

Earth Syst. Sci. Data Discuss., referee comment RC1
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Comment on **essd-2021-246**

Anonymous Referee #1

Referee comment on "Northern hemispheric atmospheric ethane trends in the upper troposphere and lower stratosphere (2006–2016) with reference to methane and propane" by Mengze Li et al., Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2021-246-RC1>, 2021

GENERAL COMMENTS

In this study, Mengze et al. present an analysis of observed (airborne) vs modeled ethane, propane and methane trends for the period 2006-2016. They estimate a global emission of ethane of 19.28 Tg/yr. Their results show trends for upper tropospheric and stratospheric ethane, propane and methane.

The paper is generally well written and contributes to the scientific understanding of ethane, propane and methane trends globally. I recommend this paper for publication after major revisions.

My biggest concern is that authors jump into conclusions too fast in some sections. There is also a lack of information regarding the model simulation setup. There are multiple places where it is hard to distinguish if the authors are referring to modeled results or observations, making the reading a bit confusing. Also, almost all figure captions (in the manuscript and the supplemental material) should be improved by adding information regarding the legends, and the type of information shown (see specific comments below). Lastly, even though this study shows results for propane and methane, these compounds are barely discussed in the text and their results are not even mentioned in the abstract. I wonder if somehow the title should be changed to clarify the readers that ethane is the main compound discussed in the paper and just a few propane and methane results will be shown.

SPECIFIC COMMENTS

Section 3.2

- There is no comment on the most significant feature of Figure 1, which is the much stronger and different seasonality of ethane for EUR compared to the rest of the regions. Why are concentrations so high for this region and why the peak shows up 1-2 months prior compared to NAM and whole NH?
- How does stratospheric ethane lack of general seasonality from this study compares to others?
- Why does NAM show such seasonality? Any insights of the reasons behind it?

Lines 219-221.

- Figure S3 does not indicate the region that the uncertainties correspond to (e.g. NAM, EUR). If it is for the whole globe, it needs to be stated. Again, captions and descriptions in the text should be improved.
- “. and model optimizations (section 2.2)”. I couldn’t find the description of the model optimizations in section 2.2. Check or add this information clearly. Use the same terms/words, so that is clear and easy for the reader to find this information. This is extremely important to better understand Figure 2.

Line 222-224. Map with location and time frames of sample collection is needed.

Lines 224-225. How were these observations selected? Which are those observations selected?

Figure 2 – Panel a) I am surprised by seen that the FEF opt contribution is almost similar to RES opt and how little BIB opt contributes from 2006-2014. Also, more information of model setup is needed. Specifically a table with emission inventories used, global and regional (NAM, EUR, etc) totals per sector will be helpful.

Line 267. Add the word "modeled" in "... the *modeled* NH upper tropospheric ethane...".

Line 269. State that SWD and TRO are modeled results.

Lines 269. How can contributions of SWD and TRO be so high in NAM? Can these contributions be explained? Do these modeled results make sense? Also, how can SWD and TRO have such high sectoral contributions, but low contributions when considering trends from the optimized models (Figure 2).

Line 271. Add the word "modeled" in "Figure 3 shows the *modeled* sectoral..."

Figure 3. Even though this figure looks really nice, it does not provide clear information on the contribution of each sector and region. The size of Whole NH, NAM, EUR, and ASI on the right side is the same for each region. On the left side, the sectors have different sizes, but there is no axis or value assigned to each contribution and also, for each sector the slice corresponding to each region is always the same. My suggestion is to change this figure for another one that shows the contributions by region as stated in lines 271-280. The way it is right now, those contributions are not clear.

Lines 283-284:

- "Five geographical sectors" ?? Do you mean geographical regions? Explain what do you mean by geographical sectors because mixing regions with emission sources is confusing.
- Why can't BIB and BIO emissions be separated by regions? Are the emissions not gridded in the model? Clarification is needed.

Figure 4: Is Model Opt the sum of all the optimized geographical regions? The authors need to clarify this.

Lines 285-286.

- "... 30%~55%, 35%~50%, 50%~65%, and 30%~40%...", the symbol "~" must be changed for "-".
- Are these percentages average values from 2006-2016? Specify.
- "Ethane emissions from ASI dominates the trends..." What are the model estimated

ethane emissions (in Tg/yr) for ASI compared to the rest of the regions? A table with regional emissions used in the model is needed.

Lines 288-289. Clearly state to which atmospheric layer correspond these trends. For example: "...contribution to ethane trends *in the upper troposphere and/or stratosphere*".

Lines 295-301. How does these results compare with other studies?

Section 3.3.3 has "methane" in its title, but it is barely discussed in the text.

Section 3.3.4

- This section should be named differently because it discusses the comparison of the model to ONLY two observations. It's current name gives the false idea that there was a model simulation using ground station data. I suggest calling it: "Model results compared to two ground stations".
- The conclusion from the last sentence should be erased. It cannot be concluded just by comparing 2 stations that the model provides realistic values for ethane surface level. A thorough analysis of multiple surface stations must be done before jumping to that conclusion.
- I suggest completely getting rid of this section and mention that the use of this model results for surface-level ethane should be studied in the future.

Section 3.3.5

- Erase the term "budget" in the title and text because this implies sources and sinks of a certain compound and here, only global emission totals are presented.
- Thus, modify the title of this section.

Lines 334-335. How does the authors conclude that the stratosphere has a minor contribution for observed trends? Authors need to explain clearly why location biased trends can be discounted, even if it is for the same reasons that were discounted for the upper troposphere.

Lines 335-336. Add "over the Whole NH" before "shows a general..." to specify which panel the authors are referring to.

Lines 336-339. Suggest the following change: "The peak in 2010 is not seen at regional levels (NAM, ASI, EUR, Figure 6 (b)(c)(d)), which **suggest** global upward transport of the upper tropospheric ethane emissions (peaking in 2010-2011) into the stratosphere." No analysis was made to clearly indicate that there was an upward transport from the upper troposphere, therefore, it cannot be stated as such.

Line 339. Suggest the following change: "The second peak in 2013 **can be due** to the regional...". Same reason as explained in the previous comment.

Section 3.4.2 Add (if any) references and comparisons to other studies.

Lines 396-397. Specify which "current inventory" the authors are referring to.

TECHNICAL CORRECTIONS

Figures 1, 2, 3, 4, 6, and 7: Explanation of legends and terms used in the figures are missing in the caption.

There are two figures with number 5 (Lines 640 and 645).

Caption Figure 3. Clearly state sectoral contributions are model results.

Caption of Figure 4 should be revised because not all results correspond to "Optimized geographical sector contribution" as stated in the first sentence (Observations are not included in the caption). Also, clearly state that the optimized geographical contributions are model results.

Suggest the use of a different color scheme for the EUR, NAM, etc regions in Figures 4 and 7 to avoid confusion model optimizations in figure 2 and figure 6.

Sections 3.3.3 and 3.4.2 have exactly the same name. Indications to "troposphere" or "stratosphere" should be added to avoid confusion. The same goes for their corresponding figures in the supplemental material.