The authors are thankful to Anonymous referee 1 for the general feedback and the valuable comments for improving the manuscript. Please find below the answers to the comments and changes in the manuscript text.

1)

(1) The abstract is relatively long and especially the first paragraph (until l.14) reads more like an introduction. It would be nice to shorten it a little, highlight more the importance and novelty of this study, and emphasize the key-findings.

(2) We modified the abstract, please find the new version at (3).

(3) Total OH reactivity, defined as the total loss frequency of the hydroxyl radical in the atmosphere, has proved to be an excellent tool to identify the total loading of reactive species in ambient air. High levels of unknown reactivity were found in several forests worldwide and were often higher than at urban sites.

Our study presents atmospheric mixing ratios of biogenic compounds and total OH reactivity measured during late spring 2014 at the forest of downy oak trees of the Observatoire de Haute Provence, OHP, France. Air masses were sampled at two heights: 2 m, *i.e.* inside the canopy, and 10 m, *i.e.* above the canopy, where the mean canopy height is 5 m.

We found that the OH reactivity at the site mainly depended on the main primary biogenic species emitted by the forest, which was isoprene and to a lesser extent by its degradation products and long lived atmospheric compounds (maximum 26% during daytime). During daytime, no significant missing OH reactivity was reported at the site, neither inside, nor above the canopy. However, during two nights we determined a missing fraction of OH reactivity up to 50%, possibly due to unmeasured oxidation products. We confirmed that no significant oxidation of the primary species occurred within the canopy: primary compounds emitted by the forest were rapidly transported to the atmosphere. Finally, the OH reactivity at this site was maximum 69 s^{-1}, which is a high value for a forest characterized by a temperate climate. Observations in various and diverse forests in the Mediterranean region are therefore needed to better constrain the impact of reactive gases over this area.

2)

(1) Even though overall the text is easy to read, there are several flaws in formulations and sentence-structures. For example, in p.22050, l.11 the sentence probably ends after “troposphere” and similarly in l. 14 the sentence probably ends after the reference. Since I am not a native English speaker, I do not want to try and correct the English formulations. But I recommend to ask a native speaker to pre-review the text prior to the final publication.

(2) We thank both referees for this comment, therefore we will provide a revised final version of the manuscript which we hope will be better understood.

3)

(1) p.22050, l.10: Biogenic volatile organic compounds are globally the most abundant class of reactive organic compounds in the troposphere. Regionally, such as in urban environments, anthropogenically emitted compounds may dominate the atmospheric OH reactivity.

(2) We modified the text according to the suggestion of the referee.

(3) The atmospheric oxidation of volatile organic compounds directly impacts the quality of air and Earth's climate. Biogenic Volatile Organic Compounds (BVOCs) are globally the most abundant class of reactive organic compounds in the troposphere. On a regional scale, such as in urban environments, organic compounds emitted by anthropogenic sources may dominate the atmospheric reactivity.

4)

(1) p. 22051, l. 16-17: The definition "low NOx" or "high NOx" can be misleading (see Wennberg 2013, "Let's abandon the "High NOx" and "low NOx" terminology").

(2) The text is modified.

(3) Isoprene dominant loss is the oxidation reaction with the OH radical, which involves the formation of six isomeric peroxyradicals which in unpolluted environments further react forming methyl vinyl ketone (MVK), methacrolein (MACR) and formaldehyde (HCHO) as primary products in the highest yields (Jenkin et al., 1998).

5)

(1) p.22053, l. 15: Please, replace "close" with "examine".

(2) We took into account the suggestion.

(3) OH reactivity was used as a tool to examine the reactive carbon budget, and help assessing the oxidative processes occurring through the canopy.

6)

(1) p.22053, l.19: I think it should be "determined" instead of "sampled".

(2) The text is modified.

(3) We determined the OH reactivity and trace gases concentration at the same time, and investigated two canopy heights, one inside the forest at 2 m, a second one above the forest at 10 m.

