We thank reviewer for insightful comments. Our responses to the comments are provided below, with the reviewer's comments italicized.

Reviewer #1

(1) There are two papers which have been published very recently – not in time to be cited by this paper, that could be mentioned as relevant to the current findings. In AMTD (Fuchs et al., 2012) report OH measurements made in the SAPHIR chamber in Julich, where OH is measured using both the FAGE and DOAS methods. Isoprene, and other biogenic species, for example its oxidation products, are added to the chamber, and in general there is good agreement between FAGE and DOAS, providing evidence, that for the conditions of this study, there does not appear to be a significant interference in the presence of these species. Also in ACPD 2012, MacDonald et al report HCHO measurements using DOAS in a tropical forest, the levels of which can only be modelling using higher OH concentrations.

Response: The Fuchs et al. work does show a good slope for the scatter plot between DOAS and LIF OH measurements. However, the paper also shows some interesting dependences of the ratio of LIF/DOAS OH on certain alkenes. So this picture of no interferences is not as clear it would first seem. We have added the following statement to the manuscript regarding Fuchs et al. (2012): "A recent intercomparison study in SAPHIR chamber between LIF and DOAS instrument, also show positive bias by 30-40% from LIF instrument for several VOC species (MVK and aromatics), but not others (isoprene and MACR) (Fuchs et al., 2012). Further tests for terpenes and other BVOCs are required to quantify the possible interference."

As for the MacDonald et al. (2012) HCHO study, it may well be that their HCHO supports higher OH in a forest from Blodgett Forest, but there are measurements at Blodgett that support lower OH values (papers referenced in the manuscript and other manuscripts in preparation). In this manuscript, we are careful to state: "It is not clear whether these findings also apply to other forest atmospheres or to the OH measurements with other FAGE-type instruments in other forests." Thus, the HCHO measurements in another forest do not really apply to our paper's results and conclusions, which are focused only on our measurements.

(2) Page 6718, there was also a forested study in Greece (Carslaw et al. 2001) which showed a similar model underprediction for OH.

Response: Cited.

(3) It is interesting that there is little variability in OH, HO_2 and OH reactivity at 9, 12, or 15 m height. There are so few measurements of the height distribution of these parameters that these represent important findings. Were these results expected?

Response: We now state: "Little variability was found for OH, HO_2 and OH reactivity at these three heights (less than 20%), which is consistent with a model study (Wolfe et al., 2011), so we here use the measurements from all three heights to improve measurement statistics."

(4) On page 6722 a filter wheel is mentioned, can more details be given of this, is it a continuously variable neutral density filter?

Response: The filter wheel (Thorlabs) has six positions; GTHOS uses three: one open, one with a 50% transmitting neutral density filter, and one with a 33% transmitting neutral density filter. The filter wheel was periodically cycled and then the OH measurements from each filter position were compared. They showed no evidence of laser-generated OH.

(5) Page 6722 – photolysis rates are calculated using TUV – where any of these measured directly?

Response: No. We only had Photosynthetically Active Radiation (PAR) measurements for this campaign. However, the sky above Blodgett Forest is generally clear with an occasional high cloud, so the TUV calculations are known to be quite good.

(6) Page 6723/4, it would be worth distinguishing and explaining external and internal OH.

Response: We now add "Here external OH is ambient atmospheric OH before sampling and internal OH is OH that is generated inside the low-pressure region of GTHOS, from the inlet to the detection axis."

(7) Is the sensitivity of the instrument changed through the introduction of the additional C3F6 injection point at the top of the instrument?

Response: No. We state in the text that : "This injection system, without C_3F_6 addition, caused negligible OH loss according to several laboratory and field tests in which the injection system was removed for an hour and the OHwave signal did not change."

(8) Page 6727, Mao et al 2012 does not appear in the references.

Response: Added.

(9) Figure 1, this is an average diurnal cycle. What does the day to day variability look like for this? Are there days when the difference in OH(chem) and the model for example, is larger or smaller?

Response: We had to use the average diurnal cycle to incorporate as many data points as possible because the model simulation is limited by the availability of multiple measurements and the OH signals were low due to low GTHOS detection sensitivity for the BEARPEX configuration. Although the comparison between the models and the measurements did vary day-to-day, much of the variation was mostly due to of the variation in temperature (please see Figure 5 in the paper and Figure S3 a in the supplemental material). Figure 6, which is OHwave-OHchem versus OH reactivity, gives a good sense of the scatter in the data, much of which is caused by the low detection sensitivity for this study.

Reference

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