We would like to thank both reviewers for their constructive comments. We tried to address all of them, as detailed below. On top of the referees' comments, we also made a few minor changes to the manuscript:

- The name and affiliation of co-author Ng has been corrected.
- A new affiliation was added for co-authors Hoyle and Shindell.
- The CRPAQS has been removed from the measurements section; although we have it in our files, there are no valid data in them. This does not affect the results, since all data from this database were treated as missing.
- We fixed a typo: the first-order loss rate of 12 h mentioned for the SOA precursor gases in GISS-CMU-TOMAS and GISS-TOMAS is really a chemical lifetime of 12 h, since the rate is in units of inverse time.
- Very few editorial changes were made throughout.
- We added a sentence at the end of the manuscript: "Improved laboratory measurements of SOA formation is also crucial for the model improvements (Zhang et al., 2014)".
- Small additions made in the acknowledgements.
- We added a reference in Table 1, for CCSM4-Chem: Heald et al. (2008).
- We added a very brief description of the Supplementary figures at the beginning of the Supplement.

Anonymous referee #1

This manuscript describes an AeroCom intercomparison of 31 global model organic aerosol simulations. The study documents the state of modeling in this area and highlights the diversity of simulation approaches and global budgets. I have two major comments, followed by more minor comments or suggestions.

Given that few of these particular models include the marine and biological sources of OA, their inclusion in the general comparisons seems misleading and obscures our ability to compare the model treatments of OA. Presuming that the models which include these sources can separate these contributions, I would suggest that the authors present these as their own separate OA type, not part of the standard POA or OA budget. The discussion of these sources could also be substantially reduced throughout (or moved to the Supplement).

Unfortunately it is not always possible to separate mPOA (and MSA for CAM4-Oslo) from tPOA, since CAM4-Oslo and IMPACT do not track them separately. In addition, although including new POA sources to the total POA budget might appear misleading at first, in reality it more accurately represents the diversity of models in representing POA, regardless of source. It makes little difference if models use different emission inventories for the same emission sectors, or use the exact same emissions but with some models adopting additional OA sources. Obviously there are issues concerning the spatial and temporal emission patterns, but these exist both when using different emission inventories and/or different source sectors. Our aim is not to compare the different OA (or POA) sources and how their diversity reflects in OA levels; this was explored previously in AeroCom (Textor et al., 2007). Rather, we

aim in putting the analysis of the model results under a global OA perspective, i.e. understand the OA budget by accounting for all known potential sources, focusing on those included in the models, even though not all sources are represented in all models. The representation of various categories of POA and SOA sources in the total OA budget is an implicit part of our analysis and is not only of secondary importance to be moved to the supplement.

I would strongly suggest that the authors considerably trim the text and move extraneous results to the Supplement. As is, the manuscript is quite lengthy and challenging to read. I highlight below some specific suggestions to reduce the length of the manuscript.

We thank the reviewer for their useful suggestions in significantly reducing the manuscript size. We now have 23 figures, compared to the original submission which had 37.

1. There are a number of sections which are unnecessarily detailed and do not provide substantial new scientific insights.

a. I suggest that the authors merge and trim 4.1.4 and 4.1.5 on deposition/lifetime which are somewhat repetitive.

We were not able to identify the repetitions mentioned by the reviewer. Clearly removal and lifetime are linked, but in Sect. 4.1.4 (deposition) we only discuss about fluxes, while in Sect 4.1.5 about lifetime. The effective deposition rate presented in Sect. 4.1.4, in units of inverse time, is still a flux unit, not a time unit, so there is no repetition. No changes made.

b. Section 4.1.7 (comparison with AeroCom phase I) does not provide any substantial new insights. I suggest trimming this down to one paragraph and including it in the introduction to set up the study.

