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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.

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We thank the anonymous referee for his time and useful feedback, which helped further improve the paper. Our point-wise responses to the referee comments (in *italics*) are as follows:

Referee: *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 bottomup 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*

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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 yr1 for goatssheep 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 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.

Authors: We will add a paragraph discussing the role of ruminant emissions and its uncertainty using additional sources (EDGARv5, FAOSTATS) to the manuscript. Please see our response to the first comment of the first referee.

Referee: *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?) clearsky 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.*

Authors: There is a correlation between the coverage and the emission estimates, however, even in JJA, there is more than 40 % of SSWR data coverage (percentage of pixels in an average map with at least 5 days of TROPOMI observations). We believe such an amount of observations should be sufficiently sensitive to the emissions from the region, unless the observations are missing over the high emission wetland's region, which is not the case. We will add the following text to clarify:

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"The lowest enhancement is observed in JJA in both 2018 (10.5 $\pm$ 4.1 ppb) and 2019 (2.6 $\pm$ 3.7 ppb). It is unlikely that these low enhancements are an artefacts of the low coverage as there is still sufficient TROPOMI data (more than 40) and the measurement are not systematically missing over the large emission areas of SSWR, the Sudd and Machar wetlands."

In response to the second comments of the first referee, we have updated our emission quantification method to account for latitudinal XCH4 gradient and ITCZ. These updates to our method result in 1.4 $\pm$ 2.1 Tg yr^{-1} total methane emissions for the JJA-2019 and 2.6 $\pm$ 3.7 ppb XCH4 enhancement. We will add the following text to our manuscript:

"We find very low total emissions in JJA-2019 (1.4 $\pm$ 2.1 Tg yr^{-1}), but it accommodates the season's anthropogenic emissions of about 0.48 Tg yr^{-1} (sum of 2012 EDGAR emissions, and 2016 oil and gas emissions from Scarpelli et al., 2020) and GFED biomass burning emissions (0.003 Tg yr^{-1})."

Specific comments:

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

Authors: It is under the strong climate mitigation scenario (RCP 2.6). We will add this information in the revised manuscript.

Referee: *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?*

Authors: The two Wetcharts versions provide emissions for different years: Full Ensemble provides emissions for 2009-2010 and Extended Ensemble provides emissions for 2000-2015. To compare annual emissions, we use both versions of Wetcharts (Ta-

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ble 1). The strong trend in CH4 emissions reported by Lunt et al. (2010) is not shown by the 18-member Wetcharts Extended Ensemble, likely because it does not have the corresponding trend in the inundation extent that is derived using the ERA-interim precipitation (see Figure 4). The Full Ensemble is used for temperature dependence analysis as it provides a wider range of possible emissions (324-member ensemble).

Referee: *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.*

Authors: These datasets are used by Wetcharts to calculate a baseline inundation spatial distribution, which is then scaled with ERA-interim precipitation and SWAMPS data available for years after 2010 to derive inundation extent in later years.

Referee: *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?*

Authors: ERA5 meteo data has an approximately 30 km spatial resolution and hourly temporal resolution. We expect the 4 consecutive hours winds not to be independent, however, they are only used to estimate the uncertainty of the wind speed using the spread during these hours. Generally, larger hour-to-hour variation would mean a larger uncertainty in the wind speed estimate by a meteorological model for a particular hour. Surface observations are not suited for this analysis as we are interested in wind speeds over a large source area (4 x 10^5 km^2) averaged across the whole boundary layer.

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

Authors: That is a typo. We will correct it.

Referee: *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 OG (possibly bigger, see general comments).*

Authors: Oil and gas emissions can be quite uncertain. This is especially true in regions with less strict flaring regulation. In addition, they are located in the SSWR and may correlate spatially with wetland emissions. The agricultural methane emissions in SSWR due to agriculture waste burning and agriculture soils are negligible as per EDGAR. To address emissions from enteric fermentation (0.36 Tg/yr), we will add new text to the revised manuscript. Please refer to our response the first comment of the first referee.

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

Authors: Done

Referee: *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.*

Authors: In general, the mean emission estimates of the bottom-up models are an order of magnitude lower. We will clarify this in the revised manuscript.

"SSWR integrated mean methane emission estimates from the process models are nearly an order of magnitude lower than those from TROPOMI"

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

Authors: Please see our response to the second comment of the referee.

Referee: *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.*

Authors: We address both the issues as a possible explanation one by one in the manuscript without claiming that one of them is the definite cause. To clarify that both the factors contribute, we will add the following sentence to the manuscript:

"The seasonality difference of models and TROPOMI emissions can be explained by a combination of temperature and inundation extent information used in the models."

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

Authors: The relation between temperature and emissions is not linear. Therefore, a small change in temperature can cause a large emission change.

Referee: *Line 362: Some models report emissions that are definitely NOT 'an order of magnitude' smaller.*

Authors: The mean estimates of the models are roughly an order of magnitude smaller. We will clarify it.

Referee: *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.*

Authors: The mean of Wetcharts Full Ensemble emission estimates with Q10 $= 3$ is 1 Tg yr^{-1} which is still nearly an order of magnitude lowered than our emissions estimates. We do not claim to disentangle the influence of inundation extent and temperature. We only try to hypothesize possible causes that explain the difference between TROPOMI and model emission phasing based on shortcomings of assumptions and

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input dataset used by the models.

Referee: *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?*

Authors: We agree with the referee that data coverage needs a proper description. We will add the following text to clarify:

"Data coverage is defined here as the fraction of 0.1 $\times$ 0.1 degree grid cells in SSWR with at least five days of high-quality TROPOMI measurement in a quarter."

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