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Comment on acp-2022-207

Anonymous Referee #1

Referee comment on "Radical chemistry at a UK coastal receptor site – Part 1: observations of OH, HO_2, RO_2, and OH reactivity and comparison to MCM model predictions" by Robert Woodward-Massey et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-207-RC1, 2022

The authors report measurements of radicals at a coastal site in the UK. In general, campaigns with a full set of radical measurements are sparse, so that further exploration of radical concentrations in different chemical conditions are valuable. The paper has a companion paper investigating the chemical budgets of radicals. In this manuscript the authors focus on the description of measurements and model-measurement comparisons. A large part of the manuscript is very descriptive, also in the discussion part, which puts the results into the context of results from other campaigns reported literature. Little new results are shown in the sense of improving the understanding radical chemistry in the atmosphere. Therefore, this manuscript rather fits a measurement report instead of a research article. It should be considered to change the manuscript category.

The authors need to improve the manuscript by a concise writing. It is not clear, if separte papers for the measurements / model reszlts and chemical budget would have been required, if the authors had carefully planned to focus the writing of new findings and had cut on lengthy descriptions of figures that can be easily grasped by seeing the figures. In addition. there are some sections, in which it is not clear, if there is a deeper meaning of the analysis that is shown or if the analysis has only be done, because a similar analysis has been done in other papers and therefore, these sections could have been omitted. The manuscript as it is written now clearly suffers from having the interpretation of model results and of the chemical budgets separated in 2 papers due to the close connection between both. Specifical the PSS calculations for OH shown in this paper is essentially the same as doing a chemical budget presented in the other paper. Merging the 2 papers would clearly be the best to present the insights into radical chemistry and likely also possible, because parts of the papers are similar, since the same data set is analysed and results from each paper is described in the other paper, and descriptive and unnecessary parts can be omitted.

The presentation quality of figures also needs considerable improvements. Font sizes in most of the figures contain are too small to be readable and scaling of axis are not appropriate. Light colours of text as used in the current figures are not suitable for reading (e.g. yellow).

Additional specific comments:

L50: For this type of paper, just showing H-abstraction to form RO2 may oversimplifying the chemistry. Overall, some of the text-book like introduction may not be required.

L84: I assume that you mean "their photolysis can also be important radical sources"

L94: The conclusion in Novelli et al. is not that Criegee intermediates are the reason for interferences observed in the field, because reactant concentrations in their work were much higher than atmospheric concentration.

L144: Are you sure that the purity of NO was 99.95%? Typically the best purity that is available is only 99.5%.

Fig. 2 It may be a good idea to improve visibility by splitting the figure into 2 panels by time.

L330: The statement about HONO is rather short and does not really reflect the high variability that is observed. On some days, values during the day were even higher than during the night.

L384: Looking at the entire time series, the second peak that appears in the median diel profile looks more like an artefact of the median calculation than a real feature of the diel profile as it sounds in this statement.

L411: I would avoid giving information in the text that is repeating what can be seen in the legend of the figure

Fig 7: Here, it may make sense to have the same scale of the y-axis for sRO2 and cRO2.

Fig. 8: Labels of the pie chart are not easy to read. Names may need further explanation in the figure caption. Numbers of fraction could be useful as well as the total median RO2 concentration.

L430 ff: Is the relative abundance of specific RO2 radicals consistent with measured OH reactants? This should be discussed.

Section 3.2: RO2: It looks like an offset between measured and modelled simple RO2. Can you exclude that there is an unaccounted instrumental offset?

Section 3.3: This analysis does not give much insights as it is done here. More discussion and comparison with previous findings with interpretation of different and similar results would be needed.

Section 3.4: Again, there is little interpretation or discussion of the correlation and it is not clear what is learned from this analysis. What is the meaning of the different slopes? What is expected for what reason?

L465: The offset does not necessarily indicate that some RO2 sources (= type of RO2 radicals) do not form HO2 as it sounds in the statement. It can also be that the lifetimes of HO2 and RO2 were much different or RO2 loss channels did not lead to HO2 formation, but the RO2 from all sources may still generally form HO2. If there were mainly RO2 sources in the night but little HO2 present, why would you expect that there is a correlation between RO2 and HO2, when the reaction of RO2+NO as most important pathway to HO2 is not relevant in the night?

Section 3.6 / Figure 15: Would you expect that an exponential behaviour of BVOC emissions is visible for the range of temperature that is experienced in the campaign? Can you make an estimate, how much RO2 concentrations will change, if you assume additional VOCs in the model to account for the gap between measured and modelled OH reactivity?

Section 4.4: It would be good to have some numbers of reactive halogen species that would be required to explain observations.