We believe that a comparison with the previous large-scale model intercomparison provides valuable insight on how models have changed (and, importantly, not necessarily improved) during the last decade, thus reducing this discussion to a single paragraph will weaken the manuscript. To satisfy the reviewer, we decided to delete section 4.1.7 and spilt its content as follows: Move the old Fig. 13 earlier, in the discussion of the results in the introduction of section 4.1; it is now Fig. 1. The introduction on ExpA and ExpB has been also moved there, since it is essential for the figure. The remaining parts of section 4.1.7 have been added in the relevant subsections (e.g. emissions/deposition). This helped avoiding some duplication in the discussion, reinforced the conclusions/suggestions from AeroCom phase II model analysis, and making the text shorter and more concise.

c. Section 4.3.3: The detailed composition from 5 models is not particularly informative and is a selective analysis of 31 models which are the main focus. I suggest that this discussion be moved to the Supplement.

We believe that this section is very original and particularly informative with regard to the chemical composition of OC/OA from different global models, including their seasonal variability. Important parts

of the discussion and conclusions are drawn from this section. Although for the interest of space we present five models in the figures, the discussion section takes into account all models, and the corresponding plots of the remaining models exist in the Supplementary Material. We do agree with the reviewer though that this is a rather long section, thus we decided to cut it in half, by keeping the most interesting stations in the manuscript, and moving the remaining ones (and their discussion) in the Supplement.

We decided to keep the following stations:

Finokalia (old Fig. 28/29): the only station where we have both OC and OA measurements.

Welgegund (old Fig. 30): the only station in our database where a full year of OA data exists. Note that we now show the HOA/OOA split in the figure, in order to be able to plot the measurements in the same graphs. The original OC speciation is available in the Supplement.

Alaska (old Fig. 31): A remote station with very diverse model results.

Manaus (old Fig. 33): One of the two Amazon stations we have. We decided to keep this one instead of Alta Floresta, because it is less affected by biomass burning during the dry season; in Alta Floresta the SOA signal is completely suppressed by the primary OA biomass burning peak.

Amsterdam Island (old Fig. 36): A remote marine station with long-term OC measurements, which is typically used to constrain models for their primary marine sources.

The figures with the remaining stations, together with their corresponding explanatory text from Section 4.3.3, were moved to the Supplement.

d. Section 5 repeats many of the result discussed in the main text, I suggest that a briefer summary would be more appropriate for a conclusions section.

Given the length of the discussion, we find that summarizing the main findings was necessary. We split the section into two sections, one for the conclusions and one for the perspectives (now named future directions), in order to have the results summary separate from the implications and future directions we suggest.

2. There are far too many figures in the main text. Here are my suggestions to eliminate/merge figures:

a. Figure 1: difficult to see, move to supplement

Done; it is now Fig. S 2 in the supplement.

b. All Figures 2-12: the bottom two panels are repetitive/unnecessary. I would suggest removing all of these and then merging figures as follows: Figure 2+3, Figure 4+5+6, Figure 7+8, Figure 9+10+11

We moved the bottom panels in the supplement, and grouped the figures both in the manuscript and the supplement as suggested.

c. Figure 17-21: show a lot of (redundant) information not discussed in the text. I suggest showing simply the MNB panels and moving the others to the Supplement. All the MNB plots could be merged into one overall figure.

We deleted the slope plots both from the manuscript and the Supplement, but decided to keep the correlation, which is important in quantitatively interpreting our results.

d. Figures 25-37: move to Supplement

See answer in comment #1c above.

MINOR

1. Page 6031, lines 13-14: how can a "modeled vertical distribution" show "diversity of over an order of magnitude"? Do the authors mean the concentrations vary by over an order of magnitude? Or somehow that the shape (i.e. the vertical distribution) varies between models?

We mean that the concentrations vary by over an order of magnitude across all models. We added the word "concentration" in that statement: "Diversity of over an order of magnitude exists in the modeled vertical distribution of OA concentrations that deserves a dedicated future study."

2. Page 6031, line 27: typo "ratio is calculated."

Corrected.

