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Atmospheric Chemistry and Physics
Discussions

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 #2

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Harper and Unger present a study of the radiative forcing brought about via differences in isoprene emission under different land use configurations in the maritime Southeast Asia (MSEA) region. These land use changes comprise the move towards more oil palm plantations, which emit more biogenic volatile organic compounds than the native natural forests. The changes in isoprene emitted to the atmosphere as a result of the increased oil palm leads to changes in ozone. Of particular interest is that the Enhanced BVOCs caused bigger changes globally to ozone in the upper troposphere (0.6 ppb) than lower troposphere (>0.1 ppb), which would seem an important result. The novelty of this study is that the authors then go on to calculate the radiative forcing expected by these ozone changes, finding a small increase of +1 mW m-2 Mha-1.

This shows that impacts of land use changes in tropical regions, which are subject to stronger convective patterns that elsewhere, are very important.

My feeling is that this is a really nice idea, but the wrong tool has been used to carry out the study. The small changes in ozone seen at the top of the troposphere are probably lost in the noise of uncertainty of the chemical scheme chosen, and thus I question the impacts on radiative forcing.

The authors use the carbon bond 4 chemical mechanism to represent the oxidation of isoprene in the atmosphere. This scheme is very old and does not include some of the recent discoveries brought about via questioning the discrepancies between isoprene predicted by models, and observed mixing ratios. These particularly relate to additional OH recycling, which directly impact the influence of isoprene on O3 (eg Lelieveld et al., 2008; Peeters et al., 2009).

The authors do mention the uncertainty in the isoprene chemistry regarding increased oxidant cycling, right at the end of the paper in the conclusions, but I think there are other problems with this choice of chemistry scheme. High isoprene atmospheres, such as that found in this MSEA region, have caused more differences in chemical mechanisms than most others. Unfortunately, the carbon bond scheme has never fared well when tested alongside other chemistry schemes under similar isoprene rich atmospheres. I wonder why there has been no model development in the chemistry scheme in this work when the science behind this paper depends so highly upon it?

For example Knote et al (2015) tested two variants of the newer carbon bond 5 (CB05) scheme (neither of which contained updates to the isoprene chemistry) and found they “tended to be biased low in O3 under low NOx/high VOC conditions (e.g. biogenic emissions rich) as well as under very high NOx conditions. In general, the CB05 schemes produced ‘lower than average 8 hourly O3’ produced by other schemes. Mechanisms were ‘found to differ more strongly in their predictions of O3 levels and other pollutants in regions with strong biogenic VOC emissions”.

Archibald et al (2010) tested 8 chemical schemes in isoprene rich regions and found that the CB05 mechanism was 'unable to generate/recycle HOx at the rates needed to match recently reported observations at locations characterized by low levels of NOx.'

An older study - Emmerson and Evans (2009) tested the carbon bond 4 scheme against 6 other schemes. However the carbon bond 4 results disagreed with the majority of the other schemes, in even the sign of the changes in ozone (e.g. loss instead of production - see figure 3 panel e). Differences (and thus uncertainty) of 14 ppb were found between the resulting ozone from the Master Chemical Mechanism and the carbon bond 4 scheme, which is 14 times more than the ∼1 ppb of ozone changes found in Harper and Unger's study at the top of the troposphere, and upon which the radiative forcing calculations are based.

Thus I don't agree with the authors' comment that no updates to the chemistry have occurred because of "its apparent inconsequence to the surface pollution impacts of regional land cover change". I think if a different chemistry scheme had been implemented that the changes in ozone found by Harper and Unger as a result of including more oil palm plantations in the model would lead to more significant differences in the radiative forcing than found by their study.

I'd recommend updating the chemistry scheme. Perhaps even to include a sensitivity study with a more up to date representation of just the isoprene chemistry – particularly one that agrees with the sign of ozone changes driven by our current understanding. The chemical aspect of Harper and Unger's work is my only criticism, which if rectified I would then recommend publication in ACP.

General comments

A map figure would be good, showing the study area with the areal extent of regions growing oil palm in 1990 and where/how these regions have increased by 2010.

Page 2 line 2. "Compared to natural forests oil palm plantations are much stronger

emitters of BVOCs" Some numbers would be good here. How much stronger?

Page 2 line 20. Try placing the (Baker et al., 2005; Klinger et al., 2002) references at the end of sentence to avoid breaking the flow of the sentence up too much.

Page 2 line 29. How is photolysis treated in the model?

Page 3 line 27. 'the' calculation

Page 5 line 12. It is not clear where this LAI dataset has come from?

Page 5 line 14 (onwards in this paragraph). LAI has units of m2 m-2

Page 5 line 21. Table S2 – mention this is in the supplementary section.

Page 19 line 21. This whole discussion of uncertainties in the chemistry scheme would be better placed in section 2.1 which introduces the method used.

References

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