7)

(1) p.22054, l. 3-4: The closest city and town to the measurement site are Marseille and Manosque. Could you please include, how many inhabitants do live there? Also, I cannot see the cities in Figure 1. It would be nice to have their location pictured in Fig.1, especially since you refer to it in the text.

(2) Text and figure are modified according to the referee's comment.

(3)

Observatoire de Haute Provence is located in the Mediterranean region, south east of France (5° 42' 44'' E, 43° 55' 54'' N, 650 m a.s.l.), with Marseille the closest largest city about 100 km south (~ 1600000 inhabitants), and Manosque the closest town about 18 km south from the site (~ 22000 inhabitants) (Fig. 1).

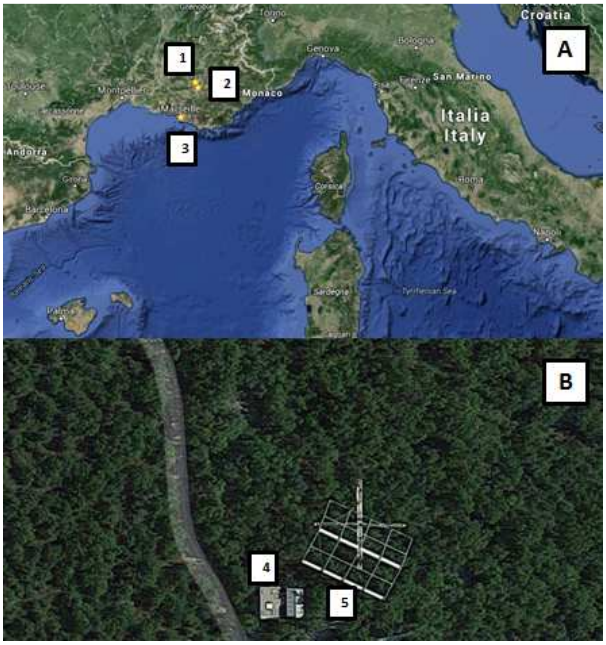


Figure 1. Site of Observatoire de Haute Provence (OHP) in the European map (panel A) and seen from above (panel B). Numbers refer to: (1) OHP field site, (2) town of Manosque, (3) city of Marseille, (4) room for instruments measuring trace gases, (5) movable trailer for measuring OH reactivity.

8)

(1) p.22054, l.16: You determined the leaf area index (LAI) during August 2010 as 2.4. Was it measured again afterwards in another year or another season? Is it not likely to change with time?

(2) No, unfortunately we do not have any more recent measurement of the leaf area index.

9)

(1) Section 2.1 Description of the field site: What was the dominant wind direction? Is the footprint of sampled air predominantly influenced by the oak forest?

(2) The dominant wind direction was north, therefore the sampled air was influenced predominantly by the oak forest. This information is included in the text.

10)

(1)

p.22055, l.7: I find the description of the set-up confusing. You have two PFA sampling lines on a mast. Are these two lines for the two instruments or for the two heights? Or do you have two lines at each height? How long were the lines? Did you have filters? Also, you write later (p.22055, l. 13) that you kept the lines heated, about 1 C above ambient temperature. So, did the line temperature vary according to the diel variation of ambient temperature? p.22055, l.21: The residence time in the lines for the CRM and PTR-MS instruments differed by about 10 seconds. Why did you not adjust these two flows to have the same residence time? Also, if you have individual lines for each instrument, did you test the two lines against each other? Can you rule out any line effect that would bias the CRM and PTR-MS observations for the comparison?