3. Page 6032, line 16: spatial or temporal correlations?

We modified the sentence to: "The mean temporal correlations across all stations are low…"

4. Page 6032, line 20-21: what do the authors mean by "processes"? examples or more precise language would help clarify.

We modified the sentence: "…knowledge about the processes that govern aerosol processing, transport and removal, on top of their sources, are important…"

5. Page 6033, line 10 vs line 12-13: Please be consistent in reporting either the means or the range.

We now report only means with uncertainty and not ranges.

6. Section 1.1: It would be helpful to fold in the definitions of Section 1.6 and 2.3 into this section such that one section provides a coherent overall discussion of terminology. For example, Table 4 precedes the descriptions of Section 1.6 and is therefore confusing.

We agree that the definitions can appear earlier. We moved old Section 2.4 to the new Section 1.7, and old Section 2.3 to the new Section 1.8. Table 4 now appears as Table 1 and Table 5 as Table 2.

7. Page 6034, line 26: what is an OA "component"? Do the authors mean a specific composition type? Or OA from different sources? Or. . .?

We changed "components" to "compounds". This can include both real chemical compounds, and the species that models include in their calculations.

8. Page 6036, line 5: "improve" suggests that the model descriptions of these properties are currently in error. Is there specific evidence of that? Perhaps "investigate" would be a better word.

We believe that "improve" is a proper term to be used here. It does capture the meaning of error, which certainly is the case when a model assigns uniform physical and chemical properties to all OA, but also captures the meaning of limitations, for models that have some complexity but not on everything. As an example, models might handle different OA tracers with different solubilities, but not different optical properties.

9. Page 6036, line 9: "mostly neglected" oversteps. Many models do include anthropogenic SOA

We changed "mostly" to "frequently". About two thirds of the models participating in this study do not have an anthropogenic source, like virtually all climate models that participate in CMIP5.

10. Page 6038, line 7: typo "oxidation to the"

Corrected.

11. Page 6039, line 9: Volkamer et al. is a not "a review", and they show that the underestimate of SOA by models increases as a function of photochemical age, not "long-range transport"

We changed the statement to "They showed that the underestimate of SOA by models increases with photochemical age, which can be partially correlated with long-range transport, …".

12. Table 1, 2, and 3: add the emissions totals for each model

The only information we have on emissions is already included in the manuscript; unfortunately, we are not able to provide neither the emissions per sector per model, nor those of the SOA precursors.

13. Table 5: It would be helpful to indicate which models include IVOC (such as GISSCMU-VBS as described on page 6048) in this table.

This is the only model that includes IVOCs, and we selected not to mention it explicitly in the table. We modified the statement in page 6048 though: "This model, which is the only one in the present study that takes into account the intermediate volatility species as additional sources of OA…"

14. Section 2: please also include the enthalpy of vaporization used for the SOA models in a table.

Added; it is Table 5 in the revised manuscript.

15. Section 2.2: In each section it would be useful if the author provided ranges of emissions.

See answer of comment #12 above.

16. Page 6048, line 8: replace "alpha" with symbol

Done.

17. Page 6048: the description of ntrSOA would generally benefit from some numbers so that the reader gets a sense of the importance of these sources.

Given the limited availability we have for the detailed model budgets, we added some numbers that provide a sense of how important these sources can be, by modifying the last sentence of the ntrSOA discussion: "For IMPACT, 52% of the total SOA comes from glyoxal and methylglyoxal multiphase chemistry (Lin et al., 2012). IMPACT also includes ntrSOA formation from the uptake of gas-phase epoxides onto aqueous sulfate aerosol (Paulot et al., 2009), which contributes by 25.1 Tg a-1 (21%) to the total SOA formation (Lin et al., 2012)".

18. Section 3: include years for the observations listed here

Added.

19. Page 6053, line 24: typo "of AMS OA measurements has"

Corrected.

20. Page 6054, line 22: why is the cutoff diameter of aerosols not expected to be a significant issue? Particularly given that the AMS OA size cut-off is often considerably less than 1 um, it seems that different measurements could certainly represent varying degrees of fine particle mass closure.

