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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Land cover change impacts on atmospheric chemistry: simulating projected large-scale tree mortality in the United States" *by* J. A. Geddes et al.

Anonymous Referee #1

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General comments

This paper describes the inclusion of a diagnostic representation of land cover in GEOS-Chem and presents the results of model simulations exploring the effects of projected land cover change due to tree mortality on air quality in the conterminous US. This is a timely and well-structured study. While it is not the first to consider the impact of land cover change on atmospheric composition the majority of previous work have focused on anthropogenically-driven changes. Given the high levels of tree mortality anticipated in the US and the substantial contribution of biogenic compounds to surface concentrations of ozone, NOx and aerosols such a study is of real interest.

Overall, the study is well presented, described and discussed. I have no hesitation in recommending its publication in ACP once the following minor comments are addressed.

Specific comments

Abstract

P29304, L13-14 – In the context of the paragraph, the sentence starting "The O3 response to emissions is controlled by ..." makes it sound as if this is a feature of the model. Perhaps the authors could phrase this differently to make clear that occurs in actuality.

P29304, L17 – Please explain the choice of this threshold; why 65 ppb?

P29304, L24-25 – As the authors go on to make clear in the introductory sections of the paper, this is not the first study to demonstrate the importance of biosphere-atmosphere interactions to air quality and climate. I suggest the authors could perhaps phrase this statement in such a way as to acknowledge this, perhaps by saying that it "further underlines the importance of . . ."

1. Introduction

P29305, L9 – Surely the authors could cite a more up-to-date reference than 2001? Perhaps Laothawornkitkul et al., 2009 or Mellouki et al., 2015?

P29305, L28 – "or not" reads rather strangely, do the authors mean "natural processes"?

P29306, L3 – Also Ganzeveld et al., 2010, which I believe was the first study to demonstrate the extent to which changes in O3 dry deposition could offset changes in biogenic emissions etc. due to LULCC.

P29306, L22 – I suggest the authors add "fully" before "explored" here as they then go on to describe a study that did just this.

ACPD
15, C10517–C10526, 2015

P29307, L3-6 – Changes in local micro-climate due to changing vegetation could also be expected to affect dry deposition.

2 Model Description

My chief concern with the paper is the lack of a clear description of the relevant chemistry included in the model. Given that the motivation for the paper is stated as being to investigate how land cover changes affect atmospheric chemistry and composition it is an unjustifiable omission. The authors take great care to explain the biogenic emissions, soil NOx emissions and dry deposition parameterizations but leave the chemistry description to a single line of "detailed HOx-NOx-VOC-O3-aerosol chemistry". Of particular importance, given the findings that substantial decreases in mono- and sesqui-terpene emissions are observed, would be a description of the treatment of the subsequent atmospheric reactions of these species. Are they treated as specific compounds or lumped groups? Are their oxidation pathways explicitly included, or just the initiation reaction with imposed SOA yields (e.g. similarly to the 2-product aerosol schemes)?

2.1 General Description

P29307, L24 – Is there not a peer-reviewed model description for GEOS-Chem?

P29308, L5-6 – See above comment. How up-to-date are the monoterpene and sesquiterpene chemistry? Is MBO chemistry included?

2.2 Default land-atmosphere exchange

P29309, L7-8 – Please could the authors list the compounds included as primary biogenic emissions, and indicate how they are lumped in the GEOS-Chem mechanism.

P29309, L7-8 and P29310, L11 – Please could the authors comment on the appropriateness of using MEGAN v2.1 emission factors with MEGANv2.02 algorithms. The parameterizations of emission rates were also altered between the two versions of the model, for example through the introduction of the light-dependence factor. Have the

ACPD

15, C10517–C10526, 2015

authors checked the consistency of the emissions estimates?

P29309, L16 – Are the roughness lengths not also a function of the land cover?

P29309, L28 – Please explain briefly how this interpolation is carried out.

2.3 Modifications to land-atmosphere exchange

P29310, L6 – Please replace the phrase “on-the-fly”.

P29310, L7 – Please could the authors explain their choice of Year 2000 as the present-day baseline year. AR5 took 2010 as the “handover” year between past and future land cover.

