Biogeosciences Discussions

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# Comment on bg-2022-2

Joe Melton (Referee)

Referee comment on "Evaluation of wetland $CH_4$ in the Joint UK Land Environment Simulator (JULES) land surface model using satellite observations" by Robert J. Parker et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-2-RC1, 2022

Parker and coauthors investigate the performance of the JULES land surface model for simulating wetland methane emissions with particular attention paid to poorly simulated African wetland regions. They run several different model setups along with different model forcings. The JULES estimated methane emissions are evaluated against atmospheric CH4 retrievals such as GOSAT/TOMCAT. I found this paper to be an enjoyable read and congratulate the authors on that. It was well-written, the arguments were sensible and well-laid out and I was able to easily follow the storyline. I appreciated the work with CaMa-Flood as that is a nice attempt to address a difficulty that land surface models have with wetlands (how to get the water to a location without it needing to fall from the sky in that grid cell). I think this paper is good for publication with only some minor revisions based upon my comments below.

Major comments:

From how I read the paper, there is a dependence upon accurate anthropogenic/other natural/fire CH4 emissions for the attribution to wetlands from the GOSAT/TOMCAT retrievals. It appeared to me that those non-wetland sources were assumed to be perfect (along with the atmospheric inversions). I would have liked to see some attempt to understand how reasonable these other CH4 source estimates were as all error terms were then pushed into the wetland methane emissions. I think it could be worthwhile to check on how much this included error affects the evaluation, perhaps byby some sensitivity tests. For example, changing the source strenghts of the other sources and checking if the wetland source distribution remains stable and doesn't change appreciably in location or strength. At the very least, please include some discussion on the impact of these assumptions, what errors this could be ignoring, and how it may change the evaluation.

Minor comments:

- Line 78: 'A deep layer of restrictive water flow' - does that just mean that you provide a no flow condition at 3 m?
- L109: Why is the time series scaled to 180 in particular? Why is this step necessary or desired?
- Fig 2: Do all of those in the grid actually give 180 Tg/yr in 2000? Ones like the bottom left seem to hardly be able to (although I realize the time shown is Aug 2011)
- L 137: When a single C pool is used does that mean both the litter and soil (humified) C are tracked in only one pool?
- L 150: Why use the SWAMPS dataset by itself, with its known inability to detect saturated, but not inundated, wetlands, and not make use of something like WAD2M? I see you use WAD2M later so are definitely aware of it.
- line 179 - fix ref.
- Fig 4 - what are the units?
- L 320 - WAD2M uses more than microwave remote sensing. Perhaps give a bit more detail here otherwise it sounds like it is just SWAMPS (which does form the seasonality but there are other important differences)
- Fig 12 - missing reference at end? (Fig: boxplot)?
- Line 492 - chimney venting? Is this aerenchymal transport that is meant?
- Code availability - user account required limits reviewers ability to check over code (should they wish to remain anonymous).
- L 532- doesn't quite make sense. Needs rewording.