The reviewer is absolutely correct, and this is what we state one line earlier: for different types of measurements, different masses can be obtained (Koulouri et al., 2008). What we also state in the following sentence of the same page is that the "not significant issue" will only apply to the current modeling approach: no models have coarse aerosol sources, thus the difference between $PM_{2.5}$ and PM_1 will not be resolved either way, since all emissions are assumed to go to sizes far below 1 μm, thus within the AMS range, even for models with aerosol microphysics. We modified the sentence to make this more clear: "The cutoff diameter of aerosols can also be an issue (Koulouri et al., 2008), but it is not expected to be significant in the present study, given the assumptions that the models adopt for the primary OA sources".

21. Page 6056, line 18-19: why talk about CAM5-MAM3 here when it's listed as a model with SV SOA in the tables?

Following comment #22 below, this statement does not appear in the manuscript any more.

22. Page 6056, line 18: why are these two models listed as outliers? They do not appear to be the most extreme models from the figures?

We thank the reviewer for catching this statement, which is a remainder of an older version of the manuscript, when not all models have provided SOA formation yields. This sentence is now deleted.

23. Page 6057, lines 9-10: reference for this statement?

We added a reference (Surratt et al., 2010).

24. Page 6059, lines 15-21: why is this not seen in CCSM4 which is also driven by GEOS-5 meteorology?

Although both models are driven by the same meteorology, the exact details are not the same. CCSM4-Chem is only using a few parameters to drive the model (wind, temperature, surface pressure), with all other physical parameterizations recomputed by the model's physics. In the case of GEOS-Chem, a very long list of parameters from GEOS-5 is used, and is available here: http://wiki.seas.harvard.edu/geos-chem/index.php/List_of_GEOS-5_met_fields.

25. Page 6060, line 14: isoprene SOA is also typically a new source not included in AeroCom phase I

The reviewer is right. We added isoprene in the new sources listed there.

26. Figure 15: Here a single figure seems inadequate. The regional details described in the text are not apparent from the global means. I suggest that the authors show mean vertical profiles for the tropics, mid latitudes and biomass burning regions discussed in the text.

We expanded the figure to include the global mean (as before), South America, northern mid-latitudes and tropics.

27. Page 6071, line 5 and page 6082, line 6 and page 6088, line 28: While it's interesting to hear that the authors plan to follow up with future studies, text to this effect should be removed from the manuscript unless follow-on manuscripts have already been prepared for submission.

Although the analysis is ongoing, since it is not ready for submission, we deleted these sentences.

28. Page 6072, line 20: I suggest that "less bias" would be clearer than "increased MNB"

Changed in "less bias".

29. Section 4.3.1:These comparisons raise the question of why models differ significantly in skill when simulating OA vs. OC (presumably the answer is that the OA:OC applied to the model simulations is not very well constrained). The discussion of this in section 4.3.3. should be integrated with the discussion here.

We added the following statement in Sect. 4.3.1.4, where we already discuss this: "Measurements of OA and OC at the same location have a different seasonality, as presented later (Sect. 4.3.3) for Finokalia, Greece, which is not evident in the models results. This shows that the OA/OC ratio changes with atmospheric processing, and as applied in the models simulations (in most cases by a spatially and temporally fixed ratio), is not appropriate."

Anonymous referee #2

This paper presents a comparison of global organic aerosol models that participated in the AEROCOM intercomparison. Models in the study range from relatively simple with nonvolatile POA and SOA calculated based on a fixed yield to volatility basis set treatments with semivolatile POA and non-traditional SOA. Some of the main conclusions of the paper include:

1. Global organic aerosol models are growing in diversity

2. Models generally underestimate organic aerosol

3. Models are likely missing an anthropogenic OA source

4. Increasing model complexity does not necessarily lead to better performance

5. There is a need to understand not just the sources of OA, but the processes by which sources form OA

Overall, a comparison of this type is definitely worthy of publication in ACP. Some changes can be made to improve clarity and make the messages more concise for future readers.

