A review report on “Oxidative capacity and radical chemistry in the polluted atmosphere of Hong Kong and Pearl River Delta region: analysis of a severe photochemical smog episode” by Xue et al. (2016)

The study investigates the atmospheric radical budgets in the Hong Kong and PRD region, using a box model constrained with a full suite of ancillary measurements, not including OH or HO2. So, the budget analysis presented here is based solely on simulated OH/HO2.

My major concern is that the study does not provide new scientific results, especially given that several previous studies addressed this issue in the last decade, as also mentioned in the study. In addition, the inconsistency between simulated OH/HO2 and those previously measured in the same region weaken the results of this study and its argument toward a science-based control strategy. Add to this, several consistencies in the discussion of the results. The sensitivity analysis needs to be redone and reevaluated (see below).

The authors should address the following issues before the Manuscript can be considered for publication.

Abstract:

Page 1, Line 29: The statement “Sensitivity studies show that controlling aromatics is the most efficient way to reduce the atmospheric oxidative capacity and mitigate photochemical pollution in Hong Kong.” does not seem correct. The atmospheric oxidation capacity is the total loss rate of all species and thus represents the atmospheric capacity to reduce/degrade the atmospheric pollutants. Thus, I think it is just irrelevant to try to reduce the atmospheric oxidation capacity.! Reducing the photochemical pollution caused by ozone and PAN, photochemical secondary products, require sensitivity analysis to determine the contribution of each VOC to ozone formation (based on their kinetic and mechanistic properties) and target these species, which often require the use of a region-specific reactivity scale.

Page 7, line 10: The statement “abundant VOCs would facilitate efficient radical recycling” needs revision, since the efficient recycling of peroxy radical (RO2/HO2+NO=OH) requires only reasonable amount of NO. Under VOC-sensitive conditions, higher VOC lead will lead to higher ROx productions.

Page 7, Line 22: I do not see the wind direction in Fig. S2?

Page 8, lines 8: would be also informative if the authors could compare these AOC values with other world regions from previous studies and show its significance.

Page 8, lines 19: Please show *j*(O^1D) on the same figure with OH/HO2 (figure 5).

Page 8, lines 29-33: So, now I see some comparisons but it is not consistent with the study's results. The simulated OH/HO2 are about 2 times lower than the measured OH/HO2 at PRD (Hofzumahaus et al., 2009).

Page 9, line 1: Figure 6(a): Since the contributions of the photolysis of H2O2, HNO3, OVOCs seem extremely low that is not even seen on the figure, why they are shown?

The contribution of HONO photolysis in this figure should be only the net HONO (subtract [HONO]pss from OH+NO=HONO since it is not a net OH source).

Page 9, lines 25-35: The authors should discuss if the mentioned heterogeneity in the primary sources is just a result of not measuring all sources (i.e, HONO was not measured in all these mentioned studies) rather than differences in regional source contributions.

Page 11, lines 22-25: Why measured HONO is not constrained. The simulated HONO by the model represents only the [HONO]pss (OH+NO=HONO, HONO+hv=OH+NO) and does not represent a net source of radicals. [HONO]pss is a direct gas phase reaction of OH+NO and is not as secondary oxidation product. Not including measured HONO will certainly underestimate the simulated OH and thus will affect the simulated secondary products (e.g., O3).

Page 11, line 32: The discussion in this paragraph is not clear, how decreasing NO2 would decrease OH? Decreasing the OH loss via OH+NO2 reaction (via decreasing NO2) should lead to increased OH. Decreasing NO (only NO) would decrease OH (via decreasing the reaction rate of RO2/HO2+NO=OH).

Page 11, line 33: The authors did not mention before if they included a mechanism for heterogeneous formation of HONO? How this was considered parallel to measured HONO?

How reducing NOx would decrease OH (line 32) and increase O3 (line 34)?

Page 11, line 34: What is a "NOx-titrated regime", its not defined anywhere in the text?

Minor comments:

Could the authors address the differences between the MCMv3.2 and the most recent version, and how this would affect their analysis?