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About the Mr Fearnside commentary in Mongabay (death of fish in dams)

From Angelo Antonio Agostinho

I carefully read the commentary "Brazil's Sinop dam flouts environmental legislation" by Dr. Philip Fearnside, published in Mongabay site, about fish deaths below dam in early February 2-4, 2019. I considered the text, herein named Commentary, hasty and based on unrealistic data and preconceived assumptions. This text is intended to point out the mistakes made in this commentary, according to field data and my conceptions.

I was one of the professionals responsible for the initial document (an Expert Opinion; EO herein) which recommended the partial removal of the vegetation before filling the Sinop Reservoir and drawing attention to the fact that the amount of vegetation to be suppressed should be based on the results of a mathematical modeling of water quality and the demands of the biota. The importance of partial removal to the fish fauna conservation has been the subject of my research and publications in the last 20 years (Agostinho AA e Gomes LC. 1997. *Reservatório de Segredo: bases ecológicas para o manejo*. EDUEM, Maringá - PR, 387 p.; 2. Agostinho AA e Gomes LC. 1998. A remoção prévia da vegetação nos represamentos. *Boletim da Sociedade Brasileira de Ictiologia*, 53:13-14. 3. Agostinho AA, Gomes LC, Pelicice FM. 2007. *Ecologia e Manejo dos Recursos Pesqueiros em Reservatórios do Brasil*. Maringá. EDUEM. 501p., and others) and is supported by 40 years of research on fish fauna in reservoirs, in distinct Brazilian basins, including the Amazonian. The recommendations in the EO do not differ from what I have been advocating in scientific meetings and courses along these years. Attributing my recommendations to the defense of the interests of the hydroelectric sector is a complete ignorance of the ethical values and principles I defend.

The imprecision of the text can be immediately detected in the first paragraph where the author starts from the wrong premise that the law determines the complete removal of the vegetation before damming. However, the Article 2º of the Law 3.824 of November 23, 1960 determines that "Areas with vegetation will be kept that, according to technical criterium, are considered necessary for the protection of fish fauna and preserves indispensable for the guarantee of fish farming" (In Portuguese: Art. 2º - *Serão reservadas áreas com a vegetação que, a critério dos técnicos, for considerada necessária à proteção da Ictiofauna e das reservas indispensáveis à garantia da piscicultura*). This Article is mentioned in a single paragraph of the Commentary, but on a trunked way (see ahead). In addition, highlighting the Sinop Reservoir as an example of non-compliance with the law in Brazil is difficult to understand. In fact, the paragraph contains two misunderstandings: the illegality of partial removal of vegetation, and the emphasis given to the Sinop Reservoir, when this practice is extended to all large Brazilian reservoirs.

In the sequence, the Commentary attributes the death of the fish downstream the Sinop Dam to hypoxia generated by the flooding of the not removed forest ("graveyard of dead trees") in the reservoir area, which occurred four days before the beginning of the reservoir filling. To support his statement, he relied on only three measurements of dissolved oxygen, without detailing the time, location and depth of the measurements. The conclusion articulated by the author about the cause of fish mortality is, in my opinion, hasty and not supported by reality. In the same day of the fish death, high levels of oxygen concentration were recorded downstream the dam by Aqunálise (a consultancy firm contracted by the power company but with a technical and legal responsibility on the quality of the data and results). In addition, it is not possible that anoxia conditions settle in the reservoir body and be exported downstream the spillways, in only three or four days after filling began. Previous studies show that conditions of

anoxia require at least 15 days to be installed (see page 15 of the Technical Note on ichthyofauna which link is provided in the published commentary). Furthermore, deaths by anoxia do not promote excoriations, lacerations, exophthalmos and hemorrhages, as pointed out by necropsies. I believe that this was the most surprising point of the Commentary, since his conclusion on what caused fish mortality did not even take into consideration the necropsy data. Also, on February 4, 2019, when the first dead fish were observed and the mortality event was the most severe, the measurements performed by the Aquanálise revealed waters with oxygen saturation levels above 119%. The logic of a specialist would lead to consider as possible causes of fish deaths the gas supersaturation, water turbulence or even decompression when passing through the spillway and channel of dissipation, but never problems with anoxia generated in the reservoir, even because there were no problems of anoxia or death of fish in the reservoir area. To consider hypoxia/anoxia as the cause of death for Amazonian fish would only be reasonable if there were no scape areas. Lower values of dissolved oxygen, including anoxia, are common in several Amazonian biotopes. The specialized literature reports this frequently. In fact, it is a feature incorporated in the life strategies of many Amazonian fish species (see, for example, Almeida-Val, VMF, Gomes, ARC, & Lopes, NP (2005) of the Amazon Fish Physiology, 21, 443-500).

