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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global climate forcing driven by altered BVOC fluxes from 1990–2010 land cover change in maritime Southeast Asia" *by* Kandice L. Harper and Nadine Unger

Anonymous Referee #1

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Global climate forcing driven by altered BVOC fluxes from 1990-2010 land cover change in maritime Southeast Asia

The authors present the findings of a global modeling study probing the impacts of historical land cover change on the islands of maritime SE Asia with a particular focus on the expansion of oil palm plantations at the expense of natural forest. They apply a chemistry-climate model with interactive land surface to investigate the resulting changes in BVOC emissions and atmospheric composition in the region. In line with previous studies they conclude that changes in surface concentrations of the air pollutants / short-lived climate forcers, ozone and secondary organic aerosols (SOA),

are negligible. However, they demonstrate that due to strong convection in the tropics, upper tropospheric concentrations are more strongly affected and calculate the radiative forcing associated with these changes, showing that land cover change over this 20-year period in this region may have resulted in local changes in radiative balance.

On the whole this is a carefully implemented study with a reasonable selection of simulations designed to probe some of sensitivities of the model to their assumptions of land cover and vegetation characteristics. Their findings are generally well-presented. There is no doubt that the issue of tropical forest loss and / or degradation is of major global importance with both air quality and climate, and maritime SE Asia is a region which is experiencing rapid and extensive changes in land use.

However, I do have a number of reservations regarding their methodology, some of the assumptions made and the style in which they have presented some of their results. At present I feel that these are of sufficient concern to preclude publication.

Principal among these is the coarse resolution of the model used; a global model at 2x2.5 degree is not sufficiently fine resolution to adequately resolve the complex terrain or the heterogeneity of land cover, emission sources and chemical background conditions. NOx emissions have also rapidly increased in this region and the land cover changes included in this study will also introduce further changes. Given the sensitivity of ozone production and loss and SOA formation to the relative abundance of NOx and VOCs finely resolved spatial distributions are required to correctly diagnose both the direction and the magnitude of the changes in ozone concentration in particular.

My second major concern is the chemistry mechanism included in ModelE2-YIBs which the authors describe as based on CBM-4. CBM-4 was developed in the late 1980s and early 1990s at a time when the atmospheric chemistry community was principally focused on urban air quality and inorganic pollutants. The limited detail that the authors provide here suggests that the mechanism has not been updated to include the recent (i.e. post-2008) improvements in our understanding of isoprene oxidation

under conditions of high BVOC and relatively low NOx concentrations, conditions that must apply to large parts of the region under study. The same applies to monoterpene chemistry and the formation of biogenic SOA from both isoprene and monoterpene oxidation products. Without these updates it is hard to have confidence in the modeled changes in atmospheric composition arising from changes in BVOC emissions and concentrations.

Finally, I find that the manuscript is highly skewed to changes in isoprene and ozone, with monoterpene and SOA impacts rarely mentioned in the main text. However the final figures of radiative forcing include the forcing due to changes in SOA. A full discussion of monoterpenes and SOA is therefore needed in the main text.

More detailed comments are given below.

p1

L22 - Could the authors provide a map of the region to indicate what they are describing as “maritime SE Asia” and “the maritime continent”

L22 - It would be useful if the authors could provide some sense of scale. What proportion of Indonesia as whole is 4.5Mha? Or perhaps more relevant, what proportion of the natural forest does this represent?

L26 - It may have quadrupled but it started from a very low base; this is one of a number of times that the authors have tended toward dramatising the results.

p2

L7-9 - In fact, Ashworth et al. reported the change in the total tropospheric burden of ozone and SOA before focusing on surface changes and Warwick et al. present altitudinal plots of the changes in some trace gases.

L20-21 - This is the first mention of monoterpenes (aside from the abstract). I suggest for the authors also to discuss the atmospheric chemistry and composition effects of

monoterpenes as per isoprene in the previous paragraph. For instance the surface flux measurements reported from the OP3 field study (Langford, Misztal) showed that natural forests are much stronger emitters of monoterpenes than oil palm plantations. And the previous investigations also included changes in monoterpene emissions which is not clear in L7-9 as the preceding lines had focused exclusively on isoprene.