P29310, L19 – I’m not sure that I agree with the authors are making here (or maybe I do not understand the point they are trying to make). Even if the land cover characteristics are determined using fractional coverage the resolution of the land cover data set and model simulation will affect these characteristics.

2.4 Impact of updates

P29311, L13 – I do not understand the point the authors are making here. Just because the spatial correlation is high does not seem to me to necessarily mean that the new simulation is not “degraded”. Please clarify this. Surely it is only through comparison with observations that any statement of accuracy or otherwise (which is what is implied by the term degrade) can be made.

P29312, L19-23 – Again I am confused by the point the authors are trying to make. I assume that the authors mean that they took the (for example) 10% loss from the NIDR and applied that to all tree species in a particular grid cell rather than assuming that this loss was specific to one plant functional type only.

3 Predicted tree mortality

P29313, L5 – Do the authors mean “substantially” (in which case please could they

ACPD

15, C10517–C10526, 2015

quantify this) or that needleleaf and broadleaf trees are equally affected?

4 Impact of tree mortality on atmospheric chemistry

While the different simulations are well described here it would be a great aid to the reader if the authors were to include a table listing the simulations. This table should include a short name for each together with a description of the differences from the base scenario. It is currently difficult to follow the later results and discussion sections as the "additional simulations" that seem at this point to have a lesser status than the first two are given a fair degree of prominence in some of the later sections.

P29313, L20-21 – While June-August may be the season in which total biogenic emissions occur, I would be surprised if this were the case for individual species of importance in the context of air quality. For example, monoterpene emissions are well documented to peak during the spring (April/May) in many northern regions. However it is likely the case that the subsequent rates O3 and SOA formation peak during the summer months. Can the authors comment on whether emissions and O3/SOA formation do indeed peak in all of the regions of importance (e.g. NW USA) in this study in June-August?

4.1 Impacts on biogenic emissions and deposition velocity

p29315, L1-5 – Can the authors comment on how realistic this large increase in soil emissions is?

P29315, L6-7 – Simulations (1) and (2)? See earlier comments.

P26315, L12 – How is stomatal conductance treated in GEOS-Chem given that it does not have an explicit representation of vegetation?

P29315, L16-20 – Does this similarity imply that roughness length is of more importance for O3 deposition in this context than stomatal conductance?

4.2 Impacts on surface ozone concentrations

ACPD

15, C10517–C10526, 2015

This section is particularly difficult to follow in terms of which simulation is being referred to (see above comments regarding the addition of a table and short names for each simulation). Furthermore, the order in which the results are presented and discussed seems odd. I suggest that the authors reorder this section so that the simulations are presented in order (i.e. the results from simulation (2) before those of the sensitivity tests (3) and (4)). The same comments apply to Fig. 6. If Fig. 6a shows simulation (1), Fig. 6b should show simulation (2) and so on.

P29315, L23 – Please clarify what measure of mean surface O3 (daily, monthly, 3-monthly) is being used.

P29315, L27-P29316, L1 – Is this simulation (4)?

P29316, L1-3 – Is this comparing simulations (3) and (4)? Why not add a panel to Fig. 6 to show this?

P29316, L4-6 – What percentage changes are these?

P29316, L8-9 – Is this simulation (2)?

P29316, L8-23 – While the authors do discuss the uncertainties in dry deposition rates and the high variability between models later it would be good to introduce this here as I assume that it is in part the reason behind the order in which they have chosen to present the results.

P29316, L19-21 – Is this simulation (2)?

P29316, L21-23 – I would like to see this statement given more prominence. The results shown here should act as another call to arms for the modeling community to address the deficiency in our modeling of dry deposition.

P29316, L24-25 – Is the E Coast considered to be remote from pollutions sources and therefore low NOx?

P29316, L24 – P29317, L10 – I would recommend that the authors label these regions

ACPD
15, C10517–C10526, 2015

on one of their figures of the USA or introduce a new figure for this purpose. Not all readers of ACP will be familiar with the nuances here. How is the mid-Atlantic region different from the Appalachians for example?