In summary, the facts that lead one to refute the conclusions presented in the aforementioned commentary are based (i) on the necropsy data; (ii) the oxygen supersaturation conditions downstream the dam when the first fish deaths were observed; (iii) the necessary temporal lag, described in the literature, for the process of decomposition of the vegetation to promote hypoxia or anoxia in the waters of the reservoir body and that water spills over downstream of the dam; (iv) absence of reports of fish mortality in the reservoir; (v) the concentrations of oxygen in the main body of the reservoir, at the period of fish death, were compatible with the presence of healthy fish, which eliminates the possibility of downstream anoxic currents.

Yet, in the topic “Fish die off”, the author tries to corroborate his arguments with the events of mortality downstream Estreito, Teles Pires and Colider dams. In fact, the mortality that happened in these dams did not have any relationship with hypoxia or anoxia resulted from the submerged vegetation. All these mortalities were proved to be related with other factors, such as turbines and the phases of construction.

In the topic “Legislation on removal of vegetation” is erroneously mentioned that the Law 3824/60 “**allows leaving vegetation that is necessary for the life of fish**”. In fact, the law determines that this should be done as aforementioned (Art. 2º - *Serão reservadas áreas com a vegetação que, a critério dos técnicos, for considerada necessária à proteção da Ictiofauna...*). In despite of this not be my specialty, I believe that modelling is the most accepted scientific tool to determine the amount of vegetation that should be removed. I do not see another method that technically meets the demand implied in Article 2º of the Law.

Despite of the recognition of the Commentary, in the topic “Effects of dead forest in reservoirs”, that the problems related to hypoxia are a result of the decomposition of labile material (leaves, shrubs, sprouts, etc.), he does not distinguish them from the ones related to arboreal vegetation. Unfortunately, besides the lack of this distinction, the most overwhelming critic (also included in the illustrations of the Commentary) has as target the submerged trunks (“tree graveyard”). In fact, submerged trunks can take more than a century to decompose and their effect to the carbon release rate was not even commented. Why it was not mentioned the problem of the vegetation regrowth (sprout) and the development of herbaceous plants in the area where arboreal vegetations were removed, due to delays in the filling of the reservoir? In

addition, it is important to emphasize that the reservoir was only in its fourth day of the filling process, which, as already mentioned, would not be sufficient time for the decomposition process to result in anoxia in the reservoir body. In support of this argument, I highlight the results of water quality measures taken during monitoring activities, which showed that the oxygen level of the water in the central axis of the reservoir, close the dam (water that is channeled to pour downstream) was higher than 5mg/L in the main days of fish deaths. This completely precludes the hypothesis of a possible anoxia as the cause of death of the individuals. Although the suggested reasons for a greater fish biomass in areas with dead tree may be object of discussions (after all, this was not the objective the EO), it is not possible to deny that structured environments harbor more fish biomass, as the Commentary does in the topic "Exaggerated benefits of dead wood as a shelter for fish", without any scientific basis, citation or previous experience in the theme. The published paper about the relevance of submerged trunks does not restrict itself to Mourão Reservoir, but also includes results from Itaipu Reservoir and lots of relevant references on the theme. The consumption of periphyton by several fish species in initial stages of development and also by some adults (prochilodontidae), in varzea areas or even igarapes is recognized in the literature. It is elementary that, to contest published results (peer-reviewed), the arguments should be based on specific studies. Although the experience of the author deserve respect in other areas of conservation, I cannot accept his assertives on the conservation of the aquatic biota as facts, unless they are proved. Comparisons with Balbina, by the odd character of the impacts of this disastrous reservoir, seems more figure of rhetoric, similar to that one used to compare what happened in Sinop with the tragedy in Brumadinho. Thus, inferences on the rigor of the heterotrophic phase, in any Brazilian Reservoir, from what happened in Balbina, are more than exaggerations, they are absurd. Despite the area, hydraulic retention time and depth of Balbina Reservoir, this damming did not consider any modeling on water quality. It is important to stress, moreover, that water quality modeling was precarious when Balbina Dam was constructed. In fact, the generalization made in the commentary, as an attempt to deconstruct our arguments on the importance of subaquatic structure, is quite superficial and inappropriate, since our recommendations, in the EO criticized, are that the amount of vegetation kept would not relevantly compromise water quality. I strongly suggest reading our EO entirely.

Finally, in relation to the perception that the author has on consultants, I do not believe that it is worth any additional comments, once his understanding on this matter appears to be based on personal experiences. I emphasize, however, that I do not see merit or demerit in charging for work performed. As a retired employee of the public service, I develop academic work regularly (research, orientations and teaching) and voluntary (without remuneration) for almost five years, besides never having charged opinion reports, evaluations, talks and collaborations from public entities, including Public Prosecution. I simply see this as a pleasurable obligation to spread and apply the knowledge acquired from researches, readings and experiments conducted during my academic career, paid by public or private financing agencies.