The changes in monoterpenes and SOA seem to very much be of lesser importance to the authors than changes in isoprene and ozone here and throughout the manuscript. While I accept that the changes are smaller they still contribute to the overall radiative forcing reported by the authors and should be given full coverage in the main text and not just the SI

L24 - Does this mean that the authors have only conducted a series of atmosphere-only model simulations? So there are no climate / land surface feedbacks included on-line?

L24-27 - Actually I am now confused as to exactly what model simulations were performed. The authors have referred to atmosphere-only, chemistry-climate, and land surface models here. Exactly what configuration is being used?

L27 - 2 deg x 2.5 deg is too coarse to adequately resolve the complexity and heterogeneity of the land mass and land cover in this region particularly given the sensitivity of BVOC oxidation and ozone production rate to VOC:NOx ratio.

L31 - I have reservations whether the chemistry mechanism employed here is suitable for the conditions encountered in this region. Although it is rapidly developing with the concomitant increases in anthropogenic emissions, much of island areas of the region are still low-NOx, high-VOC regimes. Older chemical mechanisms were designed for the typical chemistry encountered in mid- to high-NOx urban / industrial areas and considerable understanding has been gained of the very different oxidation pathways of (particularly) isoprene under lower NOx conditions. Have any of these updates been included in the chemistry here?

p3

L8 - Are the monoterpenes emitted as a single lumped monoterpene species? Or at least in part speciated (e.g to specifically include a-pinene, b-pinene, d-limonene and others as is often done)? It should be noted that the monoterpene emissions algorithms included in Lathiere et al are in fact the Guenther et al. algorithms from 1995; at the very least this paper should be referenced here. Also, these algorithms assume that monoterpene emissions are entirely temperature dependent whereas more recent field measurements have shown that many species emit a proportion of monoterpenes directly (i.e. monoterpene emissions exhibit both light and temperature dependency, see e.g. Steinbrecher et al, Guenther et al. 2012). Are the authors confident that this is not the case for SE Asian plant species?

L15-19 - How were emission factors assigned to these additional land cover types? How do they differ from the standard land cover in this region in the default land surface map? Again this is critical to the results and should be included in the main text and not just the SI.

L31-32 - Of real importance to this study is the previous performance of the YIBs model in this region; the 2013 study was global. Do the referenced comparisons include field sites in maritime SE Asia?

p4

L14-15 - It seems odd to go the lengths of using 30m x 30m resolution land cover data in a model running at 2 deg/ x 2.5 deg L20-26 - Please clarify. Are you saying that in 1990 the only land cover data available shows natural forest (or whatever) in locations that were shown as oil palm in 2000 and 2010? What fraction of data is missing? Of the 1990 data what fraction is converted to oil palm by 2000 and 2010? Of the missing data what fraction is “converted” to oil palm by 2000 and 2010? It would be useful to have a feel for how substantial the “likely underestimation” might be.

L30 - Is dirt equivalent to the “bare ground” classification included in other land surface schemes?

p5

L3 - The authors say that these are “minor land cover types”. Again it would be useful to be provided with sufficient information to judge just how minor. What fraction of land is included in these 5 types in Gunsaro et al classification?

L8-13 - Again it would be good to have an idea of the likely underestimation, and this should be relatively easy for the authors to achieve by applying an LAI reduction, as described, to the areas classified as “disturbed” in Gunsaro.

L21 - As noted above, Table S2 should be in the main text as these parameter values are critical to the results. The notes regarding their derivation can be left in SI. The values for the “standard” PFTs in YIBs for this region should also be shown in this table for comparison.