Major comments

Some of the aerosol classification categories (traditional POA, traditional SOA, marine POA) do not seem mutually exclusive and it is unclear where certain mass resides. The authors should focus on how their paper presents the quantities (not just how the models label them). Specifically, according to the authors some models include a non-volatile SOA source as part of traditional POA (tPOA). This occurs in models that have a direct yield of SOA from monoterpenes. Wouldn't it be better to label this direct yield distinct from POA? Even though those models form aerosol directly from VOC emissions, isn't it more correct to term that aerosol "effective SOA" than "POA?" It is a little unclear if this effective SOA is included in the POA emissions of Figure 2b or Fig 3b. Also, a figure similar to Figure 2, but including the sum of all POA and SOA (figures 2 and 3 combined) would help more evenly compare models where POA may include SOA. Furthermore, dividing modeled OA into HOA and OOA (page 6050) seems challenging if nonvolatile SOA is part of tPOA.

Following both reviewers comments, we merged the old Fig. 2 and 3, and added the sum of OA sources, which is exactly the sum of POA sources and SOA chemical production (including the pseudo-primary SOA source). We also improved the caption of the new figure (Fig. 2) to take that into account. In general the separation between POA and SOA was made based on the actual OA sources, not on the models terminology; where models have the pseudo-primary SOA source included into POA, we split the two contributions into their corresponding parameters.

For the split between HOA and OOA, as already mentioned in the manuscript, we take into account the species hygroscopicity as defined in each model, not their volatility. In essence, this makes OOA equivalent to WSOC, which is the best we can do with the current state of OA modeling in the present study.

The classification of MSA is also a bit confusing since page 6049 indicates MSA is included in tPOA for CAM4-Oslo but exists as a separate mPOA which is a subset of tPOA in IMPACT. It seems as though this would lead to double counting in: OA=tPOA+mPOA+trSOA+ntrSOA+MSA

There is no double counting, and we added a sentence right after the equation to make it clear: "The models that have mPOA, SOA and/or MSA included in tPOA do not track them separately, so there is no risk of double-counting any OA species".

Overall, the paper could be shortened by moving some information to supplemental information. For example, site descriptions on page 6085 could be moved.

Following the suggestion also made by referee 1, we drastically reduced the number of figures and shortened the discussion, by moving figures to the supplementary material, and by eliminating a few. Please see our replies to the major comments of reviewer 1 for details.

Minor comments

1. Would it be better to present MNB as percentages instead of fractions? That way they would have "units" of percent in the text so as not to be confused with ug/m3. This is a stylistic choice so the author may leave as-is.

We followed the referee's suggestion; note however that the MNB figures are now rearranged, as suggested by reviewer 1.

2. The last sentence of the abstract emphasizes the need to separate natural and anthropogenic OA to determine climate impacts. Is this driven by differences in their spatial distribution or properties of anthropogenic vs. natural OA?

We do not explicitly state what the reviewer mentions here; separating the anthropogenic from the natural one is essential for environmental policy and climate mitigation. This is now also clarified in the abstract. Instead, we emphasize that different anthropogenic/natural SOA formation parameterizations can result in the differences in OA radiative forcing calculated by models and this has large implications for climate mitigation. The state-of-the-art climate and Earth system models that we currently use for policy decisions (e.g. the models that participate in CMIP5 and the IPCC reports) are mostly much simpler than the ones used here when it comes to their OA representation, with probably only one or very few exceptions. What is urgently needed is to understand the nature and variability of anthropogenic/natural/mixed OA sources, which will result in different formation rates, distributions, and probably OA properties, as climate and human driven emissions are changing from the preindustrial era to the future.

3. Add one sentence to the introduction (section 1) giving a brief overview of the AEROCOM study design (such as year targeted, specific emissions or user choice, etc)

We added a sentence at the end of section 1.5: "The target year of simulations was selected to be 2006, with a free choice for each modeling group on the meteorological conditions and emission inventories to be used."

4. Page 6033, line 19, plus/minus needs formatting

The formatting appears correct in the submitted manuscript. Maybe the referee got confused with the different plus and minus values of the indirect effect.

