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Atmospheric Chemistry and Physics Discussions

Interactive comment on "How does the OH reactivity affect the ozone production efficiency: case studies in Beijing and Heshan" *by* Yudong Yang et al.

Anonymous Referee #2

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The manuscript presents OH reactivity measurements from two urban sites in China and compares the OH reactivity data to calculated and modelled reactivity determined from the individually measured, co-observed OH sinks. Ozone production efficiency (OPE) is calculated from measured and modelled reactivity and the authors conclude that missing OH reactivity can increase ozone production efficiency at both sites. Understanding total OH reactivity by considering the dominant species contributing to OH reactivity and identifying missing OH reactivity and how this influences ozone production in urban environments is important and a suitable subject for ACP. The conclusion that more aged air-masses have a higher % of missing reactivity is an important finding also. Unfortunately there are several major problems with the manuscript currently

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which mean that the results and interpretation of the results are over-shadowed: The technical concerns (already thoroughly covered by Reviewer 1) relating to the quality of the OH reactivity data from the CRM instrument under high NOx conditions where large corrections have been applied need to be addressed before final publication. Furthermore, a more comprehensive comparison between the observed reactivity and calculated and modelled reactivity should be included and discussed to strengthen the overall conclusions drawn. I struggled to evaluate much of the discussion and conclusions, largely due to the poor English, but also because data discussed in the text did not appear in the referred figures or tables: the modelled reactivity is not included in Fig. 11 upper panel and the breakdown of modelled reactivity is duplicated in tables S5 and S6. The figure axes and figure and table captions are inadequate to understand the data presented and there are inconsistencies between the data presented in the figures and discussion provided in the text. I have made a number of recommendations below where further clarification is needed or where the discussion should be improved before final publication can be considered.

Line 30: '..by adding unmeasured oxygenated..' this suggests that the model was constrained to assumed concentrations of OVOCs, but I don't think this was the case so this sentence needs revising

Line 34: change '..such as aldehydes..' to '..such as unmeasured aldehydes..'

Model description: Line 230: How were the VOC data inputted into the model given the 1 hour time resolution of these measurements and 5 min time resolution of the model?

In section 4 the authors consider the contribution unmeasured primary and secondary VOCs may make to missing reactivity. To strengthen this discussion some commentary is needed on the sensitivity of modelled OH reactivity to some of the assumed model parameters: Line 251: Are there local sources, e.g. roads, which mean that unconstrained products are not in steady state? How different is modelled reactivity on day 1 vs day 3 spin up? Lines 251-253: How sensitive is modelled OH reactivity to the

treatment of dry deposition in both locations? How was the changing boundary layer height treated in the model? Could this influence the diurnal profile of the modelled OH reactivity?

Results: Lines 317 and 319: Are these the peak OH concentrations at both sites? Given the photochemical age of the air masses wouldn't a mean OH concentration be most appropriate for this calculation? The authors should discuss briefly the sensitivity of the photochemical age to [OH] used.

Line 348: Please provide details on the data used to generate the pie-charts – is this the campaign average picture? How does this change in Heshan during the pollution episodes? It would be informative to include a time-series of calculated OH reactivity, modelled OH reactivity and measured OH for the whole of the two campaign periods somewhere in the manuscript.

Line 349: '..more significant role..' give % contributions.

Line 356-357: What was the level of the NO correction applied to the measurement data during morning rush-hour?

Discussion: Line 363: what is meant by 'relative reactivity'?

Line 366: '..not very high..' apart from Paris, Heshan VOC reactivity is highest. This section needs to be revised to accurately reflect the data in Fig. 10.

Figure 10a: Why not change the x axis to calculated NMHC reactivity (s-1)? This would then help to demonstrate the cause for this trend, i.e. a) that the type of measured NMHCs in Beijing are indeed more reactive with respect to OH than at other sites or b) missing reactivity is more significant in Beijing and Heshan vs other sites. The discussion provided in 4.1 should be revised once this figure is changed.

Figure 10b: The y axis label is missing. Also why is Paris not included in this plot?

Line 400: It is unclear whether the NOAA 2005 dataset is from Beijing? Even if it

is, it doesn't seem reasonable to simply compare missing reactivity from 2013 with branched alkene data from 2005. Could a common species, measured both in 2005 and 2013, which is strongly correlated to the branched alkene data be used to scale the 2013 branched alkene data? Why are there only 6 pts in figure 11, lower panel?

Figure 11 upper panel: modelled reactivity needs to be added to this plot

Lines 424 - 426: Key to ozone control strategies, the authors should discuss the primary species from which the modelled species derive.

Table S5: Species names should be provided in full – what is 'DCB'?

Lines 444-445: the authors should also compare the calculated and modelled reactivity from 2006 and 2014 too, so the 50% higher measured reactivity in 2014 can be evaluated fully.

Lines 452, 453: 'PAMS 56 hydrocarbons' and 'T0-15 OVOCs' need defining

Lines 465-466: in section 4.3 the authors report that the OH reactivity modelled in Beijing agreed with measured reactivity in the daytime (lines 423-424), but on lines 465-466 report differences between measured and modelled reactivity in Beijing which changed OPE by 27%. These two statements are inconsistent with each other and as the modelled reactivity is missing from Figure 11 it is unclear which is correct.

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