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Interactive comment on "Using satellite data to identify the methane emission controls of South Sudan's wetlands" *by* Sudhanshu Pandey et al.

Anonymous Referee #2

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Summary and general comments:

Pandey et al. analyse recent observations of XCH4 over South Sudan to infer wetland emissions resolved at the seasonal time scale. They compare their findings with different process-based wetland models and report significantly higher emission estimates using a mass-balance approach. With additional data and assumptions they infer that inundation extend and or temperature sensitivity are a likely cause of this top-down versus bottom-up discrepancy and suggest that satellite studies, such as this, are useful tools for studying remote or hard to access areas. The paper is very well written, clearly structured and easy to read. The topic is of significant interest to the wider community and is a nice case study of potential ways to exploit TROPOMI data. However, there are two key issues (below) the authors need to address before this study should be considered for publication in Biogeosciences.

1.) Methane emissions from ruminants in South Sudan

The authors use an emission estimate from EDGAR V4.3.2 of 0.36 Tg yr-1. Firstly, there is no real discussion of uncertainties of this number, although studies in other countries have shown that significant differences between top-down and bottom-up based estimates exist, even in countries with long-standing dedicated infrastructure for tracking livestock head counts, which might not be the case for a more recently formed country, such as South Sudan. For example, Miller et al. 2013 (https://www.pnas.org/content/110/50/20018) have claimed that agricultural emissions are severely underestimated in the US. As the proposed domain nearly covers all of South Sudan we can try a back of the envelope calculation on what national ruminant emissions could be. According to an FAO webpage (not the ideal source, http://www.fao.org/emergencies/fao-in-action/stories/stories-detail/en/c/326186/) there are 12M cattle, 20M sheep and 25M goats in South Sudan Assuming: 5 kgCH4 yr-1 for goats&sheep and 35kCH4 yr-1 for cattle in developing countries this yields ca. 0.65Tg CH4, so nearly twice the EDGAR V4.3.2 estimate. Emission factors from https://www.tandfonline.com/doi/pdf/10.3402/tellusb.v38i3-4.15135 It seems logical to address this issue in more detail in section 3.2.

2.) Discussion of impacts of seasonally changing spatial coverage and/or clear sky bias of observations.

The authors should further expand on the issue of data coverage and (potential?) clear-sky bias in the manuscript. Is this a significant source of uncertainty and how was this accounted for. The seasonal decrease in emissions in JJA coincides with a significant drop in data coverage. Emissions in JJA-2019 are reported to be miniscule (statistically close to 0), but this is not appropriately addressed in the manuscript. What is happening here? Assuming that agricultural and other methane sources are still active in JJA-2019 this result is even more extraordinary.

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Discussion paper

Specific comments:

Line 54: Please clarify: 38-56% higher than which RCP? They differ quite significantly in their anthropogenic emissions.

Line 105: Why do you choose to use Wetcharts data for 2009-2010 as basis for comparison here, although previous work you referenced (line 70) has already shown a strong trend in CH4 emissions in the Sudd wetlands after 2010, due to IE expansion?

L109: The wetland extent datasets (Lehner and Doell 2004, and Bontemps et al. 2011) seem not to be very up to date and able to include any trends happening after 2010.

L170: What is the temporal resolution of the meteo data set used here? Are the wind speeds from these 4 consecutive hours really independent or is the model only constrained at lower frequency intervals? This could lead to an artificially low variability. Are there any surface observations in the (wider) region to compare with the model?

Line 233: Why are emissions now 7+/-3.2 Tg yr-1 and not 7.2+/-3.2 Tg yr-1 ?

Line 239: Why is the TROPOMI-based wetland CH4 emission estimate compared to a minor source such as oil and gas emissions (0.05 Tg yr-1) here? The biggest anthropogenic source in the region, even according to EDGAR, is agriculture at 0.36 Tg yr-1, which is an order of magnitude bigger than O&G (possibly bigger, see general comments).

Line 246: Please correct to 'nearly an order of magnitude lower'.

Line 252: It seems the upper end of the WetCharts ensemble is not that far off the lower end of your estimate here, so claiming they differ by an order of magnitude seems unhelpful.

Line 287: JJA-2019 seems extremely low, this needs to be discussed further (see general comments).

Line 297: It is unclear if TROPOMI-derived CH4 emission estimates really track river water heights all that well or if the effect is mostly Q10 related. Maybe those two components should not be singled out or it should be made clear early in the manuscript that both components contribute.

Line 316: Temperatures are surely lower in JJA, but not low enough to explain the emissions reported for JJA-2019.

Line 362: Some models report emissions that are definitely NOT ‘an order of magnitude’ smaller.

Line 365: One model ensemble member with Q10 = 3 estimates 3.7 Tg yr-1 despite the ‘poor IE estimates’. So it seems this study cannot disentangle the two issues, which should be reflected in the conclusion here. Furthermore, after reading this manuscript it seems more convincing that Q10 is the key issue rather than IE.

Table 1: Please elaborate what the variable ‘data coverage’ signifies here. For example, for DJF-2018: is it 91% of all cells were covered at least once in this 3 month period or were 91% of all possible data collected for an average cell. For example, if you measure every cell at least once in DJF-2018 would you label this as 100% coverage (which it is not) or do you need to measure all cells, all of the time (once per day) to reach 100% coverage?

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