

# ***Interactive comment on “Atmospheric oxidation capacity in Chinese megacities during photochemical polluted season: radical budget and secondary pollutants formation” by Zhaofeng Tan et al.***

## **Anonymous Referee #3**

Received and published: 21 December 2018

This paper presents results from a 0-D box model constrained by observations of radical sources and sinks in order to evaluate the oxidation capacity of several Chinese megacities, including Beijing, Shanghai, Guangzhou, and Chongqing. The models suggest that while there are similarities in the chemistry of each urban area, such as ozone production being VOC-limited in each, there are some distinct differences in predicted radical concentrations, rates of ozone production, and OH reactivity, which may help provide insights into specific control strategies for each area.

While the paper provides some interesting contrasts between the cities, it is unfortu-

[Printer-friendly version](#)

[Discussion paper](#)



nately somewhat difficult to read due to issues related to both the amount of information and how it is presented as well as style and grammar. In particular, section 3.3.2 describing the radical budget analysis reads more like a stream of thought rather than an organized discussion.

A major assumption in the paper is that the model can accurately reproduce concentrations of OH, HO<sub>2</sub>, and RO<sub>2</sub> radicals in order to predict the oxidation capacity of each region. Unfortunately, there is no discussion of whether this is a reasonable assumption. As mentioned in the introduction, previous measurements of radical concentrations in urban areas often exhibit significant discrepancies with model predictions, suggesting that chemical models are unable to accurately reproduce the oxidation capacity of these areas (see for example Whalley et al. (2018), Griffith et al. (2016), in addition to references cited in the Introduction). As summarized in the Lu et al. (2018) review cited in the paper, “. . .current tropospheric chemical mechanisms cannot explain the OH radical concentrations in China, which strongly underestimated the OH concentrations and the local ozone production for the low and high NO<sub>x</sub> range, respectively.” The authors should expand the discussion of these discrepancies and discuss in much more detail their potential impact on their model predictions and conclusions.

Additional comments:

- 1) The paper would benefit from a more detailed description of what was measured and how they were measured, perhaps with a table in the supplement. In particular, the specific VOCs that were measured should be described in more detail.
- 2) Instead of just showing the total AHC (or preferably AVOC as indicated elsewhere in the manuscript), it would be more informative to illustrate the diurnal mixing ratios of some important individual VOCs that demonstrate the similarities and differences in the areas as described in the manuscript.
- 3) In addition, it should be clarified which VOCs and/or OVOCs were measured and which were modeled as part of the radical budget. For example, were HCHO and other

[Printer-friendly version](#)[Discussion paper](#)

carbonyls measured or was their contribution to radical production based on modeled concentrations?

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-959>, 2018.

ACPD

---

Interactive  
comment

Printer-friendly version

Discussion paper

