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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical Note: Formal blind intercomparison of HO_2 measurements in the atmosphere simulation chamber SAPHIR during the HOxComp campaign" *by* H. Fuchs et al.

H. Fuchs et al.

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We thank referee #1 for her/his extensive comment. In the following we give a step by step answer to the comments.

Specific comments:

COMMENT: The structure of the paper is a bit erratic concerning the separation between the description of the experiments/data sets, and the analysis of the actual results. As a consequence, the interpretation of results is distributed in different sections and makes the interpretation of the overall picture much more complicated. For instance on page 21200 (lines 6-15) there is a description of diurnal profiles of trace

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Discussion Paper

gases which does not fit within the merely description of the instruments/experiments in section 3. Therefore this should be included as such in section 4.1, where the diurnal profiles are analysed. Similarly, after section 4.1 which seems to start dealing with the interpretation of the results of the measurements, the section 4.2 and 4.3 are just describing the signal data sets (temporal resolution, number of points etc) which seem to fit more adequately in section 3. On the other hand, in the section 4.1 the results of the ambient air sampling period are roughly analysed but it is completely missing any reference to the differences observed in the absolute mixing ratios measured by different LIF instruments in different days, which is a remarkable feature of these results. This is discussed in section 5.3 which could be combined with section 4.1. in order to avoid redundancies. I strongly recommend the authors to revise the structure in that sense, so that the presentation and discussion of results gains in concision and clarity.

RESPONSE: Section 4 of the manuscript is the description of results of the campaign whereas section 3 deals with the experiments giving only information about what was done during the campaign. Consequently, we did not include results of the measurements such as the statistical description of HO_2 measurements in section 3, but in the results section. The reviewer is correct the paragraph page 21200 lines 6-15 may fit better in section 4.1. As suggested by the reviewer we integrated this paragraph into section 4.1 with small modifications to avoid redundancies. Section 4.1 does not aim to discuss differences between measurements, but explains qualitatively the diurnal profile of HO_2 concentrations during the campaign. The description of differences between measurements is given in the statistical analysis in section 4.4 for SAPHIR experiments and ambient air sampling and they are discussed in section 5. Merging section 4.1 and 5.3 would mix up the pure description of the diurnal profile and the discussion why differences between measurements may have been occurred. To be consequent, a similar structure would be required for SAPHIR experiments, so that section 4 and 5 would become one section. We thought to structure the manuscript in this way, but we think that a clear separation of results and the discussion of results makes reading of the manuscript easier. In our point of view, the statistical analysis concerning precision of measurements and their linear correlation is part of the result of campaign. In contrast, the discussion and investigation of potential reasons for differences between measurements are part of the discussion of results. Although we understand the concern of the reviewer that the separation of the two sections requires some redundancy, we still think that this is adequate and the amount of redundancy is acceptable.

COMMENT: Abstract: "Measurements in ambient air...This is most likely caused by sampling different air masses at the slightly distant locations of the instruments". Do these sentences try to explain the differences in the regression parameters between SAPHIR experiments and ambient air on the basis of the different composition of ambient air respect to chamber air? If so, I do not understand the second sentence referring to the slight distant location of the instruments. If not, and this second sentence only refers to the differences in the LIF regressions parameters obtained from air ambient measurements, the authors seem to believe that the differences in mixing ratios measured by different instruments are real because the ambient air masses sampled by each instrument are different. The location of the instruments is though similar and at the same height. Do you have any special reason to believe that instead of having instrumental issues to solve (potential losses, interferences, inaccuracies, etc), the composition of the air close to the instruments presents such variability? If so, please specify. Please rephrase both sentences to prevent misunderstanding.

RESPONSE: The distance between locations of instruments was up to several meters during ambient air sampling. Local sources and sinks from traffic, trees and buildings close to the measurement site may have had an impact on a short timescale, so that differences in measurements could have been real, although we do not have evidence for this, because there was no detailed characterization of meteorological parameters, We cannot exclude that also instrumental issues led to the larger differences between measurements observed for ambient sampling compared to SAPHIR experiments. We rephrased this sentence to "This could have been caused by differences in HO_2 concentrations in the sampled air at the slightly different locations of instruments." and also on page 21213 line 28: "Observations for OH indicate that the ambient air was inhomogeneously mixed (Schlosser et al., 2009), which may explain at least some of the differences in the measured HO_2 concentrations. Sources and sinks for trace gases such as vegetation were close to the instruments. Buildings and vegetation surrounded the measurements site, so that the incoming air flow was disturbed and potentially not homogeneous."