(2) We modified figure 1 to make the description of the sampling approach easier to follow. The two sampling lines are for two containers where the different instruments were placed (Fig 1, number 4 container A and number 5 container B). Sampling line directed to container A was ending with a manifold from where the instruments: PTR-MS, GC-FID, and HCHO analyser were sampling. Sampling line B was directed to a movable trailer where the instrument for measuring the OH reactivity was placed. This line was also ending with a manifold to split the line into sampling line to the zero air generator (for background pyrrole measurements) and sampling line to the reactor for ambient measurements of reactivity. Both manifolds ended with a sampling pump whose flow was adjusted in order to have about the same residence time in the two lines. We thought that a difference of 10 s was fine in this type of experiment to allow us to compare the measured and calculated reactivity. NOx, trace gases on cartridges for GC-MS analysis, and O3 were sampled through extra sampling lines. Therefore, the two lines are for the two containers. These lines had different lengths since the movable trailer (B) was closer to the mast compared to container A. Each line was jointed at 2 by a union connector, in a way that when sampling at 2 m was needed we simply had to disconnect the line at that point. Sampling line A was 30 m, while sampling line B was 18 m from the inlet to the manifold. We did not use any filter, only covered the lines to prevent rain entering inside and insect debris being sampled. The lines were heated above ambient temperature by using a thermocouple to prevent gases condensing inside the lines. The thermocouple heats up the lines by about 1 degree C and follows the diel variation of ambient temperature. Unfortunately we did not test the sampling lines against each other, therefore we cannot completely rule out any line effect that would affect our comparison. However, the two lines were used before only for measuring fluxes of isoprene and other biogenic VOC in the same forest and were both conditioned with the ambient air before the measurement campaign started. These different information will be provided in the text.

11)

(1) p.22055, l.18: C2 and C3 describe concentration levels detected during the CRM measurements and were not explained before. The CRM technique is later explained in section 2.3. Please, remove the CRM

specific terminology from this paragraph, or reorder the paragraphs, or explain it carefully e.g. as zero air and ambient measurements.

(2) This comment is taken into account.

(3) Sampling line B was used for OH reactivity measurements, here a manifold separated the main flow into two flows for measuring zero and ambient reactivity.

12)

(1) p.22055, l.25: At what height exactly did you install the extra line for sampling tubes for offline GC-MS analysis?

(2) At 2 m.

(3) In this case we used an independent extra line that was placed along the mast at 2 m from the ground.

13)

(1) p.22055, l.29: Why was the monoterpene concentration measured by the PTR-MS only every 5 minutes? Similar to comment 11), the PTR-MS technique and details about the instrumental field campaign set-up are presented later in Section 2.4.1. Hence, at this point it needs more explanation or it should be removed from the paragraph.

(2) We used a dwell time of 10 s for *m/z* 81 and *m/z* 137. We set the measurements to run full scans between *m/z* 21 and *m/z* 138, using different dwell times depending on the mass considered. A complete scan took about 5 minutes, therefore we have a data point of *m/z* 137 every 5 minutes. We removed this detail about the PTR-MS from this section.

14)

(1) p.22056, l.1-4: What height did the NOx and ozone (O3) instruments sample from?

(2) Both sampling lines were at about 4.3 m high. This detail is added in the text.

15)

(1) p.22056, l.13: Please, exchange “to take up” with “to react with”.

(2) ok.

16)

(1) p.22056, l. 22: How did you produce the zero air? Did you adjust the humidity during zero air (C2) measurements to the ambient humidity? Since the OH is generated inside the glass reactor of the CRM, its levels depend on the humidity of the sample. If during the comparison C2 (zero air+OH+pyrrole) and

C3 (ambient air+OH+pyrrole) do not have the same humidity, the OH levels inside the reactor will be different. Hence, the total OH reactivity is under- or overestimated.

(2) This comment is right. Indeed, humidity differences between C2 and C3 levels during a CRM experiment affect the concentration of OH produced inside the reactor. In our case, we split the ambient air into two flows: a flow directed to a zero air generator and a flow directed to the reactor. Therefore, the flow entering inside the zero air generator has the same relative humidity of the flow of ambient air entering the reactor. Additionally, OH is produced inside the reactor by photolysis of the water vapour from a humidified, controlled flow of nitrogen. In proportion with the other flows entering the reactor, nitrogen alone makes the half of the total flow. We saw in some cases that small differences in humidity arise even when using a zero air generator to produce zero air. For such cases a correction for humidity differences is considered on the C2 level. During this field campaign such correction had an impact on the measurements below 10%.