5. Page 6034, line 1-2: Expand the definition of SOA. What about aqueous reactions? Heterogeneous reactions? There are more SOA sources than condensation of semivolatile vapors.

We added one additional sentence there: "In addition, multiphase and heterogeneous processes can also contribute to SOA formation."

6. Page 6036, line 9: consider rephrasing the comment that anthropogenic VOC sources of SOA are "mostly neglected by global models" since page 6046 line 26 indicates a few models consider them.

"Mostly" changed to "frequently" - See comment #9 of reviewer 1.

7. Page 6038, check year in text and reference of Hodzic et al. 2014

This was a strange typo introduced during production that we missed while reviewing the proofs. The correct "in 2014" should had been "in preparation".

8. Sections 1.1-1.5 are AMS and current-model centric. What about a slightly more general view highlighting some emerging issues (IEPOX uptake?) that the intercomparison may be able to provide evidence/for against? Can provide support for/against?

IEPOX formation is included in one model (IMPACT), but due to its coarse spatial resolution it is difficult to assess whether it indeed helps improving the model's skill. In addition, it is beyond the scope of the present manuscript to compare individual OA species, since we are only using either total OC or OA measurements. Section 4.3.3 however identifies strengths and weaknesses of the current modeling approaches, and throughout the manuscript we propose ways to improve the models. Newly identified OA sources, which do not exclude the role of IEPOX on SOA, are suggested as improvements for the future. In any case, inevitably we have to discuss our results within the framework of the tools we use (current models) and the measurements we compare against (OC and AMS).

9. Page 6044, line 1: reword "contribute by up to"

Changed to "can contribute up to".

10. Can section 2.2 more closely parallel table 2? There are five classes in section 2.2 and 2 columns in the table.

We renamed the last column of the table to "other sources/comments", so now there are three columns. The five emissions classes the reviewer refers to are fuel, biomass burning, pseudo-primary SOA, marine, other. The three columns capture fuel, biomass burning, and other (which includes marine; very few models have this source which does not justify the presence of a dedicated column). In the manuscript we consider the pseudo-primary SOA emissions as a secondary source of OA, so it does not belong to this table. In order to keep the table as close to the text as possible, we moved this class to the end of the section.

11. Page 6052, what year of the datasets are used?

See comment #18 of reviewer 1.

12. Page 6053, line 29: See Budisulistiorini et al. AMTD for possible ACSM biases. The RIE for organics is not well characterized which could result in unrealistic OM levels (OM/OC ratios above 4).

We thank the reviewer for pointing out this reference. AMS and the newer ACSM monitoring instruments both report inorganic and organic mass concentrations. References within Budisulistiorini et al. (2014) (e.g. Ng et al., 2011) show that that AMS and ACSM systems compare well with each other. Reported AMS and ACSM OM values are based on calibrations using a range of organic types (specific oxygenated and hydrocarbon molecules) and the comparisons presented in Budisulistiorini et al. (2014) with regard to the OM/OC ratios bring into question the accuracy of the OM calibration used by AMS systems as well as the OC value reported by the Sunset analyzer used in that study.

We added the following statement in the manuscript: "It is important to note that the OM values provided by the AMS type instruments have uncertainties (30%) inherent with quantifying the detection efficiency for the wide range of organic molecules that make up complex SOA material (Canagaratna et al., 2007; Middlebrook et al., 2012). Care should be taken when using AMS type OM data in models that estimate organic aerosol content".

13. Page 6055: Line 25-26, awkward sentence

Changed to "Also note that contrary to biomass burning, anthropogenic tPOA sources have no seasonality in their emission inventories."

14. Page 6056, line 20: why was SOA production scaled in some models? To match observations?

This is mentioned in Table 4 of the submitted manuscript for CAM5-MAM3: "Yields listed include a 1.5X increase to reduce anthropogenic aerosol indirect forcing", and in page 6045, lines 6-7, for CAM4-Oslo: "scaled up to 37.5 Tg a^{-1}, based on (Hoyle et al., 2007)".