COMMENT: Page 21201, line 2. As the formation of HONO and HCHO in the chamber walls can be an important interference for HO_2 measurements, the manuscript would benefit from a brief description of the characterization of this interference in addition to the literature cited and from a short evaluation of the potential consequences for the intercomparison campaign, if any.

RESPONSE: HONO and HCHO are formed in the chamber and are sources for OH and HO_2 radicals, so that they determine the radical concentrations in the chamber. They are not interfering species for the detection of OH and HO_2. The lifetime of HONO and HCHO in the chamber is larger than the mixing time, so that the source of radicals is homogeneous within the chamber. Since we do not intend to explain the level of HO_2 in the chamber quantitatively, a larger description of the characterization of these sources is beyond the scope of this publication. We added on p. 21202 l. 5: "Since HONO and HCHO are long-lived species (approximately 15 min for HONO and 1 hour for HCHO) compared to the mixing time (few minutes) in the chamber, spatial gradients of HOx radicals are not expected from these radical precursors."

COMMENT: During the experiment performed on the 21th the agreement between LIF-signals is reasonably good until around 11 am. From that moment it is quite remarkable the relative variability of the signals and that the signal of the MPI-LIF decreases respect to the others while it increases notably under dark conditions. According to the figure 4, CO is already added in higher concentrations at about 9:00 hours but according to page 21201 line 18 and to Schlosser et al., 2009 (figure 5) this happened at

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Discussion Paper

11:00h. Please correct the figure 4 if necessary. In the case of the discrepancies being related to the addition of CO, please discuss how this can alter the relative pattern of the LIF systems (differences in the detection cells, flows, OH/HO_2 conversion, position in the chamber, etc).On the 18 July (page 21201, line 15) 800 ppb CO were also added to the chamber (please include the time) and the LIF-FZJ mixing ratios also remain remarkably higher as the others, whereas the difference decrease with NO increasing. Please comment on this.

RESPONSE: We thank the reviewer for noting the contradicting plots. Indeed, CO was injected at 11:00. Figure 4 was corrected. The reviewer also noted correctly that there is a change in the relationship between measurements coincides with the addition of CO. The relationship between data at high CO is similar to what is observed for comparable conditions on other days. However, measurements by FZJ-LIF are lower before CO was added compared to relationships on other days, so that this behavior does not point into the direction that CO itself impacts the instrument sensitivity. There is no impact of CO on the flow in the detection cells of instruments and there is no effect regarding the position of instrument inlets in the chamber. (1) The lifetime of OH with respect to CO in the low pressure cell is approximately 200ms much longer than the time for the conversion of HO_2 (in the order maximum few ms) in the instruments, so that CO does not influence the conversion efficiency of HO_2. Even for steady state conditions in the cycling between OH and HO_2 via CO and NO the maximum OH concentration would be approximately 2% for the largest NO concentration used in the instruments (for 500ppmv CO). (2) The mixing time in the chamber is within the range of minutes, so that the process of adding CO does not lead to gradients in the HO_2 concentration in the chamber. We addressed the concerns of the reviewer by adding on p. 21210 after line 22: "During the first part of the experiment on 21 July (ambient air in the chamber) measurements by FZJ-LIF are slightly lower than measurements by MPI-LIF in contrast to observations on other days with similar water vapor concentrations. This pattern changes when CO (500 ppmv) was added at 11:00 when FZJ-LIF shows again larger values. No reason could be identified, why CO would change the pattern,

Full Screen / Esc

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Interactive Discussion

Discussion Paper

since the concentration of CO was small enough not to affect the conversion of HO_2 in the detection cells of instruments."