17)

(1) p.22057, l.5: Was the total OH reactivity obtained as average every 10 minutes? What was the time resolution of the raw values?

(2) Raw values of OH reactivity are obtained every 10 minutes which correspond to differences between C3 (5 minutes) and C2 (5 minutes). Averages within the C2 level before and after every C3 are also considered to minimize differences between background reactivities. Depending on the settings used to perform the measurements we have between 5-10 points of m/z 68 during each stage of C3. A few points for each stage are discarded due to valve switching effects (usually the first and last point are discarded), whereas the remaining points are averaged to obtain a value of C3.

18)

(1) p.22057, l.9: Generally it would be more accurate to write “OH reactivity”. There are other oxidants such as O3 or nitrate radicals (NO3) that react with most of the atmospheric constituents as well.

(2) This is right. The comment is considered.

19)

(1) p.22057, l.12: You write that propane represents a proxy of an unknown air mass. Why is that? I would think that the average reaction rate with OH of an air mass depends on its origin. A biogenic air mass might contain more reactive compounds (such as isoprene, monoterpenes) than an anthropogenic air mass (for which carbon monoxide (CO) or nitrogen dioxide (NO2) dominate).

(2) We wrote this comment because propane reacts with OH with a constant of reaction on the order of 10^{-12} molecules/cm^3*s, which is an averaged order of magnitude when a number of different reactive molecules are considered. However, the statement is not precise since as Anonymous Referee is pointing out, this strictly depends on the origin of the air masses. For this reason this statement is removed from the text.

20)

(1) p.22057, l.15-21: The correction described in this paragraph differs from previously published CRM studies (e.g. Sinha et al. 2008). Therefore, it would be good to include the graph that is described here either in the manuscript directly, or as supplementary information.

(2) A graph describing this correction is included in the supplementary information.

21)

(1) p. 22057, l. 20-21: How large were these two corrections relative to the measured total OH reactivity? It would be interesting to learn about the relative impact of these corrections.

(2) Both corrections used for processing the data decreased the values of the raw reactivity. The correction for humidity differences led to a 13% decrease of the original values, while the correction for kinetics led to a 47% decrease. Generally, the correction for humidity has a small weight, if some measures for minimizing the differences in humidity between C2 and C3 are considered, as in the case of using a zero air generator to produce zero air, for instance. The weight of the kinetics correction depends on the experimental conditions adopted in the reactor. Specifically, when operating in a window of pyrrole/OH within 1-3 the agreement within reactivity measured and reactivity injected during a calibration with a certified test gas responds better, hence smaller corrections are needed. For our specific case, we encountered a larger correction factor for the kinetics regime in this field work, due to a lower concentration of OH formed inside the reactor, hence a larger pyrrole/OH ratio.

22)

(1) p.22057, l. 23-26: Here you talk about the calibration of the PTR-MS instrument that was used for pyrrole detection. The instrument was calibrated in dry and wet conditions. You should briefly explain why this is necessary, and refer to Sinha et al. (2010) who pointed out that the PTR-MS sensitivity to pyrrole depends strongly on humidity.

(2) Yes, this is added in the text.

(3) Calibrations of the PTR-MS for pyrrole dry and wet were carried out at the beginning and end of the field campaign and showed a very good agreement between each other (difference within 1 % for the dry calibration factor and 4 % for the wet calibration factor). It is necessary to calibrate pyrrole at dry and wet conditions due to differences in sensitivity reported by the PTR-MS operating at different ambient humidity (Sinha et al., 2010).

23)

(1) p. 22058, l. 11: This is not a sentence.

(2) it is corrected.

(3) with i being any measured compound listed in Table 1.

