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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.

Sudhanshu Pandey et al.

s.pandey@sron.nl

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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's comments (in *italics*) are as follows:

Referee: *1. Cattle. The general concern is that cattle and other ruminants are not mentioned at all. South Sudan has one of the world's highest 'per-human capita' ruminant populations. It is hard to get good numbers, but old FAO data suggest 12 million cows, 14 million goats and 13 million sheep. I have not myself been to South Sudan, though I have visited both north Sudan and Uganda: cattle are everywhere. So are*

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Discussion paper

goats. Moreover, cattle and swamps go together – some African swamps have cows on every bit of available footing. This is ruminant heaven. This is not a minor matter – there is much debate whether tropical methane emissions are rising because swamp methanogenesis is increasing the direct emission of methane to the air (e.g. via plant stems, or ebullition), or whether the methane emissions are rising because of increasing cattle populations (e.g. see Schaefer et al. Science 2016 and Nisbet et al. G.B.C. 2016,2019). If the main emitter is the swamp directly, then flux will depend on temperature and inundation. Global warming will feed back into more emission. It will be very hard to mitigate this locally, except by cutting warming globally. But if the main methane emitters are the cattle that eat the swamp vegetation, then emissions may also be partly dependent on cattle numbers – the growth in emissions will in part come from human actions in increasing cattle populations. In principle, then, emissions can be mitigated by reducing cattle populations. In South Sudan this is particularly relevant as the cattle populations are not simply food producers but also held as 'currency'. There are far more cattle than needed for food. Switching to easier-to-maintain non-cattle 'money' (e.g. cellphone accounts, as in neighbouring Uganda) can lead to reduction in cattle numbers and hence cut methane emissions. It would be a large cultural shift, but feasible and would have significant impact in mitigating emissions – if the emissions come from the cows, and are thus subject to human intervention. To sum up – surely cows deserve a mention!! – after all, there is discussion of waste water management! Also manure management is an odd factor here – where is that done in South Sudan? This is a very different place from the US with its manure lagoons and industrial cattle. My experience of the region is that plops land where plops land. They may be picked up to burn as biofuel in winter, but there is little "management".

Authors: We agree with the referee that livestock is an important contributor to the methane emissions from South Sudan, and it needs to be addressed in our paper. We will add the following text to the revised manuscript:
"Livestock is the largest anthropogenic methane source in SSWR region: 0.36 Tg yr^{-1} in 2012 as per EDGAR v4.3.2. In 2015, it had increased to 0.37 Tg yr^{-1} as per EDGAR

v5 (Crippa et al., 2020). South Sudan has a large population of livestock: 7.5 million dairy cattle, 4.6 million non-dairy cattle, 13.5 million goats and 16.3 million sheep in 2018, which causes 0.63 Tg yr^{-1} of methane emissions (FAOSTATS., 2020). This amount is twice of what we use to calculate the wetlands emissions for SSWR. In the extreme case that all these additional emissions are located in SSWR, it would slightly reduce our wetland emission estimate, however, well within its uncertainty margin."

Referee: *2. Mass balance approach. To assess fluxes, the paper depends on the work of Buchwitz et al. 2017. This is interesting and innovative, but the Buchwitz et al. approach focussed on hotspots like the Four Corners region. Although that paper did go to the more regional scale of Turkmenistan, the scale on which the method is being applied in this paper is significantly more than the main Buchwitz et al applications. Also, the region is perhaps rather inhomogeneous with regard to distribution of methane sources.*

Authors: Mass balance approaches have been used for emission quantification using TROPOMI XCH4 data in multiple recent studies (Pandey et al., 2019; Schneising et al., 2020; Varon et al., 2019; Zhang., et al 2020). Pandey et al. (2019) and Varon at al. (2019) apply the CSF (Cross-Sectional Flux) mass balance approach to methane plumes visible in individual overpasses of TROPOMI. Schneising et al. (2020) apply a Gaussian integral mass balance approach to individual overpasses of TROPOMI over the large Permian oil and gas basin of USA and find emission estimates consistent with GEOS-chem inversion results of Zhang et al (2020). The mass balance approach of Buchwitz et al. (2017) is also used in Zhang et al. (2020) to quantify emissions from the Permian basin region with effective length ($L = area^{0.5}$) of 375 km, which yields emission estimates consistent with the GEOS-Chem inversion of the study. The L of our South Sudan wetland region (SSWR) is 632 km. The size of the background for the Permian study was 24×24 degrees latitude-longitude, comparable to our background of 25 $\times$ 26.5 degrees latitude-longitude. Further, Buchwitz et al. (2017) successfully test their approach on some large regions: Turkmenistan ($L = 700$km) and Azerbaijan

($L = 300$km). In addition, our emission estimates are agreement with that of GOSAT inversion emissions of Lunt et al. (2019) for the latest year in their inversion (5.2–6.9 Tg yr^{-1} for 2016), especially when the source region differences between the two studies are accounted for. We expect the impact of emissions inhomogeneity to be small and within the uncertainty margin of the quarterly emission estimates. Overall, the performance of the method has proven to be sufficiently accurate for the purpose of this study.

