

Zanoni et al. analyze a novel 5 day dataset of total OH reactivity measurements from a Mediterranean forest in Provence, S.E. France. The dataset includes a comprehensive suite of concomitant speciated VOC measurements, alongwith the total OH reactivity measurements measured below (2 m) and above (10 m) the forest canopy for three days and two days , respectively. The dominant tree species in the forest was downy oak (~75%) and the key primary emission was isoprene. The objective was to assess the role of oxidation within the canopy and to undertake a budget analysis for the reactive compounds using measured and calculated OH reactivities. The authors conclude that within this particular forested site, isoprene completely dominates the OH reactivity both below and above the canopy and chemical oxidation within the forest canopy is quite suppressed. Further, there does not seem to be much missing reactivity as they attain a closure for the OH reactivity budget most of the time. This is a new result as previous studies in isoprene dominated forested sites have revealed large gaps in our understanding of atmospheric oxidation processes. Hence, this study is a very valuable addition to studies of OH reactivity from forested sites . The novelty of the work presented by Zanoni et al is high and I recommend publication in ACP after the following specific concerns are addressed by the authors.

SPECIFIC COMMENTS:

1) Though, the paper is well structured, I agree with the comments of Reviewer 1, that at times there are serious language issues which tend to obscure the scientific meaning/clarity of the discussion. Thus the senior authors should help revise the submission or english language editing should be undertaken for the revised version. The full list would be very big so I shall point out just a few where the choice of the words is not correct.

e.g. 1: Page 220068; Line 15 and at other places in the MS :words like scatter and flattered "the signal of total OH reactivity started to scatter around 11:30 p.m., then again at 2 a.m. and flattered back to the signal of calculated OH reactivity around 4:30 a.m. (Fig. 5)." The authors probably mean the trace becomes flat??
"24 h statistics", "9 point percentage", "breakdown of reactivity " for pie charts..

2) Qualitative conjectures should be avoided:.... ...e.g. Page 22063; Line 11: "co-variated perfectly with PAR...."
In such cases, the correlation coefficient should be mentioned...

3) Why do the authors have such high uncertainty for the CRM OH reactivity measurements (35 %) when other CRM systems typically do a better job and the authors' own previous work (e.g. Dolgorouky et al., 2012) had less uncertainty? If it is due to specific conditions encountered in the present deployment , then the same should be discussed.

4) How did the authors produce the C2 stage zero ? Did they try humidifying bottled zero air or was it using a catalytic scrubber? How was the humidity regulated for the reactivity measurements? These are important points to clarify because as seen in Fig 5 and Fig 6, the

ambient RH covers quite a range (25% RH during daytime to ~ 60% RH at night), and ensuring the humidity is ok between C2 and C3 stages is no longer trivial.

5) The authors should discuss the magnitude of the pseudo first order correction for the measured OH reactivity values. Generally, the direction in which this correction works is to bring down the measured OH reactivity value, especially if an unreactive molecule like propane is used for inferring the correction. However in the present study, as isoprene and pyrrole both have similar reactivity with OH radicals (rate coefficients are of the order of 10^{-10} cm³ molecule⁻¹ s⁻¹), I am a little concerned that the correction applied to account for the pseudo first order effect could have caused underestimation of the measured OH reactivity, which could affect the conclusions about not having much missing OH reactivity.

6) The high ozone concentrations (60-70 ppb!) and its time series seem to be at variance with the finding that not much photochemistry occurs within the forest and lack of transport of anthropogenically influenced air masses to the site (which the authors state to be the case based on the benzene and acetonitrile measurements). As the author seem to have NO_x measurements and OH reactivity measurements, it may benefit the discussion if they discuss the photochemical regime for ozone formation in the forest based on the ratio of NO_x OH reactivity/VOC OH reactivity (see for e.g. Sinha et al., 2012, "Constraints on instantaneous ozone production regimes and rates..." Atmos Chem Phys).

7) What is the role of deposition of OVOCs and ozone to the forest and its implications if any for their conclusions? Some discussion on this would strengthen the MS further.

8) Page 22058; At 2.2 mbar and 60 degree celsius, I think the Townsend ratio would be 135 Td and not 130 Td; Can the authors please check their calculation?

9) Fragmentation of isoprene peroxides and their contribution to m/z 71 seems new. Can the authors cite some work and add more light on it? Unless the studies reporting the fragmentation used the same Td ratio in the drift tube, can one extrapolate those results for different Td conditions?

10) As the measurement site is a long term measurement site, where other routine measurements are performed, is there any information on the boundary layer dynamics between day and night at the site? van Stratum et al., Atmos Chem Phys 2012 showed these to be significant for another Mediterranean site with pine trees and I wonder if that could explain the strange ozone profile.

11) The authors may want to add some new OH reactivity measurements (average ~ 50 s⁻¹) to Figure 11 from an environment where biogenic emissions are dominated by agro-forestry of high isoprene emitting trees such as poplar and eucalyptus and isoprene concentrations are as high as 3 ppb (Kumar and Sinha, Int. J. of Mass Spectrom., 374, 55-63, 2014.

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