15. Page 6062: Line 17, does disagreement with the mean necessarily imply the dry deposition is too low or is there another reason?

Both the absolute dry deposition flux (discussed in page 6061) and the effective deposition flux (page 6062) appear very low for a small number of models. As already mentioned in page 6061: "Also note that we have not assessed this feature of the models against observations, so we do not know which models are closer to observations", we can only hypothesize that the few models that deviate from the mean are underestimating dry deposition. Given the large number of models with very diverse aerosol representations that reasonably agree with each other, this appears to be a safe assumption.

16. Page 6068, line 3, remove OC from sentence since units are for OA.

We have added units both for OC and OA, since Figure 14 of the submitted manuscript has both.

17. Page 6078, line 20: Could increased SOA during the summer result from an enhanced anthropogenic source rather than biogenic SOA?

Most models do not account for any seasonal cycle for the anthropogenic emissions. However, oxidant levels are higher during summer and thus both anthropogenic and biogenic VOC oxidation is faster then. Furthermore, SOA become more volatile during summer because of the temperature dependence of their vapor pressure. However, we cannot exclude the probability of enhanced anthropogenic SOA precursor sources which are not currently parameterized in most global models. Should this be the case, an enhanced anthropogenic SOA source is also possible.

18. Page 6079, line 10-11: Can you comment on whether or not treating POA as SVOCs (thus making them temperature sensitive) could be a factor for higher winter OA levels? Figure 25 (VBS) indicates it doesn't play a role.

We added the following statement: "The missing processes include the intermediate volatility organic compounds, which are expected to condense more during winter, and the assumption of semi-volatile POA, which will favor POA evaporation during summer. The combination of these two processes will lead to higher OA concentrations during winter and lower during summer when compared with the current OA parameterizations. This is expected to vary spatially, depending on the availability of these species and that of preexisting aerosols, and assuming no seasonality in their sources". It is difficult to extract a safe conclusion from the GISS-CMU-VBS model, due to its very coarse grid that greatly dilutes urban pollution.

19. Is there a model development priority list that could be added to the conclusions? Either for processes that are significant but not widely implemented in models or that lead to large differences between models?

We added the following statement at the end of the manuscript: "Important processes currently not included in many models that need to receive high priority from modeling groups include the semi-volatile nature of OA, the temperature-dependent OA formation and aging, which affects their volatility, and an improved parameterization of the OA/OC ratio."

20. Table 1: Can you include a version number or release/freeze date for each model since it is unclear if later improvements to the models were included but not referenced

Not all models have version numbers, since they are constantly being improved. Their version is almost identical among all AeroCom phase II papers, many of which are already published (e.g. Jiao et al., 2014; Kim et al., 2014; Mann et al., 2014; Myhre et al., 2013; Samset et al., 2013). We also already include the exact AeroCom phase II version in Table 1 of the submitted manuscript, in case one wants to refer to the exact input used.

21. Table 2, add another column for sources other than fossil fuel, biofuel, and biomass burning so that additional sources are not buried in the comments column

See response to comment #10.

22. Table 4, page 6121: both the term terpenes and monoterpenes are used. If the term terpene is used only to refer to monoterpenes, use that term instead.

There are models that include terpene sources that use more than monoterpenes (e.g. sesquiterpenes), so both terms are valid.

23. Figure 4, can you distinguish missing data from zero values? I assume all zeros in Figure 4b are missing data?

The reviewer is correct, all missing columns are missing data, not zero values. We added a parenthesis in the legend of Figure 3 of the submitted version (which applies to Figure 5 as well): "Note that not all models have submitted budget data (thus their corresponding columns are not shown)…"

24. Figure 2b, Is the GISS-CMU-VBS POA emission rate before or after partitioning? Doesn't most of the POA evaporate?

We modified the legend of the Figure by adding an explanation for this: "POA emissions included in models; in the case of GISS-CMU-VBS it is before POA evaporation". Indeed most of POA evaporates, as shown and discussed in multiple occasions and figures later.

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