

Answer to Anonymous Referee #2

We wish to thank the referee #2 for her/his time and care in providing comments on our manuscript. We provide detailed answers below (answers are in bold) as well as a modified version of the manuscript (see suppl. document):

This paper attempts to develop and evaluate a process-based model for tropical floodplain CH₄ emissions. The authors develop a regional-scale version of LPX that includes representations of floodplain hydrology, vegetation, and CH₄ 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 CH₄ 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 CH₄ 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. Given 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'

In the paper, our strategy is

- i) to apply a model that is commonly used at global scale for specific conditions encountered in Amazon basin (by implementing the absolutely required modifications, e.g. floodplain extent),**
- ii) to evaluate the model performance against available observations & to estimate the sensitivity to different uncertain processes**
- iii) to propose recommendations towards more accurate estimates of Amazon CH₄ emissions.**

i) So far progress in regional modelling of CH₄ from tropical floodplains has been very limited, mainly because tropical wetlands were treated as if they were similar to any wetland. However, floodplain ecosystems are different in many aspects from the graminoid dominated wetlands dominant at other locations around the globe. This is why tropical floodplains remain the most important hurdle in modelling global CH₄ emissions that has not been taken yet. We aimed to take a first order approximation of floodplain' behaviour within the framework of land surface models.

For this purpose, we used LPX which is representative of land surface models applied to model global CH₄ emissions (see the WETCHIMP intercomparison). We explicitly aimed to work within the framework of DGVMs, instead of developing an entire new model, to allow

incorporation of our concepts to other DGVMs too and to allow evaluation of tropical CH₄ emissions relative to other wetlands within the same consistent framework. This approach also has restrictions as it forced us to leave the overall structure of LPX intact and focus on making LPX appropriate for tropical floodplains (e.g. prescribe the floodplain extent as Land Unit), adding complexity as needed. On the other hand it also allowed us to identify the current missing processes/limitations of common land surface models for application in tropical conditions (e.g. yearly static floodplain extent) and to propose solutions to these issues to further improve the model (see below). The implementation of both floodplain extents and associated conditions for vegetation in a DGVM context is the first major step to be taken. All refinements thereof are secondary to this and that is why we consider this work an important step forward.

We are convinced that process models are needed to synthesize and quantify what we know about the system under study, now and for a future climate. At the same time, all models including ours simplify various processes. We agree with the reviewer that in the end descriptions of seasonal inundation, macrophyte vegetation, lateral O₂ transport etc. need additional refinement in order to obtain more accurate estimates of Amazon emissions. A more realistic model of tropical wetland specificities is the direction in which we are heading. However, these refinements are a big step that cannot be dealt with at once as the challenge of translating field scale knowledge into a general representation for application to regional scales is major. This is something we acknowledged in the paper (e.g. the initial title contains «towards a process based model») and identifying and discussing those additional refinements is an important aim of our revised paper.

ii & iii) We thus agree with the reviewer to focus the paper more on those uncertainties and challenges (which, for the first time, could be evaluated to some extent given that we tackled the first order problem: representing floodplain extents and associated conditions for vegetation). We think that the added value of our work is to provide emissions sensitivity to uncertain processes. Both such sensitivity tests and comparison to observations allowed making recommendations about the challenges of obtaining more accurate estimates of Amazon CH₄ emissions :

→ first, we need to further constrain the floodplain CH₄ emissions and their temporal variability, and

→ then, we need to implement additional processes in the model, including the ones recommended by the reviewer.

It is something we partly underlined in the submitted manuscript: p16757; L15 : « The advanced and more explicit treatment of floodplains also introduces additional uncertainties about hydrology, vegetation and associated CH₄ emissions into the model. However, we estimated the CH₄ emissions' sensitivity to different processes and could identify which processes are critical for a successful bottom-up estimate of CH₄ emissions from tropical wetlands. »

To deal with the reviewer's comments , we modified the manuscript by :

- modifying the title following the reviewer's suggestion
- modifying the abstract and introduction to clarify the fact that our study is the 1st step in the procedure and that we aimed to evaluate challenges once floodplain extent and associated conditions for vegetations are represented
- re-writing the discussion/conclusion to more clearly express these challenges: first, describe the observational and experimental work necessary to mechanistically represent the large range of processes represented (as suggested by the reviewer); second, reduce the uncertainties in the description of the processes to better constrain the Amazon CH₄

emissions; third, implement new processes.
Please, look at the modifications in the supplementary document.

With respect to the equations: we agree with the referee to make the choice of the formulations clearer. As mentioned in (i), our aim was first to work within the LPX framework. That is why the chosen formulations are as much similar as possible to the Wania et al. (2010) study (e.g. vegetation stress in Eq.3). In addition, we tried to estimate the sensitivity to newly introduced formulations (Eq. 1 and 2 that simulate the same variable – flood depth). There is no observational information about the flood depth at large spatial scales. However, we compared the simulated flood depth to information available on sites (Fig A4 for Eq. 1 and 2). Eq.5 has been introduced to relate anoxia to the oxygen concentration (and not to the air fraction alone), which is more in agreement with basic knowledge about methanogenesis (Conrad et al., 1989). We clarified the rationale of these equations and their limitations in the revised version of the manuscript. See also our detailed answers below.