24)

(1) p.22058, l.23 and p.22059, l.18-24: The PTR-MS was calibrated with a standard gas mixture containing several volatile organic compounds. Particularly, m/z 71 was calibrated with crotonaldehyde. The atmospheric signal, however, was identified as isoprene products including methacrolein (MACR), methyl vinyl ketone (MVK) and isoprene hydroperoxides (ISOPOOH). First, I wonder if the PTR-MS sensitivity would be different for these compounds. Then, I think this section could be improved by explaining at first that m/z 71 typically was assigned to the sum of MVK and MACR, and only recently the mentioned interference of ISOPOOH was discovered. Could you please modify this part in the text?

(2) The calibration with crotonaldehyde instead of methacrolein and methyl vinyl ketone is performed due to logistical reasons and similarities of the molecules. First, it is easier to have and transport a certified gas standard of crotonaldehyde compared to MACR and MVK. Secondly, the molecules have similar chemical properties and the same mass, so it is often used crotonaldehyde for calibrating m/z 71 (see also the article of Kalogridis et al., 2014). However, we think that MACR MVK and ISOPOOH might have among them slightly different sensitivities. As far as we know, there are no studies available in literature about this, and it would be something interesting to know. Yes, the part related to the ISOPOOH is modified in the text.

(3) Previous studies highlighted the presence of isoprene hydroperoxides (ISOPOOH) fragmenting at m/z 71 in the PTR-MS, and representing a major yield from isoprene oxidation for low NOx environments, such as our case study (Liu et al., 2013 and Rivera-Rios et al., 2014). Since we did not separate between these compounds, we will therefore refer hereinafter to m/z 71 as the sum of the isoprene oxidation products ISOP.OXs: methylvinyl ketone (MVK)+methacrolein (MACR)+isoprene hydroperoxides (ISOPOOH).

25)

(1) p.22059, l.2: The PTR-MS sensitivity depends on humidity for most of the compounds in the standard gas mixture used for calibration. Were the calibrations performed in dry or in wet conditions? Did you try to match typical ambient humidity levels?

(2) The calibrations of the PTR-MS were performed at ambient humidity levels, since the certified gas standard is diluted in cleaned ambient air.

26)

(1) Could you please check the acronyms used in the entire manuscript? Generally, I found that sometimes they were not introduced properly or differ from how they are used later. For example: p.22058, l.13 “PTR-QMS” is later “PTR-MS” (p.22059, l.14). Or: “MEK” (p.22059, l.25) is not defined when first mentioned as methyl ethyl ketone. Similar later for formaldehyde: “HCHO” (p.22060, l.19).

(2) Ok.

27)

(1) p.22060, l.6: Losses are typically enhanced by stainless steel tubing and Nafion dryers. Did you test your set-up for losses?

(2) Yes. Some tests were performed to check losses due to stainless steel tubing and the Nafion dryer. Specifically, no losses were observed for VOCs, except for a small contamination occurred for isobutene (10 pptv) due to the Nafion dryer (see also the article of Bonsang et al., 2008).

28)

(1) p.22062, l.3: The samples for GC-MS offline analysis were stored at 4C and analyzed within a month in the laboratory. Did you test if there are any losses of your compounds of interest during this procedure? How large do you estimate such losses?

(2) Unfortunately, we have not conducted any specific test for demonstrating if this procedure can lead to any losses of the compounds of interest for our study. However, ozone was removed from the tubes prior to the sampling procedure by using MnO2-coated copper nets, according to the procedure reported in Larsen et al., 1997. Larsen and coauthors highlighted that for mixing ratios of ozone larger than 8 ppbv, losses are generally reported for more reactive monoterpenes as limonene for instance, a compound also measured in our study. This information is added in the text.

