Comment on egusphere-2022-948
John Worden (Referee)

General comments (By John Worden).

The paper is mostly well written, except for the section comparing posterior OH to ATOM results. The main concern I have is that the method and results in this paper are not fundamentally different from those in the Qu et al. ACP 2021 paper which also uses TROPOMI data for quantifying emissions for essentially the same time frame. The main difference between this paper and the Qu et al. paper is the version of the data, which is ostensibly more accurate than the data used in Qu et al. but then there is no discussion on how this improved data set changes, or potentially improves the results over and above the Qu et al. results.

For acceptance, the paper needs to better describe the difference from those in Qu et al, and how these results are an improvement; I think there are sufficient results in here for this purpose (e.g. monthly estimates allow for attributing some components of the methane budget). In addition, you could compare with the Qu et al. 2022 paper (methane surge) which uses GOSAT data for 2019; in principal the improved TROPOMI data sets should result in better comparisons with the GOSAT based results for this time period.

Another issue is a lack of discussion on uncertainties; I see them reported in final estimates but it's not obvious how they are computed, are these buried in the text somewhere? (I'm pretty sure I read through the entire text, 2.5 times + browsing). A more extensive discussion on uncertainties should be in Section 2.

Note that I'm not convinced that the downscaling approach described here is sufficient by itself to merit publication as it is not (obviously) an improvement over the optimal estimation based approach described in the un-cited Cusworth et al. 2021 paper (see subsequent comments).
Specific Comments:

Abstract, “… indicate rapid increases in Middle East”; the way this sentence is currently written implies you base this statement on satellite observations.

Abstract: state that you are estimating monthly values

Abstract: You stated you used observations of CO and OH, but I don’t see any description of these data in Section 2. Also see comments on comparisons to CO and OH to ATOM below

Section 2.1 Page 3: As stated in the general comments, the analogous paper here is from Qu et al. ACP 2021 which uses V1.03 TROPOMI data whereas you use the Lorente et al. based corrections; make that difference clear here. Note that as far as I can tell there is fundamentally no difference between yours and the Qu et al. results, notwithstanding the improved XCH4 data sets you are using.

Can you add discussion on this difference in Section 2.1 and then add more comparisons to the Qu et al. 2021 results in Section 4?

Section 2.6, page 7, Provide rationale for why you are downscaling to 0.1 degree resolution, especially since it depends on priors which can vary considerably (uncorrelated at 0.1 degree resolution) depending on choice of prior as you note in the text. As far as I can tell, the downscaled results are not used thereafter in the paper, is that correct? (Note that in the Worden et al. 2022 paper, we downscaled so that we can then upscale more accurately to each country; the other reason for the OE based downscaling (Cusworth et al. 2021) we developed is to step us towards using top-down emissions estimates for updating gridded inventories at this scale).

How does this downscaling approach compare to the optimal estimation based approach to downscaling discussed in Cusworth et al Earth Environ 2, 242 (2021). Can you perform a test(s) similar to what is shown in Cusworth et al. to ensure you are preserving information from original grid and downscaled grid? Your co-author A. Bloom designed these tests for Cusworth et al. so you could ask him for details. Note that I would be
ecstatic for an additional vetting of this OE/Cusworth approach by the Dylan / Daven crew... we are pretty sure we got the math right as we used two different approaches to arrive at the same result (the Cusworth / Bloom and the Bowman approach, with Worden moderating), but given that its a 30+ equation derivation some additional vetting is desired.

Also cite Liu, M. et al. A New Divergence Method to Quantify Methane Emissions Using Observations of Sentinelâ5P TROPOMI. Geophys Res Lett 48, (2021), as a potential way to use satellite data to identify and quantify emissions at these same fine spatial scales.

Section 3.2. As a reader I did not understand either the rationale for the comparison to ATOM, or how I should interpret the comparison.... This section basically needs a re-write. Note that our group at JPL also attempted to use the ATOM OH estimates but decided against it (although this was a few years ago) because we did not have a good sense of the accuracy, especially since OH is tricky to measure; some discussion is needed on the ATOM OH accuracy to better interpret the comparison between your inversion results and these in situ results. Also, what did you intend to conclude from the comparison to CO?

Section 5.0, Compare against the Ma et al. 2021 and Zhang et al. 2021 wetland results which suggest ~149 Tg CH4/yr total...this again might be a TROPOMI versus GOSAT issue as TROPOMI data results in lower livestock emissions than those from GOSAT in Brazil, which in turn would likely balance to the wetlands, relative to the GOSAT based results. A discussion here on these differences is needed.

Section 6, again compare these totals to the GOSAT based estimates (there are several now available). Discussion on potential TROPOMI / GOSAT differences are needed as well.

Section 6.2, Note that reports to UNFCC from Russia have varied considerably over the years, this should be discussed here (e.g. Scarpelli et al. 2021 versus Scarpelli et al. 2022).

7.0 Conclusions (and to some extent abstract). The paper implies that missing sources can be identified through the downscaling approach, but this is not possible if you are using prior emissions for the downscaling. Also, how can the Venezuelan source simultaneously be lower than the prior and inline with trend estimates? These are different quantities. I think you mean something else here.
7.0: Line 540 Conclusions about waste and agriculture priors being too small... yes we are finding this to be the case with all the other published TROPOMI and GOSAT based inversions, please reference these other papers.

7.0 Conclusions / Line 555: This conclusion is potentially very interesting but needs additional vetting. For one, how much of yearly Indian and Southeast Asian underestimate is due to the underestimate in the Monsoon seasons? In addition, how much of this is affected by smoothing error, which is not directly calculated using your method, but you could calculate by using different priors; basically we are finding significant impact of smoothing error, or alternatively cross-correlation of a change in one emission onto another, for emissions and their trends in this region.

References: You can peruse the Worden et al. ACP paper for missing references on GOSAT inversions that you can then compare to in the text; this same comment was made by reviewers of our Worden et al. paper.