Author Comments

Colour Key: Reviewer Comment Our Response New Manuscript Text

Reviewer 1

We thank the reviewer for their comments and appreciate them taking the time to review our study. After this second round of reviews, we thank the reviewer for accepting the manuscript as is.

We would still like to address one issue as we believe that they were correct to raise it but it's something that we unfortunately cannot address at this stage.

Reviewer 1 and the Editor have both made the point, which we absolutely agree with, that the code should be in a more freely available location without requiring registration. Unfortunately, due to the current license of the JULES model, we are unable to put neither the full code nor key modules into any sort of openly available repository as the licensing terms prohibit this. Efforts are underway to improve this situation and GMD recently agreed that the JULES repository is an acceptable code source (see e.g. https://gmd.copernicus.org/preprints/gmd-2022-139/).

Reviewer 2

We thank the review for their comments, in taking part in this round of reviews and for recommending publication subject to the amendments below.

RE: Emission scaling. Wouldn't this scaling procedure needed to be done only for the ensemble members that were masked with SWAMPS? And not in each ensemble member. This is unclear and needs to be indicated more precisely or the added paragraph need to be moved to section 2.3.4

We agree that this is unclear and have updated the text as follows:

All JULES simulations, regardless of whether they have been further constrained with a wetland mask or not, are scaled such that the global total wetland methane annual emissions for 2000 has a value of 180 Tg/yr to ensure consistency with the best estimate of this value from Saunois et al. (2016).

Also, it will be good to add a quantity to the expression "unrealistically low methane emissions" to give further validity to this approach in this particular set up.

We have added clarification to the text on what we mean here and agree that this was poor phrasing on our part, The scaling is most important when applied to the SWAMPS-based ensemble members as the geographic masking of the JULES wetland area with the SWAMPS data would otherwise result in reduced global emissions, below a level consistent with Saunois et al. (2016).

RE: Figure 5 The color bar indicates the correlation coefficient between these two data sets, but: how is it possible to obtain a higher correlation coefficient in areas where the difference between the data sets is larger and with higher uncertainty (given by the whiskers in the bars)?

We will try to make this clearer in the text with the following explanation:

The bars indicate the difference in the **amplitude** of the seasonal cycle for each year. The colours indicate the correlation coefficient of the time series. It is entirely possible to have highly-correlated time series where the amplitude of the signal is different (e.g. two perfectly in-sync cycles but one with double the amplitude of the other). This is the case for e.g. Pantanal where the seasonality between JULES and GOSAT matches well, but the amplitude of the seasonal cycle is much larger in GOSAT than JULES. Conversely, there are regions where the maximum amplitude difference is small but the data is out of phase leading to a poor correlation, e.g. Indonesia.

Also, it should be indicated why this time shift between the two data sets is happening.

It's not so much a time shift as just a poor correlation. Section 5 is intended to explore these cases in detail, but we will add a sentence to the start of this section to make this clearer.

These regions were selected as they were found to exhibit particularly poor correlation coefficients between GOSAT and JULES, suggesting issues with the timing of the seasonal cycle.

According to NCAR, WFDEI is only available until December 2016 (https://rda.ucar.edu/datasets/ds314.2/), and the simulations using JULES in this manuscript were done until 2017, how the authors obtained forcing year for 2017?

WFDEI data has subsequently been extended beyond the dataset above (i.e. 2017 data was available).

What recommendations can the authors provide for further studies that require improved wetland extent in simulations for years 2017 onwards? A test with ERA5 which has an improved precipitation field compared to ERAInterim might be a way forward.

We have also added in the following statement

We would expect our conclusions regarding the strong performance of WFDEI meteorology to also apply to the updated WFDE5 data (based on ERA-5), detailed in Cucchi et al., 2020. Future will work assess simulations driven by these inputs.

Cucchi, M., Weedon, G. P., Amici, A., Bellouin, N., Lange, S., Müller Schmied, H., Hersbach, H., and Buontempo, C.: WFDE5: bias-adjusted ERA5 reanalysis data for impact studies, Earth Syst. Sci. Data, 12, 2097–2120, https://doi.org/10.5194/essd-12-2097-2020, 2020.

Figure 1 – in row 3 replace the expression “q10” by “Q10”

Thanks, noted and changed to Q_{10}.

Section 2.3.3 - Will be good to explicitly know why 3.7 and 5.0 were used as Q10 values

These values were chosen based on the work of Gedney et al. (2019) who tested values of 3, 3.7 and 4.7 as low/middle/upper estimates, themselves based on Turetsky et al. (2014).

We have clarified this in the text.

We chose Q_{10} values of 3.7 and 5, based on the work of Gedney et al. (2019) who tested values of 3, 3.7 and 4.7 as low/middle/upper estimates, themselves based on Turetsky et al. (2014).