Atmos. Chem. Phys. Discuss., 13, C2239–C2242, 2013
www.atmos-chem-phys-discuss.net/13/C2239/2013/

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.

Anonymous Referee #2

Received and published: 11 May 2013

This paper examines the impact of changing isoprene and NOx emissions as a result of land-use changes on ozone and OH over Borneo. The study employs the global p-TOMCAT model run at fairly high resolution (1 degree by 1 degree). The modelled fluxes of isoprene are constrained by measured values during the OP3 field campaign at 2 sites, in a forested area and an oil palm plantation. The main conclusions of the paper concern the impact of increasing isoprene emissions, with or without concurrent increases in NOX. The sensitivities of the results to changing the OH cycling chemistry and ozone deposition rates are also investigated. 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.

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.

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)?

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?

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 al-

ready 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.

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).

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.

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.

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".

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?

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

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

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

P. 7440, l. 3. 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 "<".

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

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

Figure 2 needs to be much larger.

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