L27-34 - There is a real mishmash of years for the various datasets. As the simulations are being conducted for a nominal 2010 (i.e. that is the climatology) with 1990 or 2010 SE Asia landcover why introduce further limitations / discrepancies by using Y2000 landcover for the rest of the world with vegetation characteristics derived using Y2000 meteorology only to change to 2010?

p6

L33-34 - Could the authors please clarify how the simulations were driven with the meteorology? Was the same climate / meteorology applied for 13 years? Because there will be an effect of inter-annual variability on emissions, chemistry and therefore O3 and SOA formation; how has this been accounted for? Is this what the authors have attempted to do via the additional simulations?

p7

L4-6 - This is the case for all current isoprene emissions models which are linked to PAR, T, CO2, soil moisture, etc. Please clarify what aspect the authors mean is the case “because” it is interactively linked OR remove the word “because”

L6-7 - Please give more detail of how monoterpene emissions are sensitive to climate as per isoprene

L9-11 - But changing the landcover will also affect e.g. NOx emissions, either due to changes in fertiliser application or to changes in natural soil emissions. How have the authors accounted for this?

Because again the resolution of the global model will not be sufficient to pick up changes such as this simply by running a sensitivity test with a different background atmosphere.

L28-30 - Why have the authors not used the measured emission rate in the first instance?

L30-32 - Why? The authors specifically introduced this land class because measurements had shown that the global emission factors were not suitable. The work reported in Langford and Misztal suggested that emission factors were out by a factor of 3 so using 12 seems rather extreme.

p8

Table 2 - this seems to imply that the isoprene emission factor applied to oil palm is as measured in the standard run but half measured in the OPber sensitivity tests which appears to contradict what the authors have described in the previous page.

p9

L3-4 - It would also be good to see how well 1990land_base and 1990land_1990atm GPP compare with measured GPP

L5 - please define contemporary, because Table 5 in the Guenther paper contains

estimates from early 90s to around 2008. Again it would be useful to see the emissions estimates for both 1990 and 2010 land cover and climates here.

L10-19 - Here and throughout, although the authors describe this as a study of how BVOC emissions changes have affected the region the manuscript is entirely dominated by consideration of isoprene. While this is understandable given that total regional isoprene emissions are more than 5x those of monoterpenes I think the paper would benefit from more consideration of monoterpene emissions and impacts as monoterpenes and isoprene have different effects on atmospheric composition and chemistry. I suggest the authors also pay careful consideration to their use of the catch-all BVOC as this study appears only to include isoprene and monoterpenes.

L16-18 - Similarly to the previous comment, rubber plantations don’t make such a strong contribution to total BVOC emissions (e.g. compared to shrubs). However they do appear to dominate the monoterpene budget and might therefore have a strong role in SOA formation rate and yield. I suggest re-phrasing this sentence to make this distinction clearer.

L20-21 - but as previously noted by the authors, their assumptions in assigning land cover has likely led to an underestimation of deforestation. Please could the authors make some attempt to quantify the uncertainty in the changes in GPP. In addition to the method applied to fill data gaps and the non-inclusion of changes in LAI due to disturbance, the authors have used 2010 climatology in both cases which will affect the estimated GPP. I am assuming that the figure quoted here is based on difference between the two base simulations.

L25-26 - this could perhaps be better phrased as the loss of dipterocarp forest is due to its replacement by another land cover; as this is often oil palm so overall isoprene emissions go up.

L31 - I think the authors mean “Considering only the grid-cells that are majority . . .” as they then go on to give surface O3 concentrations in 2 sub-regions rather than ALL of

the study area.

p10

L3-5 - These measurements would seem to support the low ozone concentrations simulated by the model over Malaysian Borneo; however I am not sure they provide evidence of ozone concentrations over ALL forested areas in the region particularly as the authors are comparing annual mean concentrations with measurements made for one particular (short) period.

And related to this, are the authors intending to imply that they simulate much higher levels of ozone over the non-forested areas in the region? In which case, what are the average ozone concentrations for e.g. peninsular Malaysia which is far more industrialised? i.e. it is likely the case for many of the included grid cells that the proportion of the grid cell not occupied by forest in Borneo and New Guinea is ocean, whereas in peninsular Malaysia, etc many more will contain urban / industrial areas with higher NOx emissions.