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.

We meant that the mean emission across all sites is well captured but not the variability between sites. Likewise, the mean emission across all years was captured well, but not the year-to-year variability. We revised the abstract to clarify this (and removed « reasonable agreement »).

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.

It is true that not all models participating to the WETCHIMP intercomparison simulate vegetation dynamically (but please note that LPX does). To prevent any confusion, we used « Land Surface Model » instead of DGVM.

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.

We agree with the reviewer that soil N and P content has an effect on redox potential that, in turn, impacts the CH₄ production. However, accounting for such effect is not feasible at this stage.

Redox potential is not explicitly represented in LPX nor in the majority of global models (only few models account for effect of redox potential on methanogenesis at global scale and this representation is very rough (e.g. Zhuang et al., GBC, 2004) and certainly do not account for subtle soil chemistry aspects like soil N-P) In LPX, redox conditions required to methanogenesis are assumed to be reached when air fraction and [O₂] are low enough.

Soil nitrogen and phosphorus may impact CH₄ emissions in various ways (in addition to the impacts on NPP). 1. They determine litter decomposition and soil carbon mineralisation rates

and therefore the supply of carbon for methanogenesis (Pancotto et al. 2010). This effect is implicitly present in the parametrization of carbon mineralisation, although its effect is likely to vary among sites and regions. 2. Denitrification (N) and P-Fe-Ca-CO₃ dynamics will affect redox directly and therewith methanogenesis. 3. Nitrogen availability is known to affect CH₄ oxidation rates (e.g. Bodelier et al. 2000). None of these three effects are currently implemented in any of the global models and demand major developments, not only to have interacting N and C cycles, but also to improve the soil chemistry as well as the methanogenesis and CH₄ oxidation modules. We discuss these challenges in the revised discussion section.

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).

We agree with the reviewer about the necessity to account for seasonality of floodplain extent for realistic simulation of both C cycle and characteristics allowing CH₄ production (required anaerobic conditions). Indeed, the seasonality in floodplain extents could have an effect on CH₄ emissions either directly (through a change in emitting areas) or indirectly (through a change in plant distribution, productivity that in turns could modify the amount of methanogenesis substrate). As mentioned in previous answers, accounting for seasonality of floodplain extent is a direction in which we are heading, but it was not feasible at this stage.

The incorporation of a (static) floodplain extent was already a substantial modification, in which we aimed to treat the carbon balance to be specific to and consistent with wetlands (given its major impact on methanogenesis). As explained at p16757; L7, « the carbon balance of soils is treated independently for wetlands and uplands. », which is different to the strategy used in most WETCHIMP models where «the wetland extent can occupy a fraction of the grid-cell, but there is no subgrid treatment of the carbon cycle fluxes. Thus, inundation has no effect on vegetation, carbon pools and heterotrophic respiration. Instead the mean value of the heterotrophic respiration over the entire grid-cell is used to compute the CH₄ flux density (see Melton et al., 2013 ; Ringeval et al., 2013). ». These modifications/improvements had to be evaluated first.

Although we were not able to quantitatively incorporate seasonality in floodplain extent (given the major investments needed for this), we evaluated the direct effects of seasonality on CH₄ emissions through the sensitivity test described at p16755; L19 (orange curve in panel 11b). We clarified the description of this sensitivity test in the revised version of the manuscript. In addition, we provided a perspective on how to better represent seasonality in floodplain extent in the discussion. We imagine that extending the modifications we made in the model to multiple floodplain categories will be the next step of the procedure towards a realistic process-based model.

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.

As mentioned in the answer to general comments, rational of these equations and their limitations were clarified in the text and we compared the simulated flood depth to

information available on sites (Fig A4).

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.

Equation 3 is an empirical relation to mimic the influence of inundation stress on vegetation productivity and distribution. The primary objective is to bring these properties into a realistic range, rather than to describe them fully mechanistically. Eq. 3 is equal to a formulation applied by Wania et al (2009a) for peatlands and was applied to be consistent with those formulations.

Unfortunately, there are no data available that are general enough to describe how flood duration and flooding depth affect productivity for PFTs in tropical floodplains. Therefore, the parameter values were optimised by comparing NPP against observations (but we underlined that there is no consensus between site observations and MODIS) or vegetation distribution against GLC2000. Both aspects have been clarified in the revised version of the manuscript.

The parametrization is very simple and an alternative would be to modify the intrinsic photosynthesis properties of grass and tree. However,

i) there is no evidence of difference in $v_{\text{cmax}}/v_{\text{jmax}}$ between flood and non-flood tolerant plants that is supported by observations

ii) the representation of photosynthesis in LPX is not different for grass and trees and could not lead to modification of vegetation distribution (grass contribution to total vegetation cover) : p16720;L28 :« Contrary to most of the commonly used DGVMs (e.g., see Krinner et al., 2005 for the ORCHIDEE model), there are no PFT-specific parameters for the optimal maximum rubisco-limited potential photosynthetic capacity (v_{cmax}) and the potential rate of Ribulosis-1,5-bisphosphate (RuBP) regeneration (v_{jmax}). This has an effect on our strategy to model flood tolerance for the newly introduced PFTs. ».