29)

(1) p.22062, l.12: It is really unfortunate that CO was not measured during the field campaign. I would think that usually atmospheric CO levels are quite variable and dependent on transport processes and regional pollution. Since you had to use the average of springtime measurements, could you as well report how variable the atmospheric CO levels were in spring 2012? Did you include this variability in the uncertainty of the total OH reactivity calculation?

(2) Yes, this information is added in the text and in the calculation of the uncertainty for the OH reactivity calculated. CO concentration was variable of 16 ppbv within the period 23/05/2012 to 18/06/2012 which corresponds to a mean value of OH reactivity of 1.02 ±0.09 s-1.

30)

(1) Section 3.1 Trace gases profiles and atmospheric regime: I do not understand the meaning of this title. Do you mean diel profiles? Vertical profiles? And what do you mean with atmospheric regime?

(2) The title of this section is probably too unclear, therefore it is adjusted to “trace gases mixing ratios” and we thank the referee for pointing this out.

31)

(1) p. 22063, l.11: “: : :isoprene covariates perfectly with PAR” Could you please provide the correlation coefficient?

(2) Isoprene mixing ratio does not covariate perfectly with PAR, due to the fact that the two parameters are not measured exactly at the same height and place, hence the perfectly is removed from the text.

32)

(1) p.22063, l. 13-15: To be precise, you could add the exact hour of the day when the peaks occur. If you do so, I think that isoprene above the canopy would peak at about 20 CEST (Fig. 2). This is after sunset. Can you explain why isoprene has its maximum that late in the day above the canopy?

(2) Yes, the maximum peak of isoprene concentration occurred at 20 CEST above the canopy. This is driven by the profile of isoprene concentration which was slightly shifted on 08/06/2014. Indeed if the daily median and not the daily mean is considered the concentration of isoprene would have a peak at 15 (CEST) in the early afternoon. We think that this peak of concentration of isoprene so late in the day is a result of the higher temperature of ambient air in the late afternoon occurred on 08/06/2014 (the profile of temperature is reported in Fig.6).

33)

(1) p.22064, l.8 and 10: You present the daily maximum inside the canopy for methanol and isoprene as 14 ppbv and 23 ppbv. Are these values averages?

(2) No. These numbers represents the maximum values of the daily maximum peaks reported during the days of the campaign we measured inside the canopy.

34)

(1) p.22064, l.12: What are 24h statistics? I am not familiar with this terminology.

(2) This terminology was used to allow an easier comparison with the table presented in the previous article where concentrations and fluxes in the same forest were measured (Kalogridis et al., 2014). It would mean that the values reported herein are obtained when all data points, measured during daytime and nighttime, are considered. The term is changed in the text.

35)

(1) p.22064, l.21: Please, add ": : :with ethane being the most abundant: : :"

(2) Yes.

36)

