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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impacts of Current and Projected Oil Palm Plantation Expansion on Air Quality Over Southeast Asia" *by* S. J. Silva et al.

Anonymous Referee #2

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General comments

The authors present a study of land conversion to oil palm plantations in SE Asia, although this is limited to Indonesian Borneo and Sumatra. They use the GEOS-Chem atmospheric chemistry transport model to investigate how changes in land cover affect emissions of volatile organic compounds (specifically isoprene) and the impacts this has on atmospheric composition in the region. They find that increasing the area of oil palm plantations increases isoprene emissions and hence ozone and aerosol concentrations over most of the region.

While this is clearly a topic of interest, and one that is central to the scope of ACP, the research presented here is not sufficiently novel in my opinion to warrant publication at this time.

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Discussion paper

Specific comments

The main issues that I have with this manuscript are:

1. Novelty:

I do not feel that the work presented here is sufficiently novel in scope or methodology to mark an advance on previous work. Other than the use of different land use change maps the simulations do not differ from previous studies nor does the analysis of the model output extend beyond what has been considered before.

The authors introduce a new oil palm-specific plant functional type (PFT) into GEOS-Chem, adapting isoprene emission factor and LAI of the existing tropical broadleaf evergreen tree PFT using data from the OP3 field campaign. This is the exact approach taken by Ashworth et al., 2012 and Warwick et al., 2013, which built on earlier investigations of LUC (although not specifically due to oil palm) by e.g. Wiedinmyer et al., 2006; Lathiere et al., 2006; Ganzeveld et al., 2010.

The authors demonstrate that changes in oil palm distribution alter isoprene emissions (and hence concentrations) and thence concentrations of O3 and SOA. This is not a new finding (Ashworth et al., 2012; Warwick et al., 2013, and many other studies showing that different land cover affects atmospheric composition via changes in biogenic VOC emissions, e.g. Guenther et al., 2006; Arneth et al., 2011; and those listed above). The authors' results differ only in terms of distribution and scale.

Including changes in NOx emissions concomitant to the changes in land cover is not new, and in fact the study here does not go as far as Ashworth et al., 2012 who included sensitivity tests with and without NOx emissions associated with processing, nor Warwick et al., 2013 who included changes in soil NOx emissions associated with periodic fertilization of oil palm plantations.

Changes in O3 deposition have also been included in many previous studies of LUC (e.g. Ganzeveld et al., 2010; Ashworth et al., 2012).

Furthermore, since the publication of the OP3 data that the authors cite here, further data have been presented reporting high emissions of other VOC from oil palm (e.g. methyl chavicol (estragole) and toluene, Mizstal et al., 2010; 2011; 2015).

Higher than expected deposition of a number of other compounds has also been reported (e.g. Karl et al., 2009; Nguyen et al., 2014) and yet this does not appear to have been considered by the authors who refer to the reactivity of ozone as an additional reason to focus on its deposition. I would also be interested to know if GEOS-Chem partitions dry deposition between stomatal and non-stomatal routes in line with e.g. Fares et al., 2012; 2013; 2014; Simpson et al., 2012.

2. Methodology:

The spatial resolution of the model (0.5x0.66667deg) is too coarse for studying air quality (see e.q. Gego et al., 2005; Varghese et al., 2011; Schaap et al., 2015). In my view, this is a study of impacts on atmospheric composition rather than air quality and should be so described (i.e. in the title and text). Further, the authors only demonstrate how the projected changes in O3 concentrations relate to recognized WHO air quality standards although they discuss changes in formaldehyde (not a regulatory air pollutant), NOx and SOA as well. And yet, premature mortality and morbidity associated with particulate matter is almost an order of magnitude higher than for O3. I also find the choice of metric odd; the number of exceedance days is a threshold metric (i.e. a consideration of “extreme” conditions) which is likely to be poorly represented by a coarse resolution model.

The temporal resolution of the quoted changes in atmospheric composition is also not sufficient for air quality assessments. While annual limits are given for some pollutants (although mostly in terms of accumulated exposure), daily 8-hour and peak 1-hour exposure is the more normal metric considered. Presenting changes in annual average concentrations is therefore inappropriate in the context of air quality.

It appears that GEOS-Chem was driven with meteorology for a single year (2006).

The authors report that there was no substantial difference in projected changes in atmospheric composition between seasons. This is in contrast to the findings reported by Ashworth et al., 2012, and seems odd given that SE Asia is a monsoon-influenced region. That, plus the high level of fires reported for 2006, suggest that it may not have been a “typical” or representative year. Did the authors give any consideration to the inter-annual variability of their findings?

A future scenario set in 2020 seems rather limited in scope given that it is now 2016. It would have been interesting to assess how the LUC might combine with future changes in climate and air quality in the region with a longer-term scenario.

3. Other:

The analysis is limited with changes in atmospheric composition given almost entirely in terms of changes in annual averages. On the whole, presentation of results is limited to a series of virtually identical figures. As most of the changes are spatially similar there seem an unnecessary number of figures. They do highlight the issue of model resolution quite clearly. Pugh et al., 2013 identified SE Asia as a region in which model spatial resolution is particularly important for atmospheric chemistry modeling which also appears not to have been considered by the authors.

The choice of color scale for Figure 9 is poor. It is virtually impossible to make out the outline of the islands when this is printed out. Using white for a ratio of unity would seem a more sensible way to show the limited extent of the impact.

Isoprene emissions are not usually given in units of atoms C cm-2 s-1 in the context of a regional modeling.

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