Referee: *3. I note that in some quarterly periods, the 'background' region (fig 3) crosses the 'methane waterfall' at the Inter-Tropical Convergence Zone, with a major difference of maybe circa 30 ppb between values 100km north and 100km south of the ITCZ. Is this valid for background? - Methane in the Southern Hemisphere is MUCH lower. The situation is complex, because the ITCZ does reach South Sudan in the northern summer, but in the equinoctial months it is close to Uganda. Thus, given the proximity of the ITCZ, and the inhomogeneity of the region, and I'd imagine defining a background could be very difficult, yet crucial to the modelling. Note that errors in JJA (wet season) are large compared to fluxes and data coverage low. The result of -1.5 $\pm$ 2.4 ppb enhancement looks very odd at the peak of the rains. Could this be a background effect with the much-lower CH4 Southern Hemisphere air so close by, south of the ITCZ? Are the conclusions justified given this?*

Authors: We thanks the reviewer for pointing this out, and we agree that the position of the ITCZ in different seasons can give rise to biases in our estimates. To address this, we have updated emission estimation method. Now, we first subtract a 3rd order polynomial fit of XCH4 as a function of latitude from the XCH4 maps used for emission quantification. This accounts for the influence of the methane "water fall" along the ITCZ on the background. To derive the polynomial fit, we exclude the longitudes crossing the SSWR region to ensure the fit is not affected by the source region emissions. The attached figure shows averages quarterly XCH4 plots after removing the latitudinal fits (it will replace Figure 3 of the manuscript). We find a new total emission estimate of 8.2 ± 3.2 Tg yr^{-1} (old estimate: 8.0 ± 3.2 Tg yr -1) and wetland emission estimate from SSWR to be 7.4 ± 3.2 Tg yr^{-1}, in statistical agreement with our previous estimate of 7.2 ± 3.2 Tg yr^{-1}. The following text will be added to the revised manuscript:

“We remove the large scale latitudinal XCH4 gradient from the seasonal average TROPOMI XCH4 maps by subtracting a 3rd order polynomial fit from the background region, excluding the source region (see Figure 2).”

"Table 1. SSWR XCH4 enhancement and emission estimates. The enhancements (Δ XCH4) is the XCH4 difference between SSWR and the background after correcting XCH4 for latitudinal variation. Data coverage is defined as the fraction of 0.1 × 0.1 degrees grid cells in SSWR with at least five days of high-quality TROPOMI measurements in a quarter. Wind speed is the average boundary layer wind from ERA5. Emission estimates are calculated using Eq.(1). ± represents the 1σ uncertainty." (see supplement)

"Figure 2. Seasonal average TROPOMI XCH4 (ppb) at 0.1×0.1 degree resolution. The black rectangles at the centre of each panel show the SSWR source region. The area outside of it is used as a background. XCH4 is corrected for large-scale latitudinal variation by subtracting a 3rd order polynomial fit using the region shown by blue rectangles, which excludes the longitudes of the source region. Note that DJF includes December of the previous year." (see supplement)

Referee: *4. The study only uses observations under cloud-free conditions. That’s obvious – or is it? In the wet season under the Inter-Tropical Convergence Zone, cloud-free means that a major change has occurred – the ITCZ has moved, and winds and advection are different. It’s an almost insoluble problem for satellite observation: the action is under the clouds. However, there are period – for example mornings, or occasional days when thunderstorm cells are present but not in the immediate area – when the ITCZ is present but clouds absent. But these low advection periods might be cooler or otherwise different than average cloudy days. Thus it would help to discuss* *this problem – are cloud-free conditions valid conditions to determine 'typical' fluxes?*

Authors: Yes, we only use cloud-free observations, selected using the highest quality filter (qa = 1) which masks the presence of clouds and cirrus. This causes a sampling bias, but it will only affect our estimates of the emissions influenced by cloud cover. To our knowledge, there is no direct evidence that such a relationship exists, although we cannot exclude the possibility that it has some influence. TROPOMI provides enough measurements to sample the region throughout the year, which we consider the most important.

Referee: *5. Line 316. JJA and Temperature dependence. This is a very interesting finding and probably should be emphasised more in the abstract and conclusion, as the implications for emission modelling are important. As the authors correctly point out, in this region (e.g. Malakal), the summer months July and August are the coolest months of the year – because of the cloud cover. Pre-rains March and August are hottest. Incidentally, are temperatures from ECMWF (line 135)? Or are they checked against local values, for example from Malakal? Sometimes ECMWF can be not good in recognising local factors.*

Authors: Temperatures are taken from ECMWF ERA5. The seasonality of this data is consistent with average weather in Malakal (https://weatherspark.com/y/96893/Average-Weather-in-Malakal-South-Sudan-Year-Round,accessed1-10-2020). We will add the following text to the revised manuscript to highlight the JJA and temperature dependence finding.

To abstract:

"We find the lowest emission in the highest precipitation and lowest temperature season JJA, when models estimate large emissions. In general, our emission estimates show better agreement, in terms of both seasonal cycle and annual mean, with model estimates that use a stronger temperature dependence. This suggests that temperature might be a stronger control for the South Sudan wetlands emissions than currently assumed by models."

To conclusions:

"We find the lowest emissions in the highest precipitation and lowest temperature season of JJA, when models estimate large emissions as they assume a strong influence of the precipitation-derived inundation extent. We find that the Wetcharts emission estimates that use high-temperature dependence (q10 = 3) show a better agreement with TROPOMI concerning both seasonality and annual emissions. This indicates that the models may also underestimate the temperature sensitivity of the methane emissions. The causes of this need to be investigated further."

Referee: *There is a typo in Table 1 top line – XCH4 is plus/minus, not simply plus as written. A minor point – 'IE' for inundation extent, is an acronym too far. Why not just say 'inundation extent' each time and save us the reference to Ireland every time.*

Authors: Done

Referee: *The manuscript is otherwise well written and well presented. To conclude, this is an important paper that should be accepted after a re-think and minor revision. I would suggest amendments, particularly to discuss cattle, in this extremely cow-focussed region, and also to consider issues around defining the background.*

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Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2020-251/bg-2020-251-AC1-supplement.pdf

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