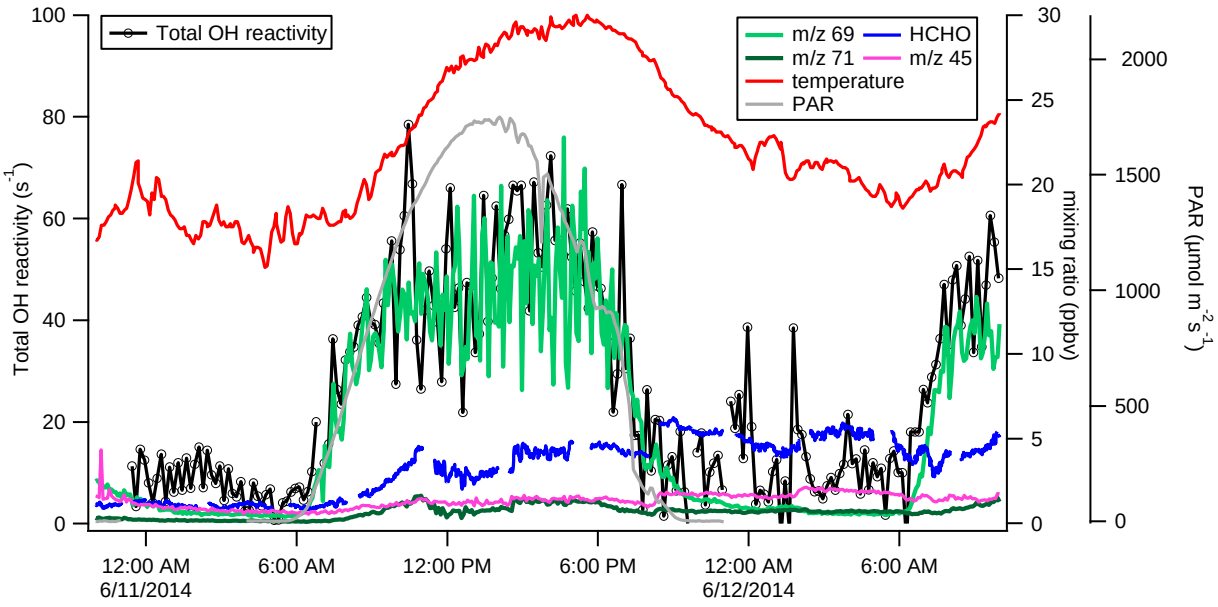
(1) p.22065, l.19-28: Could you make clear which height was sampled from at which day? So, during 11 June 2014 the instrument measured from 2m. You describe the temporal variation of total OH reactivity with two maxima during daytime. If you look at it differently, it could be a slight drop around noon. Could it be that at noon, when light intensity and temperature levels are at their maximum, the oaks favor photorespiration and tend to close their stomata? This would reduce the direct emission of reactive species at midday. Interestingly, the shape of total OH reactivity during daytime above the

canopy differs slightly from within the canopy. It follows less the diurnal cycle of light and temperature, but gradually increases during the daytime to reach peak levels at about 21 CEST. Could you comment on that?

(2) Yes. At 2 m (inside the canopy): 31/05/2014, 01/06/2014, 02/06/2014, 03/06/2014, 04/06/2014, 05/06/2014, 06/06/2014, 11/06/2014, 12/06/2014.

At 10 m (above the canopy): 29/05/2014, 30/05/2014, 07/06/2014, 08/06/2014.

Therefore, the OH reactivity on 11/06/2014 was measured at 2 m, inside the forest. Its profile covariates with isoprene concentration and it could be speculated that the drop in OH reactivity reported around noon on this day is a result of reduced direct emission of reactive species due to enhanced levels of light intensity and temperature. This effect could actually be possible, but not directly associated to isoprene, neither due to maximum levels of temperature and PAR. Indeed, as the figure below shows, the OH reactivity profile follows well the one of the concentration of isoprene, which is depending on light and temperature. However, light and temperature maximums are slightly shifted after the mentioned drop. Nicely, isoprene does not show the same first peak of reactivity (10.30 CEST), whilst oxygenated molecules might be more the possible cause of it. Therefore it looks like that a drop in concentration might happen for the oxygenated molecules, rather than for isoprene. However, isoprene is also the dominant species emitted by this type of oak, therefore if this effect is associated to the oak leaves then we would see a clearer drop in isoprene concentration as well. In conclusion, we do not think we have enough elements from our study to point out this physiological effect.



37)

(1) p.22066, l.22: How did you estimate 25% uncertainty for the calculated total OH reactivity? This value seems very small with respect to the high uncertainty every single reaction rate constant has.

Additionally, the CO level was assumed to be constant which probably further increases the uncertainty of this value. It would be interesting to add a small paragraph about the uncertainty of the calculated OH reactivity to the experimental section.

(2) The error on the calculated value of OH reactivity is estimated by considering the propagation of the 1 sigma uncertainty on the rate constant of reaction between OH and the measured compound and the 1 sigma uncertainty associated to the concentration of the measured compound. The variability on CO adds very little to the overall uncertainty, being the mean reactivity of CO estimated to 1 s^{-1} with a variability of about 8%. More explanations on how the error is calculated are added to the text.

