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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "PTR-TOF-MS eddy covariance measurements of isoprene and monoterpene fluxes from an Eastern Amazonian rainforest" *by* Chinmoy Sarkar et al.

**Anonymous Referee #2**

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This manuscript presents the flux measurements of biogenic VOCs in a tropical forest in eastern Amazonia. It reports isoprene and total monoterpene fluxes measured by a PTR-ToF-MS using the eddy covariance method. The data set, even though collected for two weeks only, is an important for understanding BVOC emissions for a highly active emission area with a substantial uncertainty on isoprene and monoterpene emissions. Thus, the data itself would be worth publishing, as it'd provide one more data point for the flux measurements, emission factors for two important biogenic compounds for a critical site. The paper focuses on the basic analysis by reporting mixing ratios and fluxes of two important BVOCs, and comparing the flux measurements with the MEGAN emission model prediction. Beyond reporting data in the rarely



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Discussion paper

observed station, the paper suggests the importance of the MEGAN2.1 1-km emission factor product and the large errors in the MEGAN2.1 PFT emission scheme, in terms of when compared to field measurements in local to regional scale studies. In summary, I'd recommend publishing the manuscript if authors could address the following concerns:

1. Measurement uncertainties (concentrations and fluxes) need to be better documented. This is important, particularly when interpreting the results comparing to the model prediction.

2. How was total monoterpene quantified if only one pinene was calibrated? What kind of assumptions were made when reporting the total monoterpene, and what is the associated uncertainty?

3. It looks like the MEGAN prediction is driven by the reanalysis meteorological field for air temperature and solar radiation data. Did the observations use the reanalysis meteorological field too? How much of the discrepancy between model and observation is actually from the difference in temperature and light data as the input data? Comparison in those process levels could really help improve our understanding in BVOC emissions, rather than simply suggesting models are overpredicting or underpredicting.

4. Several places in the manuscript mentioned 'Amazonian tree species' and their variation and distribution, and the site-specific emission factors appear to be the conclusion this manuscript tries to highlight. Can the authors give more descriptions on the tree types for the measurement site? The current description is very generic.

5. One important conclusion is that it seems like the 1-km resolution emission factor product is better than the 16 PFT emission factor method, which is not too surprising. Can the authors comment on how the 1-km product was derived in the first place for this region? Perhaps this could help shed light on how to estimate EF for those places without direct flux measurements?

Other specific comments:

a) The paper refers to MERAA-2 as the reanalysis meteorological data, but it should actually be MERRA-2 as its 'official' name.

b) Section 2.3 equation (2). The air density is needed for the eddy covariance calculation. How did the air density come from here? Was it measured or estimated?

c) Similar to Question 1. Need to describe the system errors involved in the EC flux error estimates, and discuss how the measurement errors affect model: observation comparison and other conclusions.

d) Section 3.1 title: consider 'BVOC mixing ratios and fluxes'?

e) Figure 4. Again similar to Question 3 above. Is the gamma temperature (and light) the same between measurements and models here? Are the difference driven by MEGAN input data such as light and temperature or by other processes?

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