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Comment on acp-2020-1295

Anonymous Referee #1

Referee comment on "Study of different Carbon Bond 6 (CB6) mechanisms by using a concentration sensitivity analysis" by Le Cao et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1295-RC1, 2021

This paper used a box model to investigate the O3, NOx, and HCHO simulated from different versions of the CB6 mechanisms. The authors found that CB6r1 tends to predict higher ozone and formaldehyde with the same conditions relative to CB6r2 and CB6r3. The dependence of model results on surface emissions was also studied.

Generally, the manuscript is well written. The key findings are also explained sufficiently and relatively clear. However, I still have a few questions that need to be addressed in a future revision. Thus, my recommendation is to accept the manuscript after a moderate revision. Here are my comments.

- Lines 217-219, model spin-up is an adjustment process that the model that moves from an initial state of unusual conditions to an equilibrium state. It is usually applied under conditions with surface emissions. The authors' claim here is thus inappropriate. I suggest removing these sentences.
- Line 298, I feel a little confused here. In a previous context (c.f. line 266), the authors already stated that (R158) is responsible for the discrepancy of the results between CB6r2 and CB6r3. But here they stated that (R158) is "possibly" the major reason, which is confusing. Please rephrase the sentence here.
- Line 303, (R26) is not an HNO_3 related reaction. Instead, it is an important NO_3 radical forming reaction, which plays an important role in the nighttime polluted atmosphere. Please rephrase it here.
- Line 339, the estimation of ozone by CB6r3 would also be largely different from CB6r3 and CB6r2, isn't it? Please clarify it here.
- Line 398, there is a redundant "of" in the sentence.
- Line 413, from the response of ozone to the change of emissions in their study, it seems that the scenarios the authors investigated are in VOC-excess conditions so that the increase of VOC tends to decrease ozone. I thus doubt that whether their conclusions are also valid under NOx-excess conditions or not. The authors should provide a discussion (at least a brief one) on the limitations of their conclusions obtained in this paper.
- Code and data availability. It is always better to upload the source code as well as the data of the results to some website so that they can be shared with the scientific community. Only stating "the data can be acquired upon request" here is not enough

from my point of view.

- The appendix table in the manuscript is misplaced. Please modify it.
- The manuscript is reasonably well written, but I would like to remind the authors that there are still a few sentences that can be improved significantly.