38)

(1) p.22067, l.3-p.22068, l.6: This is a very interesting paragraph. But it is written in a way that is difficult to understand. Could you please simplify and be more specific with the presentation. For example l.13-14: Why do you assume equal OH reactivity above and within the canopy? And l.16: I am not familiar with the expression "9 point percentage". p.22067, l. 15: Please, write correctly 30_C and 32_C. p.22068, l.3: Erase "definitely".

(2) We thank Anonymous referee 1 for pointing out these difficulties. This part is rephrased and the suggestions indicated are taken into account.

39)

(1) p.22068, l.25: You write that isoprene concentrations flattered much faster than its oxidation products. As reason you refer to its higher reaction rate. Most probably, it is as well the close proximity to the source, that causes the high variability. As a measure for short term you could provide the relative standard deviation (see e.g. Noelscher et al. 2012, ACP).

(2) The relative standard deviation is provided in the text.

40)

(1) p.22069, l.2: You state that ISOP.OXs/isoprene anticovariate with ozone as can be seen in Figures 9 and 10. I can see in Figure 9 that the increase of ISOP.OXs/isoprene falls together with the decrease in ozone. However, I cannot see such a behaviour in Figure 10. Therefore, I think it is too strong to talk about an anticorrelation. Can you provide a correlation coefficient of ISOP.OXs/isoprene with ozone to prove an anticorrelation?

(2) We thank the referee for this comment, indeed the term anticorrelation is too strong for these profiles and for this reason it is removed from the text.

41)

(1) p.22069, l.2 and Figures 9 and 10: Ozone levels are relatively high (about 50 ppb) for a forested environment. Do you have any explanation for such high ambient ozone mixing ratios?

(2) These high values do not derive to any process occurring in the forest, and are usually registered in this specific forest due to the proximity of urban sites as Marseille, and closer, Manosque.

42)

(1) p.22069, l.9 and following paragraphs: In order to explain the missing reactivity that was observed during two nights, you point to nighttime chemistry or lateron to surface oxidation reactions. Could you please also discuss the effect of boundary layer dynamics such as dilution or enhanced deposition during nighttime? What about humidity dependent emissions from the soil or microbial communities on leaves?

(2) Yes these effects are better discussed in the text.

43)

(1) p.22071, l.1: I do not think that forest environments may be called “perfect laboratories”. The experiment examining the total OH reactivity in air within or above a forest is not a controlled study. Often even laboratory work is by far not perfect as the technical set-up, instrumental limitations, or other unknowns bias the scientific results.

(2) This is also modified in the text.

44)

(1) Table 3 presents mean mixing ratios of the PTR-MS detected compounds. If you additionally include the standard deviation or standard error, you could emphasize the variability of the total OH reactivity.

(2) yes the standard deviation is added in Table 3.

45)

(1) Figure 1 shows the measurement site. Could you please point to the cities mentioned in the text? And could you also show where the instruments and the inlet set-up are located in the oak forest?

(2) Yes, Figure 1 is modified.

46)

(1) Figure 2: The diel mean (?) profiles of isoprene, isoprene oxidation products, formaldehyde, PAR and temperature are presented for inside and above the canopy. At which height was the temperature measured?

(2) Yes, figure 2 represents the diel mean profiles. The temperature was measured by two sensors, one placed at 2 m (used for the left panel, inside the canopy) and the other one placed at 6.15 m (used for the right panel, above the canopy). This information is added in the figure’s caption.

47)

(1) For several figures, please make sure that the labels or description text is not moved into axes or figure elements (such as in Figure 3, 7 and 8).

(2) Yes, the figures are adjusted.

48)

(1) It would be nice to point out at which height the presented data was measured. Similarly as in Figure 2, could you please add a box with “inside/above canopy” to the Figures 5, 6, 7, 8, 9, and 10?

(2) Yes. The figures are modified for this.