# Fearnside, P.M. 2000. Comments by Philip M. Fearnside.18 Oct.1999. [on World Commission on Dams Thematic Review II.2 on greenhouse gas emissions from hydroelectric dams]. World Commission on Dams, Capetown, South Africa. pp. 85-88. (www.dams.org/docs/kbase/thematic/ drafts/tr22_part2_finaldraft.pdf)

1. It is a fact that the rate of anaerobic decomposition by methanogenic bacteria is affected by the temperature. In addition to the factors described in this study a clear correlation between $CH_4$ emissions and temperature which will help in adopting the emission factors in other regions is needed.

2. A common protocol for the measurement of $CH_4$ emission from hydroreservoir should be recommended.

with best regards,

R. Uma

## e) Comments by Philip M. Fearnside, 18 Oct. 1999

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The report reviews a body of past work on greenhouse gas emissions from reservoirs, particularly the authors' previous estimates, and concludes in general that dams are not so bad as has sometimes been portrayed. Although this "bottom line" (whether dams are good or bad in terms of greenhouse gases) is not very clearly stated, it comes through in various ways in their presentation of the Belo Monte Dam as an example. I hasten to add that the Belo Monte Dam is highly atypical, with a power density (installed capacity per unit area of water) ten times the average for planned dams in Brazilian Amazonia, if calculated in the way done in the report (ignoring upstream dams).

A variety of methodological and quantitative problems make the estimates of the greenhouse gas impact of hydroelectric reservoirs, as presented in the report, gross understatements of the magnitude of this impact. One of the most important is that the estimates presented here completely ignore emmissions from water passing through the turbines. The measurements listed (Table 4.1, p. 6) are only for bubbling an diffusion from the reservoir surface, and do not include estimates of releases downstream from water that has passed through the turbines.

In addition to greatly understating the emissions from hydroelectric dams, ignoring emissions from the turbines goes counter to an important part of the logic given in Diagram 1 on p. 3, namely that for hydro "emissions do not depend on electric energy but on time dependent reservoir processes." In the case of methane release from water passing through the turbines, which represents a substantial part of the total impact, the emissions are directly proportional to energy production. As installed capacity increases, more of the river's annual water flow is passed through the turbines and less over the spillway, thereby releasing more methane. Tucurui-II is a current case-in-point: increasing installed capacity for the same reservoir is not a zero-impact decision as the government currently claims. Among its impacts is increased methane emission.

The treatment of the mechanisms of greenhouse gas production in reservoirs ignores the role of macrophytes (p. 4). The authors believe that most methane comes from decay of submerged trees, which they deduce from the peak of methane emissions in the early years after reservoir filling. The disappearance of biomass through decay is taken to explain the subsequent decline in methane emissions (p. 4). However, macrophytes also follow this pattern, covering a much larger fraction of the reservoir surface in the early years, and then declining as water fertility becomes insufficient to support the macrophyte growth. Methane emissions are much higher in macrophyte beds than in open water, suggesting that these floating weeds, rather than the submerged trees, are the major source of carbon for anaerobic decay. Macrophytes are not mentioned in the text, which restricts its discussion of carbon sources to decaying trees, plancton, and soil carbon. The group at INPE studying Tucuruí has also concluded that macrophytes are the major source of carbon decaying anaerobically to produce methane.

The report relies on mere guesses for the values of a number of important parameters, such as the percent of the carbon in the flooded biomass that is emitted as $CH_4$ versus $CO_2$. The report gives widely varying numbers for this critical parameter: from 10-30% decomposing anaerobically (notes d & e, p. 22), to 30% decomposing anaerobically (p. 22), to 30% of total initial biomass being emitted as methane over the first century, a value representing 50% of decomposition that occurs over that period (p. 26, Table 4.3).

Another critical parameter that is apparently based on a guess is the rate of decomposition of the forest biomass. The average lifetime is given as 7 years (p. 22; p. 32 note vi), yet elsewhere in the report (p. 26, Table 4.3) a very different number is apparently assumed that would result in 40% of the biomass being left after 100 years. I might add that trying to represent biomass decay with a single negative exponential decay constant is bound to be inadequate because different categories of biomass decay at radically different rates. A much more complete subdivision of biomass into different types of material and different vertical and horizontal zones has been done (Fearnside, 1995), but this is not the approach taken here.

