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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 #1

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General

This is a thoughtful and well-argued paper on an exceptionally important problem, that should certainly be published. It has much fresh insight, and makes innovative use of the powerful new data from the TROPOMI instrument. However it has some significant shortcomings that need to be considered before the study is accepted.

The atmospheric methane burden is rising, and a significant part of that growth appears to come from the northern tropics. In this context, South Sudan has some of the world's most important wetlands, primarily in the Sudd swamps of the White Nile river, and also in other wetlands along its tributary Sobat river, from Ethiopia. Secondly, it is not clear if the current rise in methane arises directly from human actions that increase emissions, or as a warming-feeds-warming feedback from global warming.

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Thus accurate quantification of South Sudan's emissions is globally important, and it is also a major question how these emissions will respond to changes in both temperature and inundation. But South Sudan is relatively inaccessible to sustained scientific field work as there are violent conflicts in the region, in the South Sudanese Civil War (2013-2020).

Pandey et al. address the task of quantifying South Sudan's methane emission by using TROPOMI observations from the S-5P satellite. This is immensely valuable in global terms – it is a key area – and the paper makes a major and vital contribution.

I do however have a number of specific concerns.

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 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".

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.

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 30ppb 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 CH_4 Southern Hemisphere air so close by, south of the ITCZ? Are the conclusions justified given this?

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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?

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.

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.

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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