L7-8 - This would be a suitable place for the authors to emphasise the difference between the effects of monoterpenes vs isoprene, with isoprene oxidation more implicated in ozone production and loss rather than SOA formation.

L8-9 - Please comment on the possible reasons for the observed enhancements over the ocean.

L7-20 - The authors present and discuss only annual average surface concentrations. This masks seasonal changes in magnitude, sign and distribution. For example, Ashworth et al reported different patterns of ozone and SOA changes depending on wind direction between the two monsoon wind periods. And given the NOx-sensitivity of the region, the relative position of NOx and VOC sources can become even more important at different times depending on wind speed and direction. This is likely to become increasingly important as the region continues to industrialise and oil palm plantations

continue to expand into areas that are currently urban / industrial rather than remote. Please discuss these limitations in the study.

L16-19 - What is the resolution of the NOx emissions input? As previously noted I do have concerns over the capability of the model to resolve the heterogeneity of this region and Hewitt et al. 2010 demonstrated the sensitivity of the atmospheric chemistry in this region to NOx levels over a range of BVOC emissions

L28 - Please re-phrase; “inflated” sounds as if the authors applied an arbitrary increase whereas in both cases the scenarios in which NOx emissions were increased were based on the differences observed between forest and plantation.

L34 - A likely key difference between the work of Silva and that presented here is that Silva applied the GEOS-Chem model at a resolution of 0.5deg x 0.667deg, a far more appropriate resolution for this highly complex region.

p11

L1-3 - But how does deposition change in YIBs which unlike GEOS-Chem couples the atmosphere to a process-based parameterisation of stomatal conductance? Otherwise I’m not sure what point the authors are trying to make here

Figure 1 - Please add a panel showing clearly where the changes in land cover were made. In the SI the figures imply that the changes were made only to the islands of SE Asia; here panel (a) shows a wider SE Asia than this. I suggest it would be useful for the authors to add a bounding box in each of (a) to (c) to show where the LCC occurred. I also suggest that panels should be added to show typical absolute concentrations (perhaps best done with 2010 base) of O3, SOA, isoprene and monoterpene emissions and changes in these emissions.

P12

L3-5 - These 2 sentences appear to be saying the same thing; are both needed?

L5-6 - Please state the heights/pressures being used define upper and lower troposphere; as evident from Figure 1 (d) the reported average changes in ozone will be critically dependent on this boundary.

L6-7 - This seems somewhat negligible (?)

L19-21 - Would suggest that the authors re-order this sentence to aid readability; the previous sentence considered isoprene so would be more logical to start with isoprene here: e.g. "When transport-driven isoprene . . ."

L26 - "-5N"? Please use "5S" for consistency with Figure 2.

L28 - Wolfe et al seems an odd choice of primary reference for the formation of HCHO from isoprene as HCHO columns have been used as a proxy since Palmer et al. 2001, 2006 and several authors since (including Palmer et al 2003) have considered the relative contributions of other VOCs to HCHO which seems of real relevance here as this study considers monoterpenes as well as isoprene, although as previously commented the text seems rather skewed toward isoprene.

p13

Figure 2 - Why are monoterpenes and SOA not included here? As previously noted, monoterpene emission changes and atmospheric composition impacts are barely covered.

L10-11 - but atmospheric concentrations of CO are of the order of 60-120 so this is a small relative change.

L17-19 - Previously the authors have strongly made the case that this region is low-NOx. What is the yield of alkyl nitrates in ModelE2-YIBs from BVOC oxidation under low-NOx conditions as this seems more pertinent than commenting on yields in high-nix environments?

p14

L8-9 - Although there is a clear mis-match between the spatial distribution of the enhancement in nitrates and reduction in NOx concentrations. And in particular, the formation of alkyl nitrates appears particularly increased at the surface where no change in NOx is evident. Could the authors comment on the reason for this?

