Reviewer 2

We thank the reviewer for the warm comments and appreciation of our work, and for the interesting suggestions which contribute to improve our manuscript. We have made changes to the manuscript based on the recommendations of both referees. The responses to the referee's comments are provided below.

Section 2.5.2: While secondary production accounts for ~15-30% of the total emissions, I wonder how accurate this estimate may be. Nicely, a brief discussion is presented in lines 554-556, showing that changing the overall yield of CH3O2+OH (from 11.4% to 13%) does not change the results. However, it is stated (line 240) that many other uncertainties are present in the chemical production.

There are indeed many uncertainties in the chemical reactions, but the 13% overall yield was constrained by model comparisons with methanol observations.

Would the consideration of these uncertainties considerably change any of the results obtained in the inversion? Additionally, I wonder how well RO2 is represented in the MAGRITTE model. I assume that most of it is coming from the oxidation of CH4, and therefore I would appreciate some additional discussion on the simulation of the OH fields in the model.

Thanks for this interesting point. We added the following text in Sect. 2.5.2: "*The model-calculated OH levels are a significant source of uncertainty for both the secondary production and the photochemical sink of methanol. For example, the representation of halogen chemistry (Sherwen et al., 2016), lightning NOx (Ghosh et al., 2025), biogenic VOC emissions (Williams et al., 2013) and their degradation mechanisms (Novelli et al., 2020) are potential causes of biases in the calculated OH concentrations. On the global scale, the MAGRITTE-calculated, mass-weighted tropospheric OH concentration average is* $11.8 \cdot 10^5$ *molec.cm*$^{-3}$*, very similar to a recently reported multi-model average* ($(11.1 \pm 1.6) \cdot 10^5$ *molec.cm*$^{-3}$*)(Naik et al.,2013).* "

Section 3: It is very interesting to note that the MAGRITTE model inversion is able to reproduce the observations in an exceptional way. Furthermore, I appreciate the investigation of the possible bias present in the IASIv4 data, which I consider the real main strength of this manuscript. However, I wonder if the authors could elaborate on the risk of using only an observational dataset over North America. Is this region really representative of the IASI bias? Based on the work of Bates et al. (2021), North America is mostly influenced by biogenic emissions, while biomass burning as well as chemical formation are comparatively much lower. Could the IASI bias be different in other areas of the globe, for example in regions strongly influenced by biomass burning or over the tropics, due to the much stronger influence of methanol chemical production?

We agree with the reviewer that the observational dataset used to characterize the IASI bias has its limitations, which motivated the extensive model comparisons with additional datasets (aircraft, surface in situ, FTIR). As discussed in the manuscript, the IASI bias determined from three U.S. campaigns overestimates the bias over boreal forests and (to a lesser extent) over tropical ecosystems. Future work should consider a more extensive set of observations for bias correction.

Figure 9: Based on what is presented (see, for example, line 385), the focus of the inversion is on terrestrial emissions, and therefore the IASI data over oceans were excluded. I would very much appreciate it if all figures including IASI data could be masked over the ocean, to avoid confusion during reading.

Thanks for this suggestion. Done.

Line 600: It would be great if the Spearman correlation could also be listed, to provide a more complete overview of the comparison.

Done. The following text was inserted: "*while Spearman's rank coefficient is increased from 0.86 to 0.89*".

REFERENCES

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