Another critical parameter is the amount of biomass in the forest that is flooded. The report uses biomass estimates that are much lower than the best current estimates for this parameter. It also generalizes from a single average value for Amazonia, whereas estimates are available that are specific to each reservoir (see Fearnside, 1995). The report uses a range of published estimates for biomass as a means of judging the range of uncertainty for the resulting greenhouse gas emissions. However, the range of biomass values appearing in the literature is very much greater than the range of true scientific uncertainty because a number of the literature values contain known errors in their derivation (see review in Fearnside et al., 1993). A case in point is the value used in the report for the low end of the range: 155.1 t/ha from Brown and Lugo (1984) (Table 4.2, p. 21). This value has been very thoroughly debunked (Fearnside, 1986), and is no longer supported by its original authors. Nevertheless, the ghost of this estimate still haunts us, as proven by this report! Current best estimates of average biomass in Amazonian forests are over 400 t/ha (Fearnside, 1999, updated from Fearnside, 1997a). The value used for the high end off the range (360.5 t/ha from Setzer and Pereira, 1991, who got it from one of my earlier estimates) is also superceded.

The problems mentioned above with parameters for biomass, the rate of biomass decay and the percentage of the decay that occurs through anaerobic pathways to produce methane are many. Although better numbers might be suggested, in the case of methane emissions it is probably better to base estimates on the various direct measurements of emissions that are now available rather than trying to derive it from assumptions on biomass decay rates and pathways. This is not the case, however, for carbon dioxide, but the report appears to be ignoring the production of $CO_2$ by biomass decay in reservoirs.

The report ignores $CO_2$ emission from hydro (especially from decay of trees that project above the surface of the water) in comparisons of hydro with thermal generation. See Fearnside (1996) for criticism of several of the numbers presented here. For example, in Figures 1-4 (pp. 23-24), the comparison is only between $CO_2$ produced by thermal versus $CH_4$ produced by hydro. A fair comparison requires inclusion of all gases from both sources, as was done in my comparison of Tucuruí with thermal generation (Fearnside, 1997b).

The report ignores the greenhouse gas impact of upstream dams in calculating emissions/unit area of water, especially for Belo Monte. The flow of the river must be regulated by upstream dams in order to achieve the power output assumed, since the small reservoir at Belo Monte proper could not store enough water by itself to generate this much power. This omission is what gives such extraordinarily positive results for calculations of the impact of Belo Monte. The report reproduces unchanged the group's previously published arguments on this, and fails to make any mention of the publication

(Fearnside, 1996) that debunks their argument. This is a rather glaring omission(!) The tremendously high power density (Tables 6 and 9 on pp. 21 and 26) and low emissions for Belo Monte presented in the report can only be obtained by ignoring the planned upstream dams. The dam that is now called the "Altamira Dam" (formerly known as Babaquara) is listed in the current ELETROBRÁS Decennial Plan (p. 148) for construction by 2013. It would flood over 6000 $km^2$, most of which is rainforest in indigenous land, mainly to increase the water storage for power generation at Belo Monte. The substantial greenhouse gas emissions that the Altamira Dam would cause, in addition to its severe biodiversity and human-rights impacts, would have to be included in the assessment of Belo Monte in order to have a fair comparison with fossil fuel.

The report contains an extensive review of controversies related to how best to express the relative impact of different gases, as through the global warming potentials (GWPs) derived by the IPCC. Given the topic of the report, I would say that the question of alternative indices is a bit overemphasized. However, I too have criticized the IPCC's GWP system, and the authors are correct in identifying some of its deficiencies. I should point out, however, that their discussion completely omits any mention of my own alternative methodology for assessing the impact of reservoirs (Fearnside, 1997b), which I would claim represents a better mousetrap not only with respect to the IPCC's GWP system but also with respect to the alternative that is proposed in the report. This is because it results in a number that has consistent time horizons for both the emissions and the impacts of the atmospheric load of gases (see Fearnside, 1997b).

