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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A study of the impact of land-use change in Borneo on atmospheric composition using a global model" *by* N. J. Warwick et al.

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We thank the reviewers for their constructive and helpful suggestions which have improved the quality of this work. We have provided our responses to Reviewer #2 below.

General Comments

1. Overall, the results are interesting, but not very new. Previous papers from some of the co-authors have addressed similar questions. This paper needs to demonstrate what new insight it brings.

Response: The work presented here is the only study so far to assess large-scale,

far-future regional/global changes in atmospheric composition from oil palm expansion using actual flux measurements of isoprene fluxes in the study region (rather than isoprene fluxes calculated by vegetation models) and simultaneous changes in NOx emissions from fertilisation, oil palm processing and transport. In the revised paper, we outline the differences from previous work more clearly and expand the discussion on the global/regional aspect of our study, including additional figures showing the modelled changes in composition over a more extensive area.

2. The discussion mentions that the model in a similar set up was unable reproduce the diurnal variation of ozone and NOX (Pike et al, 2010). Furthermore, a key aspect of the Conclusions is the impact of differing NOX emissions. Ozone is clearly very sensitive to the NOX, as demonstrated by the PALMX scenarios and yet there is no mention of the modelled NOX mixing ratios. I would therefore like to see a graph of NOX added to Figure 2. I would also like to see more in the discussion about the changes in NOX mixing ratios between the PALM and PALMX scenarios.

Response: We have added a description of the changes in NOx between PALM and PALMX to Section 5.2. We have also now discussed in detail the spatial variability of modelled NOx for the different scenarios and the difficulty of comparing a model against local measurements. For example, model surface gridpoints (land and ocean) surrounding Danum Valley encompass the NOx observations (capturing the diurnal cycle), but with values both significantly higher, and lower, than observed locally.

3. Whilst PALMX suggests large percentage increases in ozone with increased isoprene and NOX emissions, the modelled ozone mixing ratios are still relative low compared to polluted regions of the Northern Hemisphere. Hewitt et al (2009) found that there is the potential for very high ozone mixing ratios in Borneo if biogenic emissions were to be typical of oil palm plantations and if NOX mixing ratios were similar to those in rural N. America or Europe. How do the NOX mixing ratios in the present study compare to those in the Hewitt et al study and can this explain the relative low ozone mixing ratios predicted in PALMX? Alternatively is the difference due to the inclusion of

increased monoterpene emissions in Hewitt et al? Perhaps it is due to model dimension/resolution (box v 3-D)?

Response: Our model results produce similar changes in O3 mixing ratio (for the relevant change in NOx mixing ratio) as the Hewitt et al. (2009) study presents in Figure 4. The difference is that our representations of NOx emissions from oil palm processing and fertiliser application do not increase model daytime mean NOx mixing ratios in the PALMX scenario to the high levels discussed in the Hewitt paper. For example, daytime mean NOx in PALMX is less than 0.5 ppb over the majority of Borneo, with gridpoint peaks of up to $\sim$0.8 ppb. In Hewitt et al. (2009), daytime mean NOx concentrations of greater than $\sim$1 ppb in the 'plantation landscape' are required to raise ozone concentrations above the WHO 8 hour mean air quality threshold, with Figure 4 showing ozone concentrations calculated from NOx levels of up to 10 ppb. Our results therefore indicate that NOx emissions related to the oil palm industry alone will not be substantial enough to increase O3 mixing ratios to levels above WHO air quality thresholds under this land use change scenario. This would require additional NOx emissions from other developing industries on Borneo, or additional transport of ozone and NOx (via PAN) from SE Asia. We have expanded on this point in the revised manuscript.

4. I am intrigued as to why the FIX scenario reproduces the OH concentrations well without the HOX recycling, and yet the modelled OH reactivities are considerably less than suggested by measurements. This is explained as being due to the model overestimating OH lifetimes, possible due to model resolution. Could it be due to underrepresentation of VOCs in the model? What are the OH modelled reactivitives for the FOREST run?

Response: The model overestimation of OH lifetime could be due to underrepresentation of VOCs. Using numerical simulations of isoprene oxidation including the Master Chemical Mechanism, Edwards et al., (2013) have shown that the emission of isoprene constitutes a significantly larger emission of reactivity than is offered by the primary reaction with isoprene alone, indicating a large contribution from sec-

ondary oxidation products of isoprene. They therefore conclude that the development of techniques for the measurement of secondary multifunctional carbon compounds is needed to close the OH reactivity budget. The OH reactivities are now discussed further in the revised manuscript, along with modelled reactivity data for the FOREST run.

5. I think that defining the FOREST and FIX scenarios as "present day" is slightly misleading. It implies that the whole of Borneo is covered by forest when there are already extensive oil palm plantations. In the Introduction it states that 14% of Malaysia is already covered in oil palm, although it is not stated how much of Borneo is covered in oil palm. This needs to be explained and discussed.

Response: Approximately 46% of Malaysian oil palm area is situated in Borneo (data from 2008, MPOA). Including oil palm plantations in Indonesian Kalimantan, oil palm now covers $\sim$52,000km2 or $\sim$7% of Borneo. This remains a relatively small fraction of the potential oil palm plantation area. This is now explained and clarified in the revised manuscript.

6. I think the organisation of the text between sections could be improved a little. There are a couple of paragraphs in Section 4 (Emission Scenarios) (P. 7439, 2nd para. and p. 7441, 2nd para.) which describe model spin up and runs with different chemical mechanisms, rather than emissions. I suggest including a new section which describes the different runs, i.e. the spin up, and the combinations of the chemical schemes and emission scenarios, which have been described previously. It might be worth considering putting the description of the chemical schemes in their own section, rather than being embedded in the Model Description. The section describing the model runs could also include a sentence explaining that some additional runs were done in which the ozone deposition was doubled. Also note that the results of these deposition sensitivity runs are presented twice in almost identical paragraphs (Last para. of section 5.1 and last para. of section 5.2).

Response: Sections 3 and 4 have been now re-organised. The text describing the model, chemistry schemes and emissions has been combined into a single section with sub-headings: Model Description, Chemistry Schemes, Emission Scenarios and Model Simulations. As suggested, we have also mentioned the additional ozone deposition runs in the Model Simulations sub-section. The repetition concerning the deposition sensitivity results has been removed.

7. The first paragraph of the Conclusions is not really conclusions (more like the beginning of an abstract). There is nothing in the conclusions about the present day scenarios, either with respect to the different emission scenarios or OH recycling.

Response: The conclusion section has been re-written to take into account these points.

Specific Comments

P. 7434, l. 18. Just because different species of vegetation emit different quantities of isoprene, doesn't mean that a switch from forest to oil palm "will" alter isoprene emissions. It is just the logic of the sentence that is not right rather than the conclusion.

Response: We have rephrased this sentence to say that a switch from forest to oil palm will result in a change in isoprene emissions.

P. 7437, l. 5-8. Although the reference is given to Whalley et al (2011), I think it would be helpful to provide a little more explanation as to what is meant by "daytime maximum OH concentrations predicted by measurements of OH reactivity".

Response: We are referring to daytime maximum OH concentrations predicted by measurements of the OH reactivity and estimates of the OH production rate based on all known OH sources (Whalley et al., 2011). An explanation has been added to the text.

P. 7437. L. 26. The model uses the Mainz Isoprene Mechanism (MIM) Posch et al 2000. Is there any reason why the more recent MIM2 (Taraborrelli et al, 2009) is not used?

Response: We have carried out extensive box and global model evaluations of the MIM chemical scheme. We have not done the same for MIM2. We agree it is a good idea and this is a future aim.

P. 7438, l. 11. "Eqs." should really be "reactions".

Response: Changed.

P. 7438, l. 12. HACET is defined, but doesn't actually appear in Reactions 1 or 2.

Response: Reaction 2 has been corrected.

P. 7440, l. 3. OP3-1 needs to be defined.

Response: OP3-1 was the first OP3 campaign on Borneo and took place between 7th April and 4th May 2008. A definition has been added to the manuscript.

P. 7440, l. 17. I found the logic of the phrase "much of which is on Borneo (<60%)" confusing. "Much" suggests a large amount which is somewhat countered by "<".

Response: This phrase has now been removed. See also our reply to Reviewer's 1 comment on this sentence.

P. 7447, i. 11. It is not obvious what is meant by "where isoprene and NOX fluxes are varied independently".

Response: We have rephrased this sentence to 'simulations where isoprene only, and isoprene in combination with NOx fluxes are varied show that the magnitude of NOx fluxes are critical....'.

Figure 1. OP1-I and OP3-III need to be defined.

Response: Definitions added.

Figure 2 needs to be much larger.

Response: We have changed the layout of the 3 panels from vertical to horizontal to

allow the figure to appear larger.

References: Edwards, P. M. et al., OH reactivity in a South East Asian Tropical rainforest during the Oxidant and Particle Photochemical Processes (OP3) project, Atmos. Chem. Phys. Diss., 13, 5233-5278, 2013.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 7431, 2013.