided statement to show that Brazil is not clean, that Brazil is very remiss, that Brazil, implicitly, will have to take on a commitment* [to reduce emissions] *in the future. This is a great political debate and we are preparing ourselves for it."* (Brazil, MCT, 2002).

The implication that mentioning the statement is somehow unethical is strange, since Miguez, as head of the climate sector of the Ministry of Science and Technology, not only made the statement but also convened the meeting in question, had it tape recorded, had the recording transcribed, and had the transcription posted on a public website for which he himself was (and is) responsible.

One can't help being amazed by the lengthy defense spurred by the mere mention of the fact that Luis Pinguelli Rosa was head of ELETROBRÁS. As I made clear (Fearnside, 2004a), at the time of the 2002 statement quoted from J.D. Miguez, which mentions Pinguelli Rosa by name in association with ELETROBRÁS, he was not yet the head of the agency. My mention of Pinguelli Rosa's position as head of the agency was made with no additional commentary, not even pointing out that he held this office at the time that he wrote his editorial comment (i.e., without "insinuation" regarding possible conflict of interest). Pinguelli Rosa asserts that mention of ELETROBRÁS is somehow a breach of ethics. However, if there is an ethical issue here, surely it is that Pinguelli Rosa published his editorial comment identifying himself as a mere professor of physics at the Federal

University of Rio de Janeiro, without mentioning that he also held the office that made him the number one person in the Brazilian government responsible for promotion of hydroelectric dams. I believe this is relevant information in the case of a paper that claims, in essence, that hydroelectric dams are not so bad after all.

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