L11-15 - However, unless the authors have incorporated the "new" isoprene oxidation pathways under low-NOx conditions in the chemistry mechanism (in which case this needs to be made clear in the model description section) this is more a model artefact reflecting the atmospheric chemistry community's understanding of HOx chemistry in 1999. Please clarify the isoprene oxidation scheme in ModelE2-YIBs. Is it really still CBM-4?

L18-19 - Again the issue of BVOC or isoprene or isoprene+monoterpenes; which is being considered here?

L22-23 - Please justify why the model is being driven with a single (repeated) year of meteorology. While it is useful to know the uncertainties associated with internal model variability it would be of far more use to know how inter-annual variability in climate / meteorology affects the radiative forcings calculated here as ultimately what is of real interest is how future LCC in the region might play out.

L23-24 - Now we come back to one of my chief concerns with the manuscript itself. This is now the first mention of aerosol changes. Although the abstract and introduction mentioned SOA as well as ozone the changes in SOA were not presented or discussed anywhere in the results section. If the authors wish to include the effects of aerosol on radiative forcing it is essential that the changes in tropospheric aerosol concentrations are introduced and discussed prior to this; likewise monoterpenes. Is the authors' reluctance to fully consider monoterpenes and SOA due to deficiencies in the chemistry mechanism and/or gas-to-particle partitioning in the ModelE2-YIBs model?

L27-30 - Given the levels of uncertainty in calculations of radiative forcing, and the limitations previously identified with this study, it is hard to see a net forcing of $\sim$0.008

Wm-2 as globally significant. The more substantial (but highly localised changes over the Indian Ocean) could be of real interest in terms of how they affect the Indian monsoon which has seen significant changes in recent years, but this is not explored by the authors. Again, however, these are temporally "averaged" results whereas the "interesting" effects are likely to be temporally as well as spatially localised. This would be another extremely interesting avenue to explore. Do the changes peak at times and locations when small changes in climate-relevant atmospheric components matter?

p16

L1-2 - See previous comments regarding monoterpene and SOA results and discussions. But interesting to note that O3 and SOA forcings seem to scale, presumably also with isoprene emissions changes. But this comes back to my previous questions regarding the fitness of the chemistry mechanism for the conditions encountered in this region and the ability of the model to capture the heterogeneity of land and chemical climatology given its coarse resolution.

L16-18 - See previous questions and comments regarding inter-annual variability; as noted by the authors in L10-12 the isoprene flux is critically dependent on meteorology and so presumably the impact on radiative forcing would be similarly sensitive.

L20-32 - I do not understand why the authors chose a x12 enhancement in isoprene emission factor from the dipterocarp forest given the x3 enhancement observed during in-situ measurements. Further, given the incredibly low isoprene emissions from these forests relative to both other natural tropical ecosystems and oil palm plantations why there was a need for this sensitivity test. Monoterpene emissions are relatively strong from dipterocarp forests so I would have expected to see a sensitivity test involving increased monoterpene emission rates instead. Perhaps the authors could comment on why this was not done?

p17 L2-4 - However, this forest loss is likely to have affected monoterpene emissions more substantially, coming back to my previous questions regarding the importance

of monoterpenes and SOA contributions to the LCC-induced radiative forcing in the region and whether this is well captured in the model used.

L6-9 - Here and in many other sections of the discussion section I feel that the authors have lost objectivity and are attempting to over-emphasise aspects of their results to fit a particular narrative. The study covers a 20-year period. If the annual changes were constant you would expect to see 75% of the forcing associated with 1990-2005; 69% is not so far from that. As the authors have only broken the 20-year period down in one way rather than into 5-year blocks throughout they do not have sufficient evidence that the 5-year period from 2005-2010 is worse than every other 5-year period which would be needed to support the statement that the forcing is "rapidly increasing".

L19-22 - Presumably this underestimation is likely to affect both the 1990 and 2010 land cover maps. Have the authors attempted to find out from other sources (e.g. FOA, Malaysian Oil Palm Board, Round Table for Sustainable Palm Oil) whether the proportion of smallholder plantings to industrial plantations has remained constant during the rapid expansion of the oil palm industry or whether the number of smallholder plantings has remained closer to the 1990 figures?

