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## Comment on acp-2021-584

Anonymous Referee #1

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Referee comment on "Reducing the tropospheric ozone bias in global models: the potential of processes modulated by weather" by Tamara Emmerichs et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-584-RC1>, 2021

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This paper explores the impact of several changes to a global chemical transport model on tropospheric ozone, in particular the role of water vapor in chemical kinetics, dry deposition, and natural NO<sub>x</sub>, isoprene and HONO emissions. The authors have attempted to harmonize their study under the theme of 'weather-driven processes on tropospheric ozone', but their attempt fails. Not only are processes tied to meteorology in rather crude ways (which my main complaint is that the paper does not acknowledge), but the focus on annual and summer average global distributions is inconsistent with the theme of furthering understanding of the impact of weather-driven processes on ozone. The paper, including the background and motivation, lacks focus and direction, and often does not reflect current knowledge (including current understanding of processes examined). In particular, the papers referenced are often inappropriate, and the knowledge base cited is incomplete. In theory, this paper is much more suitable for Geoscientific Model Development, but needs substantial improvement, new analysis, and reframing, including clear articulation of what we are learning, before publication anywhere.

First, the authors alter the reaction kinetics related to HO<sub>2</sub>+NO and RO<sub>2</sub>+NO with respect to water vapor. Most of the impact is from HO<sub>2</sub>+NO. They discuss how this changes chemical production and loss of Ox as compared to other models. Then the authors discuss how this impacts tropospheric composition and dry deposition, but it is unclear what they find in their study that complements other work vs. what others find (that the authors are inferring happens in the model too). It seems like their findings are substantial in terms of tropospheric and surface ozone but honestly I have trouble following what they are doing and putting their findings in the context of uncertainty related to these changes to the reaction kinetics. The authors need to better describe motivate what they had before and why they made the change, and the associated uncertainty. The current motivation makes it seem like these changes are obvious/critical/completely certain, but then why has no one else made them?

Second, the authors describe the impacts of a new dry deposition scheme (previously described by a paper this year in GMD with the same first author). The analysis on dry deposition focuses on surface ozone changes during morning vs. night and simply compares the total loss to dry dep to other models. My first complaint is -- what are we learning from this analysis? As is, this analysis does not merit publication. The difference changes to surface ozone during morning vs. night with dry deposition are intriguing, but the authors barely touch on it. My second complaint is that the authors pitch their representation of dry deposition as state of the science when it is not. From what I can tell, the authors' initial dry deposition scheme was not good, and there are only rather basic improvements to the new one.

Third, the authors describe a new drought dependence of isoprene emissions. The authors only really discuss how this changes annual total isoprene emission totals as compared to previous estimates. The authors barely discuss how this changes summer ozone, although there seem to be interesting changes in Fig. 6. As is, this analysis does not merit a publication.

Fourth, the authors put a simple HONO soil emission parameterization into their model by scaling soil NO emissions by HONO/NO ratios from boreal field samples. As the authors note not many global models have soil HONO emission sources, so theoretically this is new. (The authors need to clarify if this is the first HONO soil emission parameterization in a global model, or one of the first; if the latter, please cite other work). However, I think the authors need to discuss their approach and the related uncertainties more. Just saying soil HONO and NO emissions are 'mechanistically similar' and applying boreal field sample values globally is not sufficient. (By 'boreal' – the authors mean northern mid latitude regions, right? Or do they mean northern hemisphere?). There also needs to be more substantive analysis of the impact of HONO. The authors discuss how HONO emissions impact the HONO/NO<sub>x</sub> ratio but do not describe what this ratio means, or why it should be one value over the other. (The reader is also completely relying on the author to summarize the model-observations comparison for HONO/NO<sub>x</sub> accurately – it is likely worth adding a figure comparing model vs. observed values.) The authors then discuss the impact on OH, NO<sub>2</sub> and O<sub>3</sub> extremely briefly, and with a lot of speculation. I would ask the authors to focus on what their plots are showing and what we are learning from their analysis, with a particular focus on addressing the goal of the paper (which currently seems to be to address the impact of weather driven processes on ozone, but in my view needs to change).

Fifth, the authors put the Price and Rind lightning NO<sub>x</sub> scheme into their model and show how it changes NO<sub>2</sub> (Figure 16c looks the same to me as Figure 16a though...so I'm not sure I see the improvement). They say it improves the model bias over tropical oceans and N Africa and the central US, but again I do not see this with their figures... They also show how it changes the tropospheric ozone bias compared to IASI (again, hard to see the improvement on Fig 12... I can see that there is a decrease in SOME parts of the tropical Pacific, but a worsening over others in 13c). Nonetheless, the authors' claim that "the usage of the commonly applied P+R scheme improves significantly the NO<sub>2</sub> bias between EMAC and OMI as well as the trop O<sub>3</sub> discrepancy to ISAI over the tropical Pacific Ocean" seems like an overstatement, and I'm not sure their current analysis on lightning NO<sub>x</sub> merits publication.

Overall, while the updates to the model do not always seem to be state of the science, I understand that this is a global model with limited representation of processes like convective updrafts and land surface functioning and soil microbes, & thus parameterizations need to be simple. I want to be clear that I'm not asking the authors to represent everything in a state of the science way, rather better articulate what the state of the science is, and justify their decisions in going the simple route. And of course the authors also need to convey what we are learning from their changes... not just document them.

I also understand that the authors are limited because the surface ozone data for which they use to evaluate their model is mostly for northern mid-latitudes, but many of the changes in ozone occur in the southern hemisphere and tropics, or high northern latitudes. This is an issue for all global modelers, and the authors need to find a better way of addressing this issue. For example, what are we learning from places where we do have observations that can lend confidence to elsewhere? What are other observational constraints that can be used to evaluate poorly sampled areas? There must be something better than one global map of the IASI tropospheric ozone columns and one global map of the OMI tropospheric NO<sub>2</sub> columns.

Minor comments (I have many minor comments, only some of which I have taken the time to record here).

Line 3 – I don't know what 'the water complex' refers to

Line 5 – one of the most important reactions

Line 9 – I don't think it's true that most trop ozone loss to dry deposition occurs to vegetation, a lot of loss happens to snow and ice and water as recent papers such as Hardacre et al. 2015, Pound et al., 2020 (<https://doi.org/10.5194/acp-20-4227-2020>), Clifton et al., 2020 (<https://doi.org/10.1029/2020JD032398>) have suggested, despite relatively small deposition velocities over these surfaces

Line 27 – radiative forcing should only be discussed when referring to a change time period

Line 220-230 – will the authors illustrate the changes they are making as described here with equations? I find it very hard to figure out what they are doing vs. just discussing

Line 295-7 – are the authors suggesting here that O<sub>3</sub> loss through reaction between O<sub>3</sub>+NO is underestimated here, and may cause the remainder of the high bias in O<sub>3</sub>?

Line 389 – can the authors give the uncertainty in a relative sense?

Line 390 – what is the IC/CG ratio?

Line 399 – who describes the Price and Rind scheme as robust?

Line 399 – it's not just a question of flash frequencies ... but also the amount of NO<sub>x</sub> per flash and how to distribute the NO<sub>x</sub> vertically

Another minor issue is in their methods the authors often do not give the reader sufficient information. For example, can the authors elaborate on what 'weakly nudged' means? Also, discussing the model routines by their names in the model is not very helpful for the reader. Another example is how am I supposed to interpret Table A1 (what does number

refer to, what is IGBP category and how is it used, what are the soil samples from? The title is unhelpful).