On 18 July CO was added at the beginning of the experiment (we add the time in the text and in Table 2). In our point of view the relative pattern of measurements is more related to water vapor than to CO, because FZJ-LIF and FRCGC-LIF were also higher than MPI-LIF at low water vapor concentration the day before, but FRCGC-LIF was significantly lower than MPI-LIF at high CO on 21 July. The correlation between measurements by FZJ-LIF and FRCGC-LIF was similar over the course of the day and did not change significantly with increasing NO with the exception of the first period of illumination, for which we could not find an explanation. For the rest of the experiment, the difference is similar to the difference observed the day before, when water vapor concentration was within the same range, so that we concluded that there is no NO depending difference between measurements of these two instruments. Both instruments show a changing correlation with respect to measurements by MPI-LIF over the course of the day with larger differences at low NO. However, there is no consistency with other experiments, when NO concentrations were similar such as during experiments on 19, 23 July (low NO, but good agreement between FZJ-LIF and MPI-LIF). Therefore, we do not think that it is justified to draw any conclusion from the experiment on 18 July with respect a dependence of instrument sensitivities on NO. We added on p. 21210 after line 22: "For example, water vapor concentration on 18 July was similar to the low concentration steps on 17 July, and within the range of higher concentration steps on the other days. Whereas the relationship between measurements by FZJ-LIF and FRCGC-LIF on 18 July is consistent with results from 17 July (with the exception of the first illumination period), differences between HO_2 by MPI-LIF and the other two instruments are decreasing with increasing NO concentration that was varied in this experiment. The reason for this behavior is not clear, but is most likely not caused by the addition of NO, because observed differences between MPI-LIF and the other instruments on 18 July are not consistent with difference observed on other days at similar NO concentrations (e.g. 19, 23 July for low NO)."

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

COMMENT: Page 21205, lines 25-28: "...(e.g. 17 July) which exhibit relatively low R2 values". This is actually only true on the 17th for the correlation between LIF-MPI and FRCGC-LIF.

RESPONSE: We rephrased "It is evident that the overall spread of data is not completely represented by the assigned statistical error bars, but systematic effects seem also to play a role. For example, a clear separation of data points (indicated by color code) can be seen in Fig. 9 for those experiments at SAPHIR (e.g., 17 July) which exhibit relatively low R2 values. It is also noteworthy that the scatter plots of individual days are more compact than that of the entire data set." to "It is evident that the overall spread of data is not completely represented by the assigned statistical error bars, but systematic effects seem also to play a role. However, the scatter plot shows also a narrow distribution for single experiments (e.g. 18 July)."

COMMENT: The agreement in the ambient values measured by LIF-FZJ and FRCGC-LIF is remarkable except for the nocturnal period on the 10th to the 11th, where both LIF-MPI and LIF-FZJ measured quite high mixing ratios. As stated in the text (page 21206, line21), LIF-FZJ and FRCGC-LIF again agree on the following day. However, might the implementation of the additional LIF-FZJ conversion reactor introduce a kind of nocturnal interference of similar origin as the unknown interference in the nocturnal signal of the LIF-MPI? Please comment on this.

RESPONSE: It may be possible that interferences caused affected the modified FZJ-LIF which may not be observed for the configuration without the additional reactor. We rephrased "During this night the FZJ-LIF was operated with the additional ROx converter. However, this was continued during the following day when measurements between FZJ-LIF and FRCGCLIF were in good agreement like the days before." to "During this night the FZJ-LIF was operated with the additional ROx converter. Although during the following day FZJ-LIF the patterns of relationship between the instruments were again similar to the days before, nighttime measurements by FZJ-LIF may have been affected by the ROx converter."

COMMENT: As the values of MIESR are rejected and LIF Leeds had a failure and could not participate, this blind intercomparison lacks of an absolute reference while the number of different instruments involved reduces considerably. In that context, the HO_2 *mixing ratios measured by LIF-FZJ ambient remain systematically and significantly lower than the LIF-MPI values, while in all the experiments performed in the chamber, LIF-FZJ chamber measures systematically and significantly higher absolute values than the others (figure 7 and page 21206, lines 23-28, section 5.3). Therefore, an intercomparison of both LIF-FZJ in the chamber is essential to assure 3 that both systems are equivalent, in order to be able to analyse the ambient and chamber data sets as a whole. Please comment on these differences and include such a comparison. Although based on the same principle slight differences in the set up might change the individual performance under different sampling conditions which are not covered by the calibration source. Such an intercomparison can also provide decisive information to rule out effects derived from the position in the chamber.*

RESPONSE: The reviewer is correct that a direct comparison of both LIF instruments of the FZ Jülich would be of interest. One may think of other experiments which would aim to resolve the observed differences between measurements. However, these were not part of the HOxCOMP campaign. This publication does not aim to include any further investigations which were not part of the campaign, but only reports results from this particular campaign. Furthermore, we have no concerns regarding the sampling position of instruments in the chamber. The comparison of OH measurements (Schlosser et al., 2007, 2009) and other instrument comparisons (Fuchs et al., 2009) demonstrated that trace gases are well mixed in the chamber. Although the construction of the two instruments and operational parameters are similar and both share the same calibration source, we do not claim in the manuscript that the two instruments are 100% equivalent and do not compare e.g. ambient air measurements by FZJ-LIF to SAPHIR measurements by MPI-LIF.

