Sun et al. (2025) present a comparison of bottom-up and top-down isoprene emission estimates over tropical South America for 2019. The work is the first to use the CrIS retreivals from RAL's MLS scheme for this purpose. The model performance using updated emissions is compared to remotely sensed HCHO and in-situ isoprene measurements. The authors discuss potential drivers of biases in bottom-up emission estimates, and potential impacts on modeled ozone production sensitivities in the region.

Overall, the work is on an important topic and suitable for the audience of ACP. Additional details are needed to verify methodological soundness. Specific comments are given below.

Major comments

(1) More details are needed to confirm the maturity of the CrIS isoprene product used in the top-down analysis. The manuscript refers the reader to a Palmer et al (2022), and in the supplement of that paper, there is a discussion of the retrieval method; however, the uncertainties in the retrieval are not sufficiently detailed for the foundation of a top-down emissions study. For example, this manuscript states that "IMS column averages then to be lower than those derived from surface-based observations". It seems almost to be expected that IMS-based emissions would be lower than those from bottom-up emissions inventories. However, it is not clear how large these biases are. To address this concern, I suggest the authors (1) provide a quantitative assessment of measurement uncertainty (2) providing a comparison with other CrIS isoprene products, which may have more validation studies.

(2) It seems flux measurements are a more direct method of evaluating the top-down emissions estimates rather than relying on concentration measurements. Flux measurements have been reported previously for the ATTO tower (e.g., Langford et al., 2022). It may be that there were not flux measurements available in during the time period of this study- I am not sure. However, to whatever extent possible, the authors are encouraged to leverage observation-based assessments of isoprene fluxes to contextualize their work.

(3) The GEOS-Chem model simulation requires further evaluation. Doe the model systematically misrepresent NOx concentrations and/or PBL height? If so, how sensitive are the results to any biases in these parameters? Are there other measurements (e.g., isoprene oxidation products, ozone?) that could be used to assess model performance?

(4) The manuscript compares NME between TROPOMI and GEOS-Chem as a method of assessing the improvement offered by top-down emissions. Because difference in spatial patterns are also highlighted as a result, it follows that spatial correlation between GEOS-Chem and TROPOMI in the top-down and bottom-up scenarios should also be addressed within the main body of the manuscript. Table S1 in the supplement suggest the bottom-up inventory may produce a similar, and even sometimes better, correlation. How do the authors interpret this result?

(5) I do not find the HCHO:NO2 work to contribute meaningfully to the aims of this paper. There is a cursory acknowledgment in the problems associated with threshold-based analysis (line 340), but those caveats are not considered addressed or considered. The use of fixed numerical values (line 365) from a paper centered on China should either be removed, or rationalized more clearly.

Minor and technical comments

Line 33: A lifetime of ~1 hr is given for isoprene. According to the GEOS-Chem model, what is the range in mid-day isoprene lifetime in gridcells over tropical South America?

Line 97: More detail is required on the "standard" HEMCO configuration, as the manuscript currently presumes the reader knows what is included, and "standard" could change. For example, is CO2 inhibition or drought response included? Where it the LAI data from? Is this offline emissions using GEOS-FP meteorology?

Line 139. "Although the a priori constraint on the retrieval is weak, this is also accounted for": I am not sure what "this" refers to, what "accounted for" means, and how that is accomplished. Please clarify.

Line 163: For what regions was this bias-correction formula derived for? How do we know they are applicable here?

Line 172: Isoprene measurements from three measurement heights are used. How are these matched to GEOS-Chem levels? How is the data aggregated into the results shown in figure 3? Is there a sharp spatial gradient seen?

Line 174: Notation on the Ringsdorf citation needs to be corrected.

Section 2.5: What regression method was used? Does it incorporate error in the x and y axis? The paragraph starting at line 194 and the first paragraph were somewhat redundant. Combining these could help with readability.

Line 326: The third sentence of this paragraph is a fragment.