

## ***Interactive comment on “Volatile organic compounds over Eastern Himalaya, India: temporal variation and source characterization using Positive Matrix Factorization” by C. Sarkar et al.***

### **Anonymous Referee #4**

Received and published: 26 May 2015

The manuscript “Volatile organic compounds over Eastern Himalaya, India: temporal variation and source characterization using Positive Matrix Factorization” by Sarkar et al. presents measurements of selected VOCs over an area not thoroughly investigated in the past and attempts the identification of the major sources responsible for such emissions.

The main problem I see in this study is in the calibration/quantification of gases. Carbon tetrachloride mean value is reported as 0.18  $\mu\text{g m}^{-3}$ , which corresponds to about 30

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pptv. In my opinion, this is impossible. Internationally recognized laboratories report average values that are almost 3 times this concentration (AGAGE network, CDIAC data, etc).  $\text{CCl}_4$  has never been so low in the past many decades. No scientific paper can present atmospheric levels of  $\text{CCl}_4$  so low without even pointing out this discrepancy and thoroughly discussing it.

Also, levels of benzene and toluene are exceptionally high. The average benzene is reported to be 81  $\mu\text{g m}^{-3}$ , about 24 ppbv. As a comparison, Baker et al., 2008, calculated an average benzene mixing ratio of 0.5 ppbv in Los Angeles and Detroit, the two cities with the highest benzene among 28 US cities investigated in early 2000. Barletta et al., 2005 found that the highest average benzene mixing ratio among 43 Chinese cities investigated in 2001 was 10 ppbv (average benzene in Beijing was 6 ppbv or 4 times lower than in Eastern Himalaya). I find hard to believe that tourist activities over Eastern Himalaya or long range transport can increase atmospheric pollution so much. Toluene is also of concern: mean of 140  $\mu\text{g m}^{-3}$ , about 36 ppbv, which is also exceptionally elevated.

These concerns about the VOC calibration are supported by the lack of info in the experimental part. The authors should add details about how the quantification/calibration was performed, what was the LOD? Precision? Accuracy? Also, concentrations are reported with too many significant figures. For instance, toluene’s mean value is 140.67  $\mu\text{g m}^{-3}$ : can the authors really measure it down to the second decimal? Also, the VOCs were measured with a MS, but the authors do not specify how the MS was operating: SIM or SCAN mode? How long was the sampling? The flow rate was 100 mL/min, but for how many minutes was the sample collected?

Finally, most of the discussion is very descriptive; about what was low and what was high, I felt that the discussion was quite limited for the audience of ACP.

In my opinion the manuscript is not ready to be published in the present form. The authors are encouraged to first address all the main issues and then re-submit the

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paper. Please find below some minor comment/typos.

MINOR COMMENTS:

- I am not a big fan of double parenthesis: abstract L5; section 3.1 L24.
- page 32136, L3, please replace “they also have the potential towards....” with “Some of them also have the potential....”
- page 32137, L20. I am not sure what “apparently” means in this context.
- page 32141, L23: change “get increased”
- page 21141, L24: “... remain same in these...” change to “....remain the same in these....”
- page 32142, L12. I am not sure how the info about the transport of carbonaceous compounds fits here, particularly because at the end it seems like the main source is attributed to tourist activities, not biomass burning.
- section 4.3 is particularly descriptive with no conclusions. Also, the high levels at night compared to the daytime could also be the result of the shallower BL. I am surprise that the author don't mention this possibility.
- Figure 3 is very hard to read, please increase the font in the figure.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 32133, 2014.

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