COMMENT: Discussion: page 21207, line 20: ”systematic differences....”. Are you *talking about systematic differences between ambient and SAPHIR relative patterns, or only within ambient or within SAPHIR results? It seems to be already clear from the analysis of the data in previous sections that there are no obvious systematic differences between measurements when being analysed as a whole. Therefore (see general comment above) it is necessary to proceed straight forward to the classification of differences according to sampling conditions as made in section 5.1 and 5.2, and in figures 10-11. I recommend shortening the text of this first part of the discussion (see also next comment). In that context, a discussion of the results/discrepancies observed by changing NO in the chamber is completely missing.*

RESPONSE: In the first paragraph of the discussion possible reasons for the observed differences between measurements are given. We think that it is necessary to discuss potential explanations for the observations first and to rule out some of them before discussing particular points in detail in section 5.1 and 5.2. As pointed out in above, the comparison of measurements do not give hints that there is a dependence of the relationship between measurements on NO. This is now mentioned (see above). We also think that the structure of the manuscript with a clear separation of results and discussion is helpful for the reader (see response above).

COMMENT: Page 21208, line 5: ”where the same LIF instruments and a DOAS instrument showed good absolute agreement ....Schlosser et al, 2009” and line 16: ”since the comparison of OH measurements does not exhibit a day-to-day variability as observed for HO_2...”and line 24 ”again the good agreement.....”. I do not agree. Taking a closer look at the data published by Schlosser et al., there can actually be identified clear similarities for OH and HO_2 in the pattern of the relative discrepancies of the mixing ratios provided by the same instruments. For instance, on the 19th the agreement between instruments is remarkably better than on the 17th and 18th. On the 17th the LIF MPI data also seem to reach a better agreement with the other LIF data along the day for increasing H_2O mixing ratios. Similarly, during the ambient measurements the MPI-LIF values remain clearly higher than the measured by the other instruments

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

(both in OH and HO_2). On the other hand, there are other interesting features: The FRCGC OH data remain generally systematically higher while the FRCGC HO_2 data are systematically lower than those of the other instruments. Please improve this argumentation.

RESPONSE: The statistical analysis of OH data did not reveal any systematic differences between measurements by DOAS and LIF on water vapor, ozone or NO as shown in Figure 8 in Schlosser et al., 2009. This also holds also for two LIF instruments. For example, on 17 July the median of the difference between DOAS and LIF is small for the first water vapor concentration, but much larger for the second. Differences between FRCGC-LIF and MPI-LIF are largest for the second water vapor concentration, but are largest between FZJ-LIF and MPI-LIF for the third. Although they become again smaller for the last water vapor concentration, they are still larger compared to the first. Figure 8 in Schlosser et al., 2009 also shows that differences between LIF-measurements are within the same range for 17, 18, 19 July. One may get the impression that the agreement on 17 July is worse, but this is only true for the second water vapor concentration, when FRCGC-LIF values are significantly larger than those of the other two LIF instruments. However, the agreement is similar to what is observed on the other days for the other water vapor concentrations. Furthermore, MPI-LIF and FZJ-LIF measurements agree well on this day. The maximum difference is observed for the highest water vapor concentration (in contrast to what is seen for HO_2), but this difference is similar e.g. to the difference for the second ozone concentration on 19 July. We added on p. 21208 l. 8: ”Neither the correlation plot between OH LIF measurements (Figure 7, Schlosser et al., 2009) nor the box and whisker plot of differences between OH DOAS and LIF measurements for SAPHIR experiments (Figure 8, Schlosser et al., 2009) exhibit significant dependencies between measurements on the particular experiment or concentration levels of H_2O, O_3 or NO.”

The reviewer is correct that the pattern of differences between measurements is more similar for OH and HO_2 during ambient air sampling, but arguments on p. 2108 to

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

which the reviewer refer are only related to SAPHIR measurements (l. 5). Therefore, we rephrased on p. 21213 l. 27: "...but is only partly observed. Although MPI-LIF values are larger for ambient air sampling for both, OH and HO_2, differences between HO_2 concentrations are larger compared to OH."

The reviewer is correct with his/her observation that there is a difference between the agreement of OH and HO_2 by FRCGC-LIF for SAPHIR experiments. This supports that there is no common error in the calibration procedure of the sensitivity of OH and HO_2. We added on p. 21208 l. 19: "Furthermore, during SAPHIR experiments OH measurements by FRCGC-LIF are slightly larger than those by MPI-LIF and FZJ-LIF, whereas HO_2 values by FRCGC-LIF are generally smaller. This again supports, that there is no shared error between calibration of the sensitivities for OH and HO_2 for this instrument."

