We thank the referee for their consideration of our manuscript. Below are our responses to each of the comments, including the proposed changes to our revised manuscript.

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.

We appreciate the reviewer's in depth analysis and comments on our work, however we disagree with several key points made by the reviewer. We note these throughout the responses.

We would also like to clarify the reviewer's characterization that that the work is "limited to Indonesian Borneo and Sumatra". We include the oil palm distribution across all of Malaysia as well, thus capturing >90% of all global oil palm agriculture. For the future scenario sensitivity, we only change the distribution of oil palm over Sumatra and Kalimantan due to a lack of information on the future distribution of palm throughout Malaysia. We believe that this is an acceptable sensitivity test because, as stated in our manuscript, the majority of land across both Indonesia and Malaysia available for oil palm plantation expansion is on Sumatra and Kalimantan.

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.

We have updated the text to further emphasize the novelty of the work (described in the detailed points below).

We further note that the reviewer did not discuss the novel satellite analysis presented in Section 4, wherein we describe how and why current observing systems are not capable of detecting the air quality impacts of the massive land use change signature of oil palm. Our conclusions are critically important for implementation of observing systems of the future. If we cannot observe the impacts of the massive change in biogenic emissions associated with oil palm plantations based on current satellite measurement capabilities, we are likely to struggle to detect the impacts of any forest to agriculture land use conversions in the future.

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.

Our implementation of the oil palm-specific plant functional type differs from both Ashworth et al. (2012) and Warwick et al. (2013) in several key ways:

Ashworth et al. (2012) scaled the isoprene emissions of only broadleaf evergreen trees within a given grid box based on the fraction of oil palm expected within that grid box. The scaling factor used was a ratio of measured emission factors for oil palm and broadleaf evergreen trees: 50/35. For this work, we reduce the fraction of all vegetation in a grid box proportionally, not just broadleaf evergreen trees. This accounts for the fact that oil palm is not only farmed in regions where the natural rainforest has been removed. We additionally directly calculate the isoprene emissions through the MEGANv2.1 algorithm using the measured basal isoprene emission factors from OP3. By using the MEGAN algorithm in conjunction with the measured emission factors, we are able to more robustly estimate oil palm emissions outside of the timeframe of the OP3 field campaign (accounting for seasonal temperature differences, PAR, LAI, etc.) This was not explicitly accounted for in Ashworth et al. (2012) or Warwick et al. (2013).

Warwick et al. (2013) explored a future scenario where the entire island of Borneo was covered with oil palm vegetation, and replaced the MEGAN emissions algorithm with emissions measured during the OP3 field campaign. They acknowledge that this is "obviously an extreme situation", but that is useful for exploring a certain air quality trajectory. We use a more realistic land map for both modern and near-term future oil palm distributions, including the distribution of oil palm on Sumatra and the Malay Peninsula.

We clarify some of these differences in our manuscript on P2 Lines 31-34. And P5 L9-11

The authors demonstrate that changes in oil palm distribution alter isoprene emissions (and hence concentrations) and thence concentrations of O_3 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 bio- genic 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.

We agree that the finding that changes in isoprene emissions can change O_3 and SOA concentrations is not unique to this study. We argue that the differences that we model in terms of distribution, scale, and magnitude are important enough to consider this *work novel. In particular, our simulations explore the integrated effects of changing emissions AND deposition and are at higher resolution (0.5x0.67) than Ashworth et al. (2012) and Warwick et al. (2013). Previous studies also do not explore the impact of oil palm on densely populated regions, such as the Malay Peninsula, where we see the largest surface O3 increases.*

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.

We agree with the reviewer: the treatment of NOx emissions is not a novel aspect of our study. Given the uncertainties associated with fertilization (as discussed for the OP3 field campaign by Fowler et al. (2011)) and processing emissions, changes in NO_x *are not a focus of this study. More information on additional* NO_x *sources from fertilization or processing is needed to realistically explore this. However, we note that our satellite analysis in Section 4 confirms that we are not missing major palm-related sources of* NO_x *in our simulation, suggesting that these sources may indeed be modest.*

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

We agree that changes in O_3 *deposition have been included in studies of land use change before. However, the simultaneous exploration of both emissions and deposition changes related to oil palm had not been studied before. Warwick et al (2013) and Ashworth et al. (2012) both explored separate sensitivity studies to show the potential influence of deposition. Warwick et al (2013) doubled all deposition velocities, to test their model sensitivity. Ashworth et al. (2012) perform several short sensitivity studies of deposition, and conclude that the changes in deposition are likely to increase* O_3 *concentrations. This is at odds with our work, where we show the opposite through a complete simulation of the depositional changes.*

The Ashworth et al. (2012) result is likely due to the fact that they used a higher biomass density for the natural vegetation than for oil palm plantations, and only allowed oil palm to replace the tropical broadleaf evergreen trees. This reduction in biomass density led to a reduction in deposition. Our work uses a dry deposition scheme (Wesely and Hicks, 2000) that does not use biomass density as a parameter, but instead uses LAI. This is likely a more realistic parameter due to the heavy dependence on stomatal and cuticular deposition in heavily forested regions. The observationally-derived land cover inputs used in this work show that oil palm plantations have higher LAI than most of the natural rainforest and other land types across Southeast Asia. This leads to the increased deposition in our work.

