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Interactive comment on "Methane emissions from floodplains in the Amazon Basin: towards a process-based model for global applications" *by* B. Ringeval et al.

Anonymous Referee #2

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This paper attempts to develop and evaluate a process-based model for tropical floodplain CH_4 emissions. The authors develop a regional-scale version of LPX that includes representations of floodplain hydrology, vegetation, and CH_4 mechanisms. They forthrightly discuss shortcomings in (1) components of their approach and (2) observations necessary to properly confirm model formulations. They also acknowledge that they were unable to reduce uncertainty in wetland Amazonian CH_4 emission estimates with their modeling approach.

I agree with their conclusion that process models have a very long way to go before they can be relied on for accurate (or even reasonable) regional CH_4 emission estimates in the tropics. I think the paper could be published if important improvements in the model were made and a more thorough analysis of their impacts was performed (described below). In particular, the authors need to account for seasonal inundation, macrophyte vegetation, lateral O2 transport, and plant inundation stress before the paper should be published.

Even after these processes are included, the model will have large uncertainty. Given these uncertainties, I would like to see the authors change the paper's focus from development of yet another 'process-based' model to a description of the observational and experimental work necessary to mechanistically represent the large range of processes represented. Honestly, all the relationships applied in equations 1, 2, 3, and 5 to account for various processes are arbitrary and not buttressed by comparisons to observations of those particular mechanisms. Different, yet equally reasonable, choices for these formulations would give different model predictions, but given other uncertainties in the model and observations it would be impossible to distinguish which formulation was more correct. Representations for other processes are also either missing from the model or very uncertain. Give these issues, I suggest the paper title be changed to something like: 'Large uncertainties in tropical flood plain CH4 emission predictions: Challenges in developing a process-based model for global applications'

Below are issues that I would like to see the authors address before the paper is published:

1. (p. 16714; lines 17-20; page 16750, lines 12-16) How is it possible to conclude that LPX simulated both (a) reasonable agreements with observations at the field scale and (b) poor agreement with between-site variations or between-year variations within a site? Those appear to be contradictory statements.

2. (p. 16718; line 8) I don't think many of the WETCHIMP models were actual DGVMs. Most of those models had static vegetation distributions, although plant physiological stresses were dynamically calculated. Perhaps a definition of what you mean by DGVM is appropriate here.

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3. (p. 16721; line 20) You mention that N and P limitations on vegetation are not included, and then use a scaling factor on NPP to account for mismatches with the uncertain MODIS NPP estimates. However, soil N and P content also impact redox and therefore CH4 production. Please discuss this problem and your estimate of its importance and how these dynamics could be integrated in future model formulations.

4. The lack of seasonality in wetland extent seems to be a primary problem with the model formulation, not only for establishing redox conditions for CH4 production, but for dynamics of C inputs belowground, longer-term plant dynamics, and SOM dynamics (including respiration). You say that this limitation exists because the current LPX version does not allow an update of LU area fraction more than once a year. However, given its importance, I think you need to make this change in the model and examine the impacts, particularly given the opposing effects of flooding depth and wetland extent (which you mention).

5. Equations 1 and 2 seem completely arbitrary. Can you indicate why you think this approach is reasonable? Also, you need to compare predictions with these equation again observations, even if they are from non-Amazonian systems.

6. Equation 3 (impact of anoxia on plant processes), and the approach described in lines 6-11 on page 16729, are arbitrary in the absence of any mechanistic explanation or observations for constraint. If there are no data to constrain these approaches, you need to explicitly state that, and if these formulations are based on observations you should provide a comparison with those observations. If there are neither, you need to develop an approach that is mechanistically testable against observations, integrate it, and test it in the model. Also, you should describe how the uncertainty in these model formulations propagates to your site and regional CH4 emission estimates.

7. Page 16731, Line 16-17. You state that the spin-up is performed in the absence of inter-annual variability (IAV) in floodplain extent. But that means that your equilibrium vegetation state is out of equilibrium with the observed IAV, and I would expect

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substantial transients in model predictions associated with this problem. You should perform a spin-up with the IAV included and indicate the impact on your predictions of NPP, soil organic matter, respiration, and CH4 emissions.

8. What is the 'mean y/x ratio' mentioned on page 16744, line 21?

9. You mention incompatibility of LPX and observational spatial scales on page 16747, lines 3-5. Can you not run the model at the site scale? If you can't run the model at the site scale with realistic inputs, than you can't argue that the different spatial scales are the reason for a poor match between the model predictions and observations. The reason could just as easily be a poor model formulation.

10. If, as Wassmann et al. (1992) argue, lateral water flow and O2 transport are important controls, your model needs to include some representation of them. Otherwise, it's impossible to know why the model does not match the fluxes during the period of rising water. If these fluxes are an important component of the cumulative flux, having a representation of rising water levels, without the concurrent impact on O2, is likely to lead to incorrect inferences regarding water level impacts on CH4 emissions.

11. Floating macrophytes are critical components of these systems, yet you haven't included them in the model. You say on page 16729, line 20, that you will examine sensitivity to them, but I could not find where, or how, you did that sensitivity analysis. You do mention later how much larger CH4 fluxes are from macrophytes than forests in the observations. Give their importance, I think you need to include in the model at least a first-cut attempt to represent their impacts.

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