COMMENT: Page 21209, line 11: "agree well when the chamber was illuminated". Actually this is not true for the FRCGC- HO_2 data which are systematically lower than the others, whereas they agree with the LIF-FZJ data in the dark periods.

RESPONSE: We rephrased the sentence to: "The linear correlation between measurements by the three LIF-instruments is high and MPI-LIF and FZJ-LIF measurements are in agreement when the chamber was illuminated, ...".

COMMENT: Page 21210, line2 11-14: As stated in the text, MPI-LIF already mentioned in Schlosser et al. 2009, the existence of an interference under dark conditions which at the time of publication was being investigated. Is it possible to update the results/ conclusions of this investigation?

RESPONSE: Unfortunately, there are no final results regarding the interference in the dark. Further investigations of potential interferences will be part of a separate publication as soon as investigations will have been finished.

COMMENT: Page 21212 line 9, to (1): Actually the OH data in Schlosser et al, 2009 *show similar discrepancy pattern at lower [H_2O]. to (3) cannot this actually be a source of uncertainty in the correction of the FRCGC-LIF as it bases on empirical data at low H_2O and on quenching calculations at higher H_2O?*

RESPONSE: On 17 July OH and HO_2 measurements by FRCGC–LIF are larger than those by FZJ-LIF, but OH values by MPI-LIF are larger than those by FZJ-LIF in contrast to HO_2, which is significant lower. Furthermore, the differences between measurements do not show any systematic pattern depending on the water vapor concentration as can be seen in the box and whisker plot (Figure 8, Schlosser et al, 2009).

The FRCGC-LIF data are corrected for fluorescence quenching for the entire range of water vapor concentrations (p 21211, l. 28-29). The validity of this correction was checked during the campaign for low water vapor concentrations, but laboratory measurements showed that this is also valid for higher water vapor concentrations (p 21212, l. 1-2). Furthermore, one may expect worse agreement with measurements by the other two instruments for high water vapor, if the disagreement was due to the uncertainty of the calculated correction factors that were not checked during the campaign.

COMMENT: Page 21212 line 24: "at high humidities (>0.6%).....", Is the discrepancy at about 1.2% H_2O within the stated accuracies? Please state the value of those accuracies.

RESPONSE: The state includes the discrepancy for water vapor mixing ratios larger than 0.6% including those at 1.2%. Accuracies are given in Table 1. We rephrased "At high humidities (>0.6%), however, all instruments agree within the stated accuracies and exhibit very good linear correlations." to "At high humidity (0.6-1.8%), however, all instruments agree within the stated accuracies (Table 1) and exhibit very good linear correlations."

COMMENT: Page 21213, line 28 "which indicates that the ambient air was inhomogeneously mixed" Any special reason for this? (see comment above for Abstract). Also on page 2125, line 27: "the data set of ambient air was most likely influenced by inho-

mogeneities.....”

RESPONSE: As said in the response above, we cannot exclude other reasons. We rephrased this sentence on p. 2125 l. 27 to ”the data set of ambient air could have been affected by inhomogeneities....”

COMMENT: Page 21214, line 19, there are some pattern similarities with the OH concentrations reported by Schlosser et al., see comments above.

RESPONSE: We addressed the concerns of the reviewer by the response to his/her comments above.

COMMENT: Page 21214, line 25: ” was well characterized for this campaign” Actually the description given on page 21211 seems to lead to the conclusion that the experimental characterization was not considered for the corrections but partly replaced by theoretical considerations.

RESPONSE: The point is that all participants were aware of the fact that the sensitivity of instruments depends on water vapor concentration. Theoretical considerations were in agreement with characterization measurements, but both are much smaller than the observed discrepancies. We rephrased the sentence: ”... is well-known and was characterized for all instruments.”

COMMENT: Page 21216, line 6-11: As states in the manuscript, the LIF community was already aware of the potential effect of O_3 *and* H_2O *on the LIF radical determination. Please emphasise more clearly the concrete contribution of this intercomparison to the further understanding of these effects.*

RESPONSE: Effects of ozone and water vapor on the sensitivity of LIF instruments are well-known and understood. They were carefully investigated in laboratory measurements. Therefore, the results of this comparison do not question these effects. However, results of the campaign show that there are additional effects. We added on p. 21215 l. 1: ”Therefore, results of this intercomparison do not question the effect of water vapor quenching." l. 22: "...signal observed here, so that ozone photolysis most likely did not cause the observed interference. Further investigations are required to clarify the reason for the observations."