We agree that improving the representation of flooding stress in next iteration of this work is important and we added a discussion about that in Section 4.

Finally, the influence of the simulated inundation stress on regional CH₄ emissions was investigated through sensitivity tests in which the PFT specific parameters were varied (Simulation 1 and 2 in Table 5 ; p16740;L15 « The aim is to estimate the CH₄ emission sensitivity to (...) the parameterization of vegetation...»). We clarified the main text and Table 5.

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 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.

All simulations, except regional simulation 7, were performed without IAV in floodplain

extents during both the spin-up and the transient simulation. Simulation 7 was performed as follows : spin-up and 1932-1978 transient simulation are performed without IAV in wetland extents. Thus, up to 1978, the procedure is similar to other simulations. But, from 1979 onwards, IAV in floodplain extent is introduced in simulation 7. We recognize that this procedure introduces some perturbation at the beginning of 1979. In fact, the modifications implemented in LPX to simulate floodplains (flood stress, mortality) alter the forest characteristics: e.g. floodplain forests are characterized by a large number of small trees while non-floodplain forests consist of a smaller number of larger trees. Due to these differences, any conversion from one LU to the other leads to an increase in mortality of tree population. E.g., this happens for flood-tolerant PFTs that experience stress on the classical “natural” LU when floodplain extent shrinks. Thus, equilibrium reached in 1978 for both floodplains and non-floodplains are perturbed when IAV in wetland extent is introduced.

However:

- the perturbation is strongly softened over the study period. In fact, only the 1993-2004 period of simulation 7 is used in our study (because it is the period on which WETCHIMP outputs are available; Table 7). Over 1979-1993, soilcarbon of floodplain decreased ~0,9%/yr while it decreases only of 0.2%/yr over the 1993-2009 period. Over non-floodplain LU, new equilibrium is almost reached in 1993. Accounting for the fact that some tree species can adapt to both flooded and non-flooded conditions would lead to decrease the perturbation introduced in 1979 and is already discussed in the first draft version (p16758; L20).
- PCR-GLOBWB outputs are available only for the 1979-2009 and it does not make sense to use them for the spin-up of simulation 7 given that floodplain extent is coupled to climate and decoupling them in the spin-up would cause other problems that are likely bigger than the problems associated with the behaviour in 1979..

We clarified by putting together all informations in relation to the LPX spin-up at section 2.6.2. Specificities of simulation 7 are now given in section Sect. 2.6.3 “Simulations for the Amazon Basin”. Some sentences about the perturbation introduced in 1979 have been added in the same section.

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

We rewrote the whole sentence :

“Different parameter settings, even when reproducing the same y/x ratio averaged over the Amazon basin, lead to very different spatial patterns at smaller scales.”

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.

There are an incompatibility between :

- the spatial scale of input data (climate) used to force LPX: half-degree resolution
- the spatial scale of remote sensing product used to evaluate intermediary variables such as NPP or vegetation cover (MODIS, GLC2000): ~0°0'32, and
- the spatial coverage of flux chamber measurements used to evaluate the simulated CH₄ flux densities: (0.2m²) (and even smaller for some ebullition measurements).

We clarified the sentence on p16747; L3: “The evaluation of LPX CH₄ emissions with site data is hampered by the scarcity of available measurements as well as by the mismatch between the

spatial coverage of such measurements and the spatial resolution of Land Surface Models” and discussed that in Section 4.

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.

We replied to comments 10 and 11 simultaneously ; please see below.

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.

Our final aim is to implement processes as lateral flux or macrophytes that could make the model more realistic. But as mentioned in the answer to the general comments, we think these implementations are beyond the scope of the paper. Instead, we chose to more extensively discuss the uncertainties and challenges.

The comparison with observations we performed and in particular the difference in the model performance found between the Negro River basin and the Amazon main stem suggests that the implementation of lateral water flow and macrophytes could be important. In the revised version of the manuscript, we discuss the ways to implement missing processes in LPX. But before all, our results stress the need for more research to constrain floodplain CH₄ emissions and their temporal variability, even before including other fundamental mechanisms as floating macrophytes or lateral water fluxes.

We performed preliminary tests to evaluate the effect of a few macrophytes characteristics, namely the absence of both plant-mediated transport and root exudate production in soil. This is mentioned at p16740; L7: “For sites 4 and 5, the “optimal” simulation aims at evaluating the sensitivity of the simulated CH₄ flux densities to some floating macrophyte properties, namely a suppression of the transport by plants and the fraction of NPP going to exudates set to 0. »

Regarding Amazon basin simulations, this was mentioned at p16740; L17 : « The aim is to estimate the CH₄ emission sensitivity to (...) macrophyte properties (last column of Table 5). »

We clarified the section dealing with these sensitivity tests at p16729; L20.