

## ***Interactive comment on “Global climate forcing driven by altered BVOC fluxes from 1990–2010 land cover change in maritime Southeast Asia” by Kandice L. Harper and Nadine Unger***

### **Anonymous Referee #3**

Received and published: 25 June 2018

#### Overall comments:

This paper examines the impacts of land cover change in maritime Southeast Asia induced mostly by oil palm expansion and the associated changes in BVOC emissions on surface ozone concentrations and tropospheric ozone profiles, and the subsequent impacts on radiative forcing. This is a novel piece of work that highlights the importance of considering atmospheric chemistry-mediated climate forcing in climate and land use change studies. The data integration and modeling approach are all scientifically sound, rigorous and valid. There are, however, insufficient or unclear exposition and explanation of the results at various places of the paper, as well as inadequate

C1

discussion of the results in relation to previous works. I recommend the publication of this paper, if the concerns raised below are addressed.

#### Specific comments:

P1 L21: The introduction section appears too short, and do not set up a context nuanced enough to motivate the work (the findings of which are exciting). I recommend the authors to expand the introduction (by 30-50%) by discussing at greater lengths the various references cited. More suggestions in relation to this are given below.

P1 L25: The “%” sign usually immediately follows the number without space.

P1 L27: Can there be a sentence or two describing why we are concerned with oil palm plantation from an environmental or ecological perspective (not just a climate perspective as included in the current second paragraph)?

P2 L8-9: Please expand this paragraph by discussing briefly the key findings of these few papers (Ashworth et al., 2012; Silva et al., 2016; Warwick et al., 2013). How large or in what ranges are the concentration changes? Is the surface air quality changes significant relatively to, e.g., the impacts of anthropogenic emissions or warming?

P2 L14: Why does upper tropospheric ozone have a larger climate impact than surface ozone?

P2 L19: How does the land cover change derived from this data source differ from or compare with that used by Silva et al. (2016)?

P3 L4: Please explain and justify whether the discontinuity created by using two different biomass burning datasets is acceptable, especially considering that biomass burning emissions are an important source of ozone there.

P3 L4-6: “Interactive” is a modeler’s jargon, and even for modelers can mean different things for different purposes. I recommend avoiding it and state more clearly that these emission schemes are “semi-empirical”, “mechanistic” or “dynamic functions of x, y, z,

C2

...”, especially for those that are not described more below.

P3 L13: Avoid the use of “online”.

P3 L28: Avoid “online” and “model’s”.

P3 L33: Please explain and justify the single chemical representation of monoterpene. Can all monoterpenes really be modeled as  $\alpha$ -pinene?

P5 L30: What about the LAI values for the new PFTs used for this study for MSEA? They are not described above. Are dynamic but grid-level LAI observed from, e.g., MODIS, used, or are PFT-level LAI values used for these new PFTs? If so, where are these values from? As LAI is so important for atmospheric chemistry, these need to be better stated and explained.

P10 L1: In the methodology section above, the authors have only discussed about model validity and model-observation comparison for the vegetation aspects (e.g., GPP, biogenic emissions). What about an evaluation of the ozone simulations by the model? How does the model’s simulated ozone globally compare with observations and with other models? Is the general high biases of simulated ozone in many climate-chemistry models also seen in this model? Since ozone concentration is crucial to this paper, I strongly recommend having a paragraph somewhere (preferably in the methodology section) discussing these.

P10 L14: Wong et al. (2018) also examined and quantified the factors behind the sensitivity of surface ozone to vegetation changes including isoprene emission and dry deposition. They also found a large impact of background NO<sub>x</sub>. See reference list below.

P11 L1: Dry deposition definitely also plays a role, and have you quantified the relative importance of isoprene emission vs. dry deposition to surface ozone in your model simulations? This appears to be a major missing part of this analysis and should be better addressed or discussed, even if the authors have already found that dry deposi-

C3

tion plays only a minor role. For instance, Wong et al. (2018) found it necessary and developed a method to formally disentangle the contributions from isoprene emission and dry deposition when leaf density changes.

P12 L2: The physical reasons for the enhancements (as opposed to reductions) of ozone over the ocean have to be explained. Can these enhancements be explained by, e.g., the mechanisms suggested by Hollaway et al. (2017)? A discussion in relation to this paper is recommended. See reference list below.

P12 L16: In Fig. 2a) and 2c), why is there a second peak for isoprene and HCHO enhancement near the tropopause?

P14 L5: Now I see that the oceanic enhancements are explained. But this explanation, with reference to Hollaway et al. (2017), should be mentioned early (see comment to P12 L2).

P17 L8-9: “This sensitivity study demonstrates that the climate forcing associated with regional land cover change is rapidly increasing.” I feel that this is too strong a statement. All the results are showing is that 2005-2010 as a 5-year period is responsible for a noticeably large fraction of the total RF compared to other possible 5-year periods, but without breaking down the other years into incremental 5-year periods (e.g., 1990-1995, 1996-2000, 2001-2005), we can’t really say there is a rapidly rising trend in RF.

P18 L5-7: “increase in regional surface ozone concentrations is unlikely to have a significant impact on the induced ozone forcing since, as Lacis et al. (1990) find, changes in surface ozone have a much smaller effect on climate forcing relative to equivalent ozone changes in the upper troposphere.” This is contingent upon the assumption that the formation and long-range transport of isoprene nitrate will respond in the same way even as the surface environment becomes more high-NO<sub>x</sub>. This needs to be justified.

P18 L24-31: I think one major missing discussion is to compare the ozone-mediated

C4

RF with the biogeophysical RF (e.g., changing albedo, latent heat, sensible heat, etc.) and biogeochemical (CO<sub>2</sub> exchange) associated with oil palm expansion. Indeed, most climatologists are still just concerned with the biogeophysical or biogeochemical RF, and having a comparison between those and the ozone-mediated forcing would give much insight into the importance of considering atmospheric chemistry in climate/land use change studies.

P19 L18-19: "Inclusion of a temporally variable BVOC BER in the global model would allow for an improved estimation of radiative forcing induced by land cover changes in this region." I think the current debate is exactly that we are not sure about the circadian control or not, and thus this statement is not necessarily true.

P19 L33-34: "(2) its apparent inconsequence to the surface pollution impacts of regional land cover change" Is there really no OH titration problem in MSEA in ModelE2-YIBs? Is that because the BER is low to begin with, compared to, say, the Amazon?

References:

Hollaway, M. J., S. R. Arnold, W. J. Collins, G. Folberth, and A. Rap (2017), Sensitivity of mid-nineteenth century tropospheric ozone to atmospheric chemistry-vegetation interactions, *J. Geophys. Res. Atmos.*, 122, 2452–2473, doi:10.1002/2016JD025462.

Wong, A. Y. H., A. P. K. Tai, and Y.-Y. Ip (2018), Attribution and statistical parameterization of the sensitivity of surface ozone to changes in leaf area index based on a chemical transport model. *J. Geophys. Res. Atmos.*, 123, 1883–1898, doi:10.1002/2017JD027311.

---

Interactive comment on *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2018-365>, 2018.