P29317, L10-12 – Perhaps the authors could clarify this statement. Presumably they mean the sign of the response rather than the magnitude, although Figure 7 suggests that even this is not clear cut?

P29317, L16-18 – I really like this way of analyzing and presenting the results shown in Figure 7 and described here. However, the authors need to back it up by showing that there is indeed a statistical difference between the distributions; it is certainly not obvious that this is the case for the top panel.

P29317, L19-21 – See the above comment. This seems a rather optimistic claim given the little apparent difference between the distributions.

P29317, L21-23 – This is not a new finding so please reference other cases where this has been observed or demonstrated.

P29318, L4-8 – Biogenic emissions also show a strong diurnal pattern which must also contribute to the observed changes.

P29318, L14-L27 – Again, please present and discuss the results in a logical order. Why start with a sensitivity test that does not include all of the factors altered by changing land cover?

P29318, L14-17 – Please make clear again that “clean” and “polluted” regions in this analysis only include 10% (each) of the grid cells.

P29318, L22-L28 – Again, is this considering only a total of 20% of all grid cells? How would these figures change if the authors applied a threshold of percentage land cover change (e.g. the 10th percentile of gridcells with at least 5% change in land cover)?

4.3 Impacts on reactive nitrogen

ACPD

15, C10517–C10526, 2015

In general, I found this section much easier to follow than the previous but would still recommend the authors state the simulation number rather than simply describing the scenario.

Perhaps the authors could also comment on the implications of the impacts on NOy. P29319, L2-3 – Is this simulation (2)?

4.4 Impacts on organic aerosol

P23920, L2-3 – Simulations (1) and (2)?

P29320, L3-5 – Surely this is simply a function of the chemistry mechanism?

P29320, L7-13 – Please provide context for these changes. Perhaps the authors could remind the reader of the EPA threshold limits for aerosol.

P29320, L10 – I would suggest moving the phrase "the model predicts" from L12 to this statement to make clear that all of the percentages quoted here are deduced from the model rather than observations.

P29320, L13 – Please provide a percentage change or a baseline for comparison for the changes in the northwest.

P29320, L14-16 – How is dry deposition (settling) of aerosols modeled within GEOS-Chem?

P29320, L18-21 – Perhaps the authors could distinguish between the different terpenes? Presumably the highest relative impacts occur in regions with the highest proportion of monoterpene (and/or sesquiterpene) emissions rather than those where isoprene emissions dominate.

5. Discussion

P29321, L11-15 – See previous comments regarding the structure and order of results. It would greatly aid clarity if the authors were to present and discuss the projected final

ACPD

15, C10517–C10526, 2015

result (i.e. accounting for all changes, simulation (2)) first before unpicking this by considering the sensitivity tests (simulations (3) and (4)).

P29321, L19 – The authors might consider rephrasing their statement that this "improves air quality".

P29321, L20 – I suggest that the result does depend on the SOA model use (rather than "may").

P29322, L3 – Were NOx emissions the only changes? For example, sulfate emissions have well demonstrated effects on SOA yield and have also changed markedly.

P29322, L3 – Please could the authors check this statement. According to their description of GEOS-Chem (section 2.1) the base scenario already used anthropogenic emissions for 2005. Please could the authors also state clearly what the difference in NOx emissions were (e.g. on average a 5% decrease)

P29322, L4-6 – Please quantify or otherwise clarify how the sensitivity changes when 2005 NOx emissions are included.

P29323, L1-2 – Is this not also likely to be a temporary effect?

P29323-P29324 – Human response to "natural" changes in land cover and subsequent intervention is also a factor that is not considered here.

6. Conclusion

P29325, L3-6 – Again the authors might consider rephrasing this final conclusion.

Figures

See previous comments regarding the order of presentation of results for comments on specific plots.

Fig. 2-Fig. 9 – I would strongly recommend that the scale is altered for all of the panels showing differences. While it is always nice to have differences centred on

ACPD

15, C10517–C10526, 2015

zero, in most of these cases the differences have the same sign and it is very hard to distinguish between different magnitudes of changes with the current scales.

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ACPD

15, C10517–C10526, 2015