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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Large difference in aerosol radiative effects from BVOC-SOA treatment in three ESMs" *by* Moa K. Sporre et al.

Anonymous Referee #3

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This paper compares the responses of three Earth System Models to changes in BVOC emissions and BSOA yields, with regard to radiative effects. It is very useful to compare models in this way, and important to show that such models can give very different results if used to investigate the climate impacts of BVOCs.

Although the paper is generally well written, I think the caveats and conclusions need to reflect some of the issues which this study couldn't address. I actually disagree with the last line of the conclusions which says that "in particular" one needs to work more on how NPF parameterizations affect size distributions. Although I agree that these NPF issues are important, I don't see the evidence that this is the main problem with SOA modeling. I suspect it reflects more the author's plans and interests than the general level of SOA understanding.

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Discussion paper

A few connected issues are:

The authors clearly state that their semi-volatile SOA compounds aren't semi-volatile - they are formed as inert and condensed species upon BVOC oxidation. This is a major assumption, but the implications are not discussed anywhere in the manuscript. Compared to a true VBS equilibrium framework these models presumably give much more SOA in less polluted regions, and in the free troposphere. Isn't this one of the main uncertainties of all of these models?

No model evaluation is presented, and no indication is given as to where such evaluations can be found. The model versions used here seem to differ from those used by Tsigaridis et al, 2014, so readers have no idea if the modelled SOA are reasonable or not.

There is no comparison of the BSOA production calculated here with that of other studies, e.g. Hallquist et al 2009 or Tsigaridis et al. 2014.

Although the paper mentions the Spracklen et al 2011 study concerning anthropogenic influence (also commented below), there is no mention of the role of NOx on the BSOA yields assumed in this paper. Most VBS schemes would have both high and low NOx yields, and perform some interpolation between them depending on oxidant availability. If one believes in some anthropogenic influence, then the assumed yields should depend on NOx as well as oxidants.

I also missed any mention of POA, BBOA, or ASOA in Sect. 2 and elsewhere. Do these models only have BSOA? What are the implications of this?

Other issues

p1, L10. Why 10 years? The importance of BVOC to SOA formation has been known for decades!

p2, L1. Say implies rather than introduces

p2, L5. Not all BVOC are "quickly" oxidized.

p2, L10. Give reference for the direct aerosol effect comment

p2, L11. Add radiation as one of the major drivers of BVOC emissions

p2, L12. It is usually good to cite articles if possible, and could have used e.g. Hantson et al 2017 or Schurgers et al. 2009 here.

p2, L13. There are several studies suggesting that increased CO2 can inhibit BVOC emissions (e.g. Arneth et al, 2007, refs in Hantson et al 2017). This is also a major source of uncertainty that needs a mention.

p2, L22. Season matters. BSOA often dominates SOA in summertime, but there is plenty of evidence that in wintertime wood-burning often dominates or plays a major role (e.g. Brown et al., 2016, Glasius et al., 2018).

p2, L24. Although the Spracklen et al 2011 study was very innovative and interesting, there are several issues with the conclusions, see e.g. Hodzic & Jimenez, 2011. This question is very complex and unresolved as far as I know.

p3, L30-33. I think the sentences starting on L30 are very clear, fair, and with the important caveat represented by the last sentence. These lines could usefully be used in the abstract.

p5, L2. The descriptions are brief, not "detailed".

p5, L13. Which MEGAN version?

p6, L1. So, is this a new version of NorESM, or a version used just for this study? If the latter, then the conclusions aren't relevant to other NorESM work, which would seem to remove some of the point of including this model.

p6, L9. I think you mean hydroxyl, not hydroxide, and better to say nitrate radical, as nitrate is often used for the aerosol compound.

p6, L11. This was confusing. If I understand right, one has two types of ELVOC then, one that can influence NPF, and the other behaves exactly as L/SVOC. As you ELVOC and L/SVOC compounds have the same mass, why not simply put the non-NPF ELVOC in as L/SVOC?

p7, L32. This was also confusing. Table 1 suggests that ECHAM uses fixed yields of L/SVOC and ELVOC, but L32 suggests partitioning depends on pre-existing organic mass. And what is meant by pre-existing OM? Does this influence the DRE/CRE calculations?

p9, L2-7. I am not sure the argument about interactive oxidants can explain a factor of 3. Sure, when the BVOC are emitted one can expect reduced OH and other oxidants, but this just delays the oxidation close to the surface. Isoprene which isn't oxidized near the surface will still be oxidized a little further up in the troposphere. Why would the total amount change by a factor of 3? Did you check changes in oxidant fields associated with this argument?

p16-17. This is where I think the limitations and results of this study need to be put alongside the many other uncertainties surrounding SOA modelling.

p26, Fig. 1. State which years are shown here. Also, I was surprised to see no error-bars on the ECHAM runs, and that even those for NorESM were so small. Did ECHAM also just use one fixed year (2000) of BVOC emission, same as EC-Earth?

References

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