Minor comments:

COMMENT: Please include somewhere in the text the year of the HOxComp intercomparison. The reader should get this information without having to look for it in other references (otherwise it only appears in the x axis of the figure 2). In that respect, the sentence on page 21193, line 23 " after the HOxComp campaign in 2007" is confusing.

RESPONSE: The year of the campaign was added on p. 21191, l. 3, and p. 21193, l. 25.

COMMENT: Page 21191, line 24: please introduce a blank between "(HO_2)" and "play" Page 21204, line 5: please correct "provides"; line 11: please remove the redundancy: "to maximum values to very high values" Page21213, line 14: please correct " can have be caused"

RESPONSE: We thank the reviewer for finding the typos and corrected them.

COMMENT: Conclusions: According to the content, seems to be more adequate for this section the title "Summary and conclusions" than "conclusions".

RESPONSE: We changed the title according to the referee's suggestion.

COMMENT: Line 4:"This was the first formal, blind intercomparison of HO_2 measurements". Is the content of this sentence (so phrased) very relevant for the conclusions?

RESPONSE: We rephrased the sentence to "The HOxCOMP campaign included a formal, blind intercomparison of HO_2 measurements."

COMMENT: Line 11: "within the range of"

RESPONSE: We corrected the typo.

Full Screen / Esc

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Interactive Discussion

Discussion Paper

COMMENT: Line 25 "The results of the SAPHIR experiments are supported by the good correlation between ambient data". I do not understand this sentence, please clarify and rephrase: how can the correlations of the ambient data at the beginning of HoxComp support the chamber experiments, if the conditions are completely different and the slopes are also different?

RESPONSE: Results regarding the connection between differences between instruments and ozone and water vapor were drawn from SAPHIR measurements. Since concentrations of both species were not very variable during ambient air measurements, a good linear correlation is expected and indeed found. This means that most likely the major effects can be identified from SAPHIR experiments. Therefore, results of ambient air measurements support results from SAPHIR experiments, even because conditions were different. Since the analysis of data was done after the campaign there is no temporal relationship required. Slopes in the regression analysis can still be different due to e.g. different calibration errors for ambient air and SAPHIR experiments. To address the reviewers concern we rephrased the sentence to "Both trace gas species which were identified to correlate with differences between measurements during SAPHIR experiments, were less variable for ambient air sampling. The good linear correlation between ambient air measurements does not give hints for additional species that influenced the instrument sensitivities."

COMMENT: Please correct reference Bohn et al., 2005, in the list of references

RESPONSE: The typo is corrected.

COMMENT: Figure 1: What is the meaning of DWD? Please remove as it is not mentioned in any part of the text and it does not seem to be of any relevance for this manuscript. Also remove or explain the empty boxes located close to position 1 and 2 in this figure. In addition, the distances between 5 instruments indicated on page 21199 do not seem to correspond with the figure. Please improve the figure.

RESPONSE: The figure was changed to a photograph of the measurements, which may also illustrate better the potential of local sources which may have affected ambient air measurements.

COMMENT: Figures 8, 9 and 11: I strongly recommend keeping common axis in the plots with different regressions wherever applicable in order to enable an easy visual comparison of the results, which otherwise may become confusing, i.e., HO_2 *(MPI) in the y-axis versus FRCGC or* HO_2*-(FZJ)*

RESPONSE: Since we do not want to show redundant correlation plots for all possible combinations of instruments, we decided to plot correlations for only three combinations of instruments which is the minimum number of independent plots. In order to avoid the impression that one of the instruments serves as a reference each of the instruments appears one time on the x-axis and one time on the y-axis. It is unavoidable that measurements by one instrument appear on the x- and y-axis for two plots. Confusion of the reader is minimized by keeping the same combinations throughout the manuscript, if possible.

COMMENT: Figures 2 and 6 can be combined in only one figure showing in the upper panel the HO_2 *data which are used for analysis and presented in figure 6. Please include a vertical grid to guide the temporal variation (for instance every 6 or 12 hours).*

RESPONSE: Figures 2 and 6 could be indeed combined. However, we intended to keep the same format for ambient air measurements and SAPHIR experiments. Therefore, we like to stick with two separate figures. Vertical lines are now added.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 21189, 2010.