Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-824-RC3, 2020

Atmospheric Chemistry and Physics Discussions

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Interactive comment on "Impacts of future land use and land cover change on mid-21$^{\mathrm{st}}$-century surface ozone air quality: Distinguishing between the biogeophysical and biogeochemical effects" *by* Lang Wang et al.

Anonymous Referee #4

Received and published: 20 February 2020

This paper looks at the biogeochemical and biogeophysical impacts of land-cover change on surface ozone. Surface ozone perturbations are much larger when including the biogeophysical changes, than when looking at the biogeochemical aspects alone. This is an interesting result and could be published.

However, the paper suffers due to methodological and conceptual errors. Considering the internal variability of the atmosphere, the authors need to do considerably more work to show that the biogeophysical ozone signal is due to changes in the land cover instead of internal variability. This paper might be publishable after extensive revisions.

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Discussion paper

1. I believe the authors may have looked at the statistical significance of their difference maps (the figures appear to show some cross-hatching), but they are difficult to read and are not mentioned in the text. Statistically significant areas need to be highlighted and the significance level discussed in the text. In many cases non-significant signals are discussed. They should not be. For example, there is extensive discussion of the ozone signal in Europe, but judging from Figure 4 these changes are not significant.

2. The response of the atmosphere to the land-surface is complex. It is not as simple as simply applying the thermal wind balance to surface temperature perturbations. If the authors do wish to include the cause and effect of the atmospheric perturbations to land-cover change they would do well to enlist the help of an atmospheric dynamicist. As it stands he paper will only be strengthened by omitting the rather simplistic meteorological explanations of the impact of land-cover change on the atmosphere.

There have been many simulations of the atmosphere to perturbations of surface temperatures (noting the response is much different in the tropics than the mid-latitudes). The authors could support their hypothesis by citing relevant papers. On the other-hand, many studies do show a response in the general circulation to changes in land-cover (e.g., see Lague et al. [2019, preprint DOI, 10.31223/osf.io/dbyqu] and references therein.) I am struck by the very similar northwards displacement of the jet-stream in the RCP4.5 and RCP8.5 simulations, despite different landcover changes and changes in the surface temperature response. This may argue for similar changes in the overall circulation. These changes appear to be on the hemispheric scale. It is unclear if any of the local changes are really significant.

3. In their interpretation of the response to landcover change the authors should be mindful of the large internal variability of the atmosphere. Even where the differences are found to be statistically significant the interpretation of these differences to changes in land-cover (instead of internal variability) may be problematical. The differences in the simulations could be simply due to decadal variability in the atmosphere. As shown in Deser et al. (2012) [Clim Dyn (2012) 38:527–546DOI 10.1007/s00382-010-0977-x],

for example, in most places it takes more than 30 ensembles of 10 year average differences in transient simulations (e.g., 2028–2037minus 2005–2014) to see significant differences in precipitation. While the present simulations might have less variability due to the fixed sea-surface temperatures it is unclear to me how much this reduces the variability. The presence of internal variability may obfuscate any signal from the change in land surface. For example, if I take the stipples in Figure 6 as gridpoints with significant differences (?) there are many regions of stipples throughout the world (even in parts of the S.H.) which seem significant. If one takes a significance level of 95%, this suggests 5% of the points may only appear to be statistical different.

However, the timeslice experiments in essence add another ensemble member. Similarities between the timeslice analysis and the transient analysis may point to robust differences. The authors need to do more work to attribute the changes to changes in land-cover. Their meteorological attribution, as described above, is probably not correct.

Minor Comments:

1. I felt the paper could be better referenced. Please back up with more references, e.g. L106-108 "Dry deposition. . .." L113-114 "The dry deposition. . .. L236-237

and other locations. . ..

2. It is probably important to emphasize somewhere that the impact of the surface on the atmosphere is complex. For example, taking Lague et al. (2019, preprint DOI, 10.31223/osf.io/dbyqu) as an example, the impact can be through changes in albedo, evaporative resistance, and surface roughness. How these play out together and interact with each other is not simple. These changes may impact the clouds, boundary layer turbulence etc. A short paragraph explaining these influences might be in order in the introduction. I don't think the paper mentions surface roughness anywhere. In addition, the explicit impact of the surface on the boundary layer should be discussed as this will impact the dry deposition of ozone and the mixing and venting of ozone in

the boundary layer.

3. Figure 1. While the land surface may influence the upper troposphere, the exact connection is not really clear. In addition, the tropical response (where there is no real jet-stream) is likely much different than the mid-latitude response.

4. Data and Methods Section. Prior to going into the model specifics, it might be useful to give a broad overview of the simulations. For example, it was confusing when the paper first discussed online and offline simulations and the setup of both. Note also the online and offline simulations likely will have very different boundary layers with different clouds and radiation so a comparison of these two model setups is not straightforward (e.g., Brownsteiner et al., 2015).

5. Could the authors clarify the difference in dry-deposition in the off-line land cover change simulations? Are the differences shown only due to differences in stomatal conductance. Is the dry deposition also sensitive to LAI or the type of vegetation even without considering stomatal conductance? I assume the parameterized boundary layer turbulence is the same in each case, correct?

6. L641 “vice versa”, please spell out.

7. L357, Do the isoprene emissions depend on the makeup of the forest expansion?

8. It is unclear why NOX is shown in Figure 3. I don’t think it is discussed.

9. In discussing the biogeophysical response the authors did not discuss changes in the ozone deposition velocity. This seems to be a field that would be easy to show and could be more directly attributable to land cover change.

10. Figure 4 and other figures showing the difference between online model simulations. I believe these figures show the difference between the final 10 years of the transient simulation and 10 years from the on-line CTL. Please state explicitly (maybe in the figure caption?).

11. In discussing figure 4, the authors mention the correlations between difference fields. In each case please give the correlation coefficient between the fields and its significance. I would suspect that in many cases these correlations are not actually significant, in which case the authors need to refine their language in discussing the relation between the fields.

12. L457 “meteorological changes”, these would include not only stomatal response which is part of the story, but the impacts of surface roughness and surface heat exchange on boundary layer turbulence.

13. L502 -L520 (also L542-L550). Please delete. This is rather speculative. The response to land forcing is likely to be complex. The argument concerning the thermal wind relation and the jet-stream is pretty “hand-wavey” and I doubt it is correct. Has anyone else seen this? The changes are most likely dynamically consistent with each other (as described in the paragraph), but this is much different than arguing that they are due to changes in the land surface and in particular through the mechanism described.

14. L522 What is the correlation coefficient?

15. Fig. 5 and Fig. 7 and Fig 8. The discussion would be clearer if you put in a panel showing surface ozone (I don’t think you need to show the topography in Fig. 5; in Fig. 8 most of the changes don’t appear significant).

16. L555-556, “suggesting”... This is doubtful. India is in the subtropics. The atmospheric response to land cover change is likely to be rather different than in the midlatitudes.

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