It bears mention that the Kyoto Protocol explicitly specifies that the SAR 100-year integration GWPs will be used for comparing the different gases. Even though the authors are correct that there are grave deficiencies with this procedure, they also need to provide information in terms of the GWP measure adopted by the Protocol, especially since part of the discussion involves the possibility of carbon credit for avoided emissions under the CDM.

The Appendices on the CDM, valuation of greenhouse gas impacts, and the discussion of the inequalities of rich and poor nations in climate negotiations are rather far afield from the topic of the paper, despite being very important topics in themselves. An Appendix is also included reviewing previous studies (Appendix 2, p. 41). This review is remarkably dated; I suspect that it must be reproduced verbatim from something written around 1992. The authors state that the only existing previous research are a 1990 study by Bruce and a 1992 paper by myself. Most notable is lack of mention of the series of estimates I have published for Amazonian dams (Fearnside, 1995, 1997b). Although these studies are mentioned briefly elsewhere in the text, they are missing from the bibliography (p. 33). The work by the group at INPE is also completely unmentioned. The report criticizes my 1992 calculation by observing that "the criticism that could be made of Fearnside's approach is precisely the lack of 'on-site' measurements and the extrapolation of data, which could certainly result in the overestimation of emissions from hydroelectic dams". Although it represented the state of the art at the time, my 1992 estimate could indeed be in error because of the need to use data from elsewhere. This error could be either up or down, not only up. In the years since that estimate, much more information has become available. That information has consistently pointed to higher greenhouse gas emissions, especially of methane, than the ones I calculated in 1992.

The report contains some information on atmospheric chemistry that is either erroneous or unclear. The life of $CH_4$ is given as "around 15-20 years" (p. 30). The current IPCC Second Assessment Report (SAR) estimate for adjustment time for emissions occurring now is $12.2 \pm 3$ years (SAR, WG-I, p. 70). In terms of "turnover time" of the current atmospheric load, it is 8.6 yrs (SAR, WG-I, p. 70). The former value is what the authors mean. The global warming potential (100-yr integration) for methane is given as 20.1 (p. 31). However, this is a value on a per-molecule basis, whereas all of the calculations in the report are in terms of tons. The current IPCC value (in terms of mass) is 21 (SAR, WG-I, p. 121).

The term "radiative forcing" is used by the authors to mean the <u>integration</u> of heat absorption over a period of years, such as a 100-year time horizon (pp. 1-2). This is not at all the way the term is used by the IPCC, which is as an instantaneous measure of absorption (in Watts/$m^2$), either at the time of emission or after a delay of few months when stratospheric and tropospheric effects have equilibrated (SAR, WG-I, p. 109; see also the definition of GWPs in IPCC 1994, p. 215). The term "GWP" is also sometimes used with meanings different from the IPCC sense. For example, the report states that "methane has a short lifespan of around 15-20 years during which its GWP is over 20 times that of $CO_2$" (p. 30). However, the GWP refers to a standardized time horizon, such as the 100 years adopted under the Kyoto Protocol, not to the lifespan of a particular gas. For a 15-20 year period the value would much higher than the "more than 20 times" suggested here: for a 20 year period the current IPCC value for a ton of methane is 56 times that of a ton of carbon dioxide (see Table 2.1, p. 50).

To summarize, while the report brings together a considerable body of work on the subject of emissions from reservoirs, it is also notable in its omissions. These omissions are not random: they ignore a variety of kinds of evidence indicating greater emissions of greenhouse gases from hydroelectric development than the authors suggest.

**Literature Cited**

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## f) Comments by Luc Gagnon- October 29, 1999

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**Overall comments on the thematic review**

1. It is essential to make the distinction between "gross" emissions (output that is being estimated or measured) and "net" emissions (considering also emissions that would have happened without the project). Some data presented in the paper give some indication that this is a key issue: in Box 1.5, emissions (per square meter) of $CH_4$ from natural Amazon floodplains can