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Comment on acp-2021-955

Anonymous Referee #2

Referee comment on "The 2019 methane budget and uncertainties at 1° resolution and each country through Bayesian integration Of GOSAT total column methane data and a priori inventory estimates" by John R. Worden et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-955-RC2>, 2022

This paper presents an ambitious attempt to estimate methane sources globally at 1 degree resolution with detailed information on country-level and sectoral emissions included. Given the Paris climate accord's requirement for all countries to take stock of greenhouse gas emissions, this is a critically important need though also inherently challenging. Satellite data may contribute important information, particularly in areas that have poor bottom-up emissions estimates and heterogeneous natural sources like wetlands/lakes/rivers, but there remain substantial limitations in connecting top-down inverse estimates to policy relevant spatial scales and reporting categories. The authors emphasize that this is a pilot study attempting to create such an inventory using a number of new attribution techniques and is intended to spur a conversation and further research. However, there are important gaps in the presentation of methods and recommendations on how such data is best interpreted and used that need to be addressed.

Major Comments

Title: The title 'The 2019 Methane Budget...' is a bit confusing/misleading. There is a broad and well established community effort known as 'The Global Methane Budget' and many readers may think this is an update from the same group. However, there is little to no overlap between the two author lists and the approaches are very different. I'd recommend changing the name to avoid confusion. In addition, highlighting 'Each Country' in the title seems inappropriate given that results show that the GOSAT data used constrain ~25% of the countries considered (Table 3 and discussion).

Abstract: The abstract is long and could be shortened to focus more on key findings and less on details of methods. It's not clear if the authors want readers to focus on the findings or inherent limitations that come from uncertainty in the methods and input datasets.

L204 – Model errors, particularly related to representation of atmospheric transport, are critically important and should at least be mentioned in this section. Are transport errors characterized in this study? If not, what might the effect be? Recent papers on this issue for CO₂ inversions (e.g. Schuh et al., 2019, <https://doi.org/10.1029/2018GB006086>) raise substantial questions that are particularly relevant to GEOS-Chem inversions. These should at least be discussed in the introduction and conclusions as major factors that could alter results substantially in the future.

L268 – Seasonal variations are assumed to be correct, which could have a large impact on the results. How do seasonal variations in the EDGAR v4.3.2 inventory used here compare with newer version like v5.0 and v6.0?

L274 – I'm particularly confused about the treatment of wetland sources. They are not included in the state vector, but are presented as distinct in Section 3 results (e.g. Figures 3 and 4).

L390 – Is there a demonstrated improvement in independent data collected in areas that show more influence from nearby sources? It's true that surface data are primarily located outside of source areas, but some aircraft data like ACT-AMERICA do target areas of North America and some TCCON stations would be more relevant in this context. Could the authors provide more detail given the importance of independent validation of such results?

Section 2.1 – More information is needed about the data used for the inversion. In section 1, the authors point out that inversions with GOSAT and TROPOMI data can provide different results due to biases in the data (attributed largely to TROPOMI). This underscores the importance of the dataset used. More details on what retrieval method is used, version, bias correction, etc. are needed to better understand how these factors affect the results.

L426 – Why is 1 degree spatial resolution chosen when the atmospheric model is run at coarser scale (~2 degrees) and many priors are available at finer resolution (0.1-0.5 degree)?

Section 2.3 – The use of information at different spatial scales is confusing. The model is run at 2 by 2.5 degrees, prior information is then used to provide additional spatial and sectoral information (down to 1 degree). I am not clear how the 7 or 8 regions described in this section are used in the attribution (and the number is inconsistent within the discussion).

L512-513 – The authors point out an important limitation of the utility of such methods – emissions are co-located and cannot be distinguished. How does this affect the intended use of this product in assessing BU inventories?

Section 3 – How do the authors think that limitations in current satellite data (e.g. albedo biases, lack of data in cloudy regions) affect the results? Are these factors particularly confounding for estimation of certain sectors like rice cultivation and wetlands?

L629-630 – Can the authors provide a point of comparison for a different inversion system that is not based on GEOS-Chem?

L698 – How are low albedo GOSAT data handled? Are they excluded or is there a difference in the retrieval method that should give one more confidence in the GOSAT based inversions. See previous comment on need for more specifics about GOSAT data used in the study.

L718 – How robust are results for smaller geographic countries (e.g. Myanmar) given the limited resolution of the model used?

Section 3.3 – The table including DOF information is useful and the authors take care to note in the text that the inversion really only provides additional information in 58 of the 242 countries listed (and that information is still quite limited, with DOF < 2, in all but 31 countries). However, a casual reader could easily overlook this information and think that the article is claiming to provide satellite-based analysis over small countries/emitters, which I don't think is what the authors intend. Could table 3 be color coded to indicate confidence in the results – for example, green if DOF >2, yellow is DOF between 1 and 2, and red for DOF < 1?

L27, 689, 735, 785, 814 – The authors state multiple times that this study is intended as a starting point, which is honest given the complexity of the task. But it is not clear what the next steps beyond a starting point would be. Should these data be used by policy makers involved in the Global Stock Take? What are the priorities for the research community in moving the current state of the art forward? Providing such context would help readers understand how they can best make use of the results presented here.

Minor/technical comments

L371 – Satellite names should be corrected and should be consistent with L806, L820.

Fig. 3-4 – Color scale and size make these hard to see. Consider eliminating some panels to make larger and/or adjusting range of color bars.

Table 3 – Since a number of different inventories are discussed and used at various points in the study, the authors should clarify which estimates are included here.