We added text to P6 L4-8 and P6 L27-33

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).

The reviewer correctly points out that the oil palm plantations have higher emissions of estragole and toluene, however their chemical transformations (and thus impact on O_3 and SOA) are not well constrained, and Guenther et al. (2012) indicate that emissions of these species are relatively unimportant, particularly as compared to isoprene. As a result, we do not include these species in our analysis and do not believe that this omission significantly impacts our results. We add a sentence to P5 L18-20 discussing this.

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.

The changes in land cover do change the deposition of other compounds in GEOS-Chem. We focus on O_3 due to the significance of O_3 on regional air quality, and because its reactivity makes it a good candidate for describing the changes in deposition related to oil palm expansion. The magnitude of changes in deposition for all other species is nearly always less than 10%, and never more than 15%. The dry deposition module in GEOS-Chem does consider deposition through both stomatal and non-stomatal pathways, as outlined in Wesely and Hicks (2000), but only calculates one net surface sink.

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.

This model resolution has commonly been used to study air quality in Southeast Asia, including: Kim et al. (2015) and Marlier et al. (2012). In addition, both Varghese et al. (2011) and the Schaap et al. (2015) indicate that using approximately 0.5deg *resolution models (the resolution used in this study) for air quality is appropriate and useful.*

We believe that this is a study of both atmospheric composition and air quality, and since we make important conclusions with regard to air quality, we feel the title accurately represents the work.

We considered only the changes in O_3 for the air quality standards because the relative changes in O_3 were much larger than the changes in any other WHO standard species, including particulate matter. This is due to the substantial background PM concentration associated with fires in the region. Many of these fires are used to clear land for oil palm plantations. This impact has been studied further in Marlier et al. (2015).

We clarify this in our manuscript on P11 Lines 24-27

The metric of number of days in exceedance is one that is commonly used in the atmospheric chemistry community for models of similar (and coarser) resolution, e.g.: Fiore et al. (2002), Parrish et al. (2010), Leibensperger et al. (2008), Lin et al. (2001), Van Loon et al. (2007), and Marlier et al. (2012).

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.

We presented our results as general long-term averages, followed by a metric-relevant analysis of daily maximum 8-hour average surface O_3 for urban air quality. This is similar to the way data is presented in Kim et al. (2015) and Marlier et al. (2012).

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, suggests 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?

We took this comment into consideration, and completed simulations using 2007 and 2008 meteorology and emissions. These results indicate that the absolute magnitude of changes presented in this work are not highly sensitive to the choice of model year. The relative changes are more sensitive to the choice of model year, wherein the high amount of fires in 2006 lead to more modest relative changes. We have added a sentence on P4 L8-9 in the manuscript:

"Additional simulations using emissions and meteorology from 2007 and 2008 indicate that the choice of model year does not substantially influence the results of this work."

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.

In light of both Reviewers comments, we have changed the way we discuss the 2020 projections throughout the manuscript. To focus on the important notion that it is a near-term pessimistic future, and not a prediction of the exact distribution, we have added several sentences at the end of section 2.2:

"It is important to note that the 2020 distribution used here is the best estimation of a pessimistic future, and may not be an accurate prediction for the specific year 2020. It is meant to represent a realistic near-term scenario, and for this reason we refer to it from here on as the "future" distribution."

Throughout the paper, we now typically refer to "near-term future" rather than 2020. We agree that it would also be quite interesting to investigate how future air-quality and climate in the region interact with this land use change. However, we focus here only on near-term changes to the oil palm distribution, and the resulting influence on biosphere-atmosphere fluxes. Because of this, that specific climate analysis was considered out of the scope of this work.

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 figures are presented in a similar way to facilitate comparisons among them and with the satellite analysis in section 4. We also feel that the number of figures chosen allows for the best interpretation and reproducibility of these results in context with other studies.

We do in fact address issues related to resolution on P8L19 and P10 L21 in discussing the disagreement between our model and experimental data. However, we do not consider the model resolution to be an issue for the validity of our results, for the reasons and citations listed in the previous responses.

Pugh et al. (2013) demonstrate that using a 0.1˚x0.1˚ model resolution is far superior than using a 2˚x2˚ model over Southeast Asia. They further recommend that an *effective way to deal with high model uncertainty is to use "higher resolution land cover data, even when paired with coarser meteorological data". The model resolution we use is 0.5°x0.667°, significantly better than 2°x2°. Furthermore, we use a higher resolution land cover data (0.23°x0.31° resolution) as recommended by Pugh et al. (2013).*

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.

Thank you for this suggestion. The color scale has been changed.

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

We have changed the units to μmol C m^{-2} hr^{-1} for consistency with other work (Guenther et al. 2012).

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