L26 - Can the authors clarify what they mean by "extrapolating the smallholder estimate"? Do they mean they assume that 40% of reported oil palm area across the whole of SE Asia and through the entire time period represents smallholdings? So they assume that the area they have taken from Gunarso represents only 60% of the actual extent of oil palm cultivation? See above regarding justification of this assumption.

L33-34 - To what extent is this insensitivity the result of the coarse resolution and outdated chemistry scheme?

p18 L5-10 - Please explain why an increase in surface ozone concentrations could not be concomitant with increases in upper tropospheric ozone?

L11-12 - Please could the authors attempt to list some of the uncertainties not considered by the sensitivity tests that might be expected to be substantial?

L21-22 - The authors have just stated that their best estimate is +8.4; that is therefore the figure that should be quoted here, in which case we are looking at 3.5x

L27 - As highlighted previously, the authors are over-emphasising the magnitude and implications of their findings. Using 8.4 suggests a figure of 11.5 rather than 12.7 mW m-2.

L28-31 - It would have been of real interest if the authors had looked at future projections rather than confining the study to historical LCC and radiative forcing.

p19

L1-2 - Just out of curiosity, how much more uncertainty is associated with isoprene BERs from oil palm in comparison with isoprene emissions from other tropical species / ecosystems OR monoterpene BERs from rubber palms which the authors earlier highlight as important factors in the changes in BVOC emissions in the region.

L10-19 - Equally, many previous studies (e.g. Grate et al 2007) have shown strong apparent seasonality in BERs that are not adequately accounted for by consideration of leaf age. Inclusion of seasonally varying BERs might also be argued as improving the estimated radiative forcing in this study.

L18-19 - The isoprene vs BVOC issue again. Presumably given the focus of the paragraph to this point the authors are referring to the isoprene BER in YIBs.

L18-19 - And again, the issue of misrepresentation. In the title and throughout the manuscript the authors refer to BVOC emissions and emission changes yet YIBs includes only a very limited number of BVOCs, and here the authors have only altered isoprene and monoterpene BERs. I suggest that the authors remove the term BVOCs from the title and discussions as it is not an accurate reflection of the study performed. Likewise the authors need to devote far more attention to the changes in monoterpenes

and SOA throughout the main text.

L21-34 - This discussion of OH recycling in the conclusions section is disturbing on a number of counts. (1) This is the first and only discussion of the apparent limitations of the chemical mechanism in ModelE2-YIBs, as previously commented above. Can the authors please describe exactly what BVOC oxidation chemistry is included in the “based on Carbon Bond Mechanism-4” scheme? As previously noted, CBM-4 was developed for high NOx anthropogenic VOC-rich urban environments and such schemes have been found to inadequately capture observed concentrations / chemistry / oxidation products in low-NOx high BVOC environments such as those in SE Asia. (2) The field of isoprene oxidation chemistry has moved on considerably since the sensitivity studies employing crude “OH-recycling” schemes referenced here with new pathways identified leading to the regeneration of HOx in low-NOx environments. (3) While surface ozone concentrations might be only negligibly affected the authors have repeatedly argued elsewhere that these are not the changes that are significant in terms of radiative forcing. (4) The new understanding of isoprene chemistry gained in trying to reconcile the apparent differences between modeled and observed gas-phase chemistry has also identified mechanisms driving high yields of isoprene-derived SOA via MACR oxidation. The points made in this paragraph bring into question the validity of the modeled changes in ozone and SOA presented here. It also raises questions regarding the monoterpene oxidation scheme and gas-to-particle phase partitioning included.

p20

L1-2 - Actually this should have been included in this work as the chemistry seems to be a critical source of uncertainty that has not been adequately considered.

L8-9 - Concluding with the absolute maximum single pixel increase is not scientifically balanced. Please maintain objectivity rather than cherry-picking the results to fit a particular narrative.

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