We thank the reviewer for his/her comments on our paper. To guide the review process we have copied the reviewer comments *in black italics*. Our responses are in regular blue font. We have responded to all the referee comments and made alterations to our paper (existing manuscript text **in bold text,** new/altered parts additionally **highlighted in yellow**).

Reviewer #2

This is a well written, appropriately titled manuscript describing modeling efforts to quantify the effect that deposition of gas-phase semi-volatile organic compounds has on predictions of secondary organic aerosol. While it now appears from the literature that deposition/uptake of such compounds in smog chambers affects the ability of associated experiments to predict yields, only little effort has been aimed at quantifying this effect in the ambient atmosphere. Therefore, this is an appropriate topic for ACP. What work has been done previously is cited and provides the basis for this work, which provides a quantitative conclusion. The paper uses appropriate methodology and cites previous work liberally; methodologies (with the exceptions noted below) appear sound. The abstract is complete and accurate. That being said, there are a few issues that need to be addressed prior to my being able to recommend publication.

R2.1 Significant comments: Much of this work is predicated on the work of Hodzic et al. (2014a), which was not made available for review. Not enough information about this work is given to allow for an evaluation of whether this approach should be carried into WRF-Chem/VBS. Please provide more information.

The work by Hodzic et al. (2014) has been published in GRL and is available online. It contains detailed description of how the parametrization was derived and how it can be used within the VBS bins.

R2.2 Given that all biogenic and all anthropogenic species are lumped together, how accurate is the unweighted average that is used for their characteristics? For example, the compounds that result from isoprene or terpene or sesquiterpene oxidation that might be in the same bin of volatility are going to be very different structurally – how does this impact their water solubility/H? How widely do the individual H* vary? This needs to be explained more systematically in this manuscript. This would also then impact the deposition velocities, of course.*

We agree with the reviewer that oxidation products of isoprene and terpenes that fall into the same VBS bin have a very different structure and functional groups. We have averaged

H* values among all oxidation products of biogenic (anthropogenic) precursors located in the same bin due to computational constraints. This simplification is however not expected to significantly affect the accuracy of our results as for a given VBS bin, H* values vary by less than an order of magnitude e.g. for C*=10 ug m^3, H* varies from 3.5 x 10^8 for isoprene to 1.4 x 10^9 for monoterpenes in the low-NO_x case (Table 1 in Hodzic et al., 2014), and we used the average value of 7.0 x 10^8 in our study. In addition, as shown in the sensitivity studies, dry deposition is not very sensitive to small changes in H* values for H* values greater than 10^6 M atm^{-1}. Given that Hodzic et al. (2014) is published now we refer the reader and the reviewer to that publication for details.

R2.3 In the LOWVOL case (page 13739, line 4), what is the basis for using the C = 1 bin? I recognize that it is "shielding" but why C*= 1? Why not C* = 10? Or C* = 0.1?*

The first reason for choosing the C*=1 ug/m^3 bin is that this is the bin with the lowest volatility of the "original" VBS scheme (as described in Lane et al., 2008; Ahmadov et al., 2012) - which was the basis of our developments.
Secondly, we intended to create a sensitivity run in which secondary organic aerosol that has aged for a long time is of such low volatility that it does not partition into the gas-phase any more. Aside from some direct emission, the C* = 1 ug/m^3 is mostly comprised of mass moved into this bin through aging of material with higher volatility. It could hence be considered the bin with the "oldest" SOA, and hence the material we want to "shield" from removal in this study.

R2.4 Page 13742, line 10. By considering only "traditional" SOA pathways, I believe that the authors may have missed a good opportunity. Yes, their point was to emphasize that deposition of SVOCs should be included in models – and they state that including other SOA pathways would simply shift things around (that is, that the end effect will be similar). However, I would argue that models for the non-traditional pathways also exