Atmos. Chem. Phys. Discuss., 13, C4274–C4278, 2013
www.atmos-chem-phys-discuss.net/13/C4274/2013/

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.

N. J. Warwick et al.

Nicola.Warwick@atm.ch.cam.ac.uk

Received and published: 27 June 2013

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

1. The rationale for the modelling study could be more carefully drawn, so that the value added by the study is clearer, particularly with respect to the OP3 modelling studies of Hewitt et al. (2009), Pugh et al. (2010a; 2010b; 2011), Stone et al. (2011), Pike et al. (2010), and Pyle et al. (2011). Presumably this rationale will concentrate on

the particular strengths of high-resolution 3D modelling compared to box or trajectory modelling.

Response: We agree and have altered the text to explain the differences between this study and previous studies more carefully in the introduction and conclusions. See also our replies to Reviewers 1 and 2.

2. An important complementarity in the present study with Hewitt et al. (2009) is the chosen NOx emission scenarios. The present study chooses to study only NOx emissions from oil palm plantations, rather than more general increases in NOx emissions due to urbanisation and industrialisation, which underpins the modelling study in Hewitt et al. (2009). This should be made clearer in the current ms I feel. I would not agree that the present study assesses the maximum impact of oil palm on local air quality as may be implied by the text on p7447 (lines 7-8).

Response: We agree with this comment and have now more clearly defined the differences in NOx emissions considered in our study with those in Hewitt et al. (2009). In our study, we analyse an extreme, hypothetical scenario in which the whole of Borneo is converted to an oil palm plantation. In this scenario, the only NOx emissions present would be those associated with fertilisation and processing, manufacturing and transporting the palm oil. Any additional increases in NOx emissions on Borneo would be inconsistent with the whole of Borneo being hypothetically converted to oil palm (if we argued for more NOx emissions from other industries, we would need to decrease the isoprene emissions from oil palm). However, we do neglect any increase in the transport of O3 or NOx (via PAN) from SE Asia, and therefore accept that our estimate does not represent the absolute maximum impact on air quality in this region. A discussion following these lines has been added to the manuscript.

3. Later, when considering the implications of the results it would be useful to return to the strengths and weaknesses of the modelling approach used here: some discussion of more detailed chemistry (perhaps with reference to Stone et al. (2011); discussion

of sub-gridscale effects (perhaps with reference to Pugh et al. (2010b; 2011); and discussion of ecological - climatological feedbacks such as changes of albedo (Bonan, K.W. Oleson et al. 2002; Feddema, Oleson et al. 2005), hydroclimatology (Spracklen, Arnold et al. 2012), the surface energy balance (Fowler, Nemitz et al. 2011), atmospheric roughness (Betts, Cox et al. 1997) and deposition (beyond the description on p7443/4).

Response: We agree the above discussions would be useful and we have expanded our conclusions to consider these points.

4. In the description of model set-up and elsewhere (e.g.,P7443, Line 5), I think it is important to remind the reader that there were measurements of both concentrations and fluxes during OP3. Prescribing model isoprene concentrations to fit the observed concentrations disconnects the study from the bVOC emissions observations in OP3.Pugh et al. (2010a) showed, in a box model framework, that it was not possible to reconcile isoprene emissions with isoprene concentrations and with OH concentrations simultaneously with invoking sub-gridscale segregation. A minor related point is that it could be explained more clearly precisely how the isoprene tracer was held constant in the Borneo grid boxes and allowed to evolve according to the chemistry in other boxes (e.g., checks that were carried out to ensure there were no spurious chemical waves formed as a result of the fix).

Response: We have added a clearer description of the OP3 measurements to the introduction highlighting that there is both concentration and flux data for isoprene. The reader is also now reminded of this in the model set-up and model analysis sections. Isoprene concentrations were prescribed in model gridboxes defined as land over Borneo, and allowed to evolve with the chemistry elsewhere. As isoprene is so short-lived, and Borneo is an island surrounded by ocean (i.e. there are no isoprene-emitting chemistry-controlled gridboxes adjacent to the prescribed isoprene concentation gridboxes), isoprene concentrations in the chemistry-controlled region adjacent to Borneo are very low/negligible. Therefore multiple sharp chemical gradients leading to prob-

lems with model dynamics should not be formed in this scenario. We found no evidence for spurious chemical waves when analysing the model concentration data.

Minor Comments:

P7435, line 14. The best summary reference for land-atmosphere exchange during OP3, including ozone fluxes, is Fowler et al. (2011). I'm not aware of any fluxes of reactive nitrogen oxides having been published for OP3.

Response: This is a typo and should have read concentrations rather than fluxes. Changed.

P7437. I think that the boundary layer scheme used in the model study should be mentioned, perhaps pointing forward to the discussion on p7443 which states that Pike et al. (2010) showed the importance of boundary layer physics and ozone deposition in determining trace gas concentrations at Danum. One might add Pugh et al.(2010a; 2010b) to this latter discussion.

Response: These changes have now been included.

P7437, line 26 and throughout: Poschl should be Pöschl.

Response: Changed.

P7438, line 10ff: there are some minor discrepancies here between the text and the chemical reactions (no mention of HACET in the reactions; numbering R1, R2 referred to as Eqs. 1 and 2 in the text).

Response: Changed.

P7441, line 17. It would help the reader if the baseline NOx emission was also reported here, and both were compared to the emissions used in Hewitt et al.(2009).

Response: The baseline emission has now been reported and compared to the emissions in Hewitt et al. (2009). 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. This is discussed further in the revised manuscript.

P7445, line 1. Should the reference be to Hewitt et al.(2010) or (2009)?

Response: Changed to 2009.

P7445, lines 7ff (and the Conclusions). It does not make sense to compare a monthly mean to an 8-hour running mean threshold, unless some indication of the variability within the monthly mean can be given.

Response: We have now added an indication of the variability within the monthly mean.

P7446, lines 11ff. This paragraph is a repeat of an earlier paragraph and should be merged/deleted.

Response: Change made.

Conclusions. The sensitivity of ozone changes to regional contexts and to the land-use climate-change scenario chosen is also discussed in Sentian et al. (2011) and at length in Sentian (2009), which can be supplied to the authors by the reviewer or directly from Lancaster University.

Response: Many thanks for the suggestion. We have contacted this reviewer for the references and will consider these studies in the revised conclusions.

Acknowledgements. The number of the ms in the sequence of SEARRP papers is not given.

Response: This will be included in the revised version.

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