

Interactive comment on “Variability of OH reactivity in the Landes maritime Pine forest: Results from the LANDEX campaign 2017” by Sandy Bsaibes et al.

Anonymous Referee #3

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Bsaibes et al. presented ambient observed OH reactivity datasets from two instrumentation – CRM and LP LIF. The field site is a coniferous temperate forest site in south-western France. They presented intercomparison results between CRM and LP-PIF followed by a series of data analysis for the observed differences between the inside and the outside of the canopy in addition to between the day and the night. They particularly utilized a turbulence index as a determinant for a stagnation causing higher total observed OH reactivity likely due to the suppressed vertical mixing. It is not a trivial work to have all the listed instrumentation in one field site to get a comprehensive dataset as presented. However, the presentation and the analysis of the dataset can be improved substantially to provide better scientific insights. The list for the suggested

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improvements are following.

- 1) The conclusion of this study is obviously hand waving as they conclude that the origin of mixing OH reactivity is either uncharacterized emission or oxidation products. Those are basically the nature of all VOCs in the atmosphere anyway. A deeper discussion may be utilizing a box model is recommended to narrow down the source of missing OH reactivity
- 2) It is not entirely clear whether ambient VOC samples and OH reactivity samples were collected with the same sampling tubes. Please clarify this point as it is very important to evaluate potential imparity
- 3) As the oxidation product of CO is HO₂, it is more likely susceptible to interference from OH recycling during the calibration process with high CO concentrations. What CO levels do you use for calibration? Could you provide at least simple discussion that was not the case in your calibration process?
- 4) It appears that the trace gas OH reactivity such as CO, NO_x, O₃ and SO₂ is not considered in the calculated OH reactivity assessments. Considering the rural location, this may not be a substantial factor, but it still requires to be included.
- 5) Page 13 Line 12: Further quantitative discussion on the impacts from MT to the isoprene mass. What species would be susceptible for the fragment and how prevalent it can be?
- 6) It is well known that PTR sees higher MT then the sum of speciated MT quantified by GC. Add this discussion whether that was the case during the observational period. This may give us some insight on the missing OH reactivity.
- 7) Page 14 Line 3: Further quantitative discussion is required. It is not clear how the 4 % value has been drawn.
- 8) Figure 2: it is extremely confusing what I should look up to for the comparison. It would be better separate into figures describing in the different periods. I would

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recommend to present an intercomparison figure first so that readers can get a sense on the potential bias from the instrumentation. Also, please make it clear which MT species are constituting the total MT presented in the figure.

9) Figure 3: If you take a diurnal average and adjust the intercept, then do two diurnal variations agree better? It seems CRM has 4 s⁻¹ offset but the text description says otherwise. Please make them consistent! In addition, even without the intercept, there are ~ 20 % differences in the relationship. Please discuss the potential reasons!

10) A more description on u* is required: how you measured them and justify the classifications.

11) Page 20 line 12: Have you seen the described extreme weather events during the observations? If you have not, then this discussion is irrelevant.

12) The stable nocturnal boundary layer could cause accumulation of long-lived oxidation products of VOCs instead of vertical mixing. Therefore, the speculation for the MT emission attributing missing OH reactivity is not conclusive. The authors need to substantiate argument.

The dataset worth thorough investigation for better understanding in potential roles of MT in atmospheric reactivity. I would recommend the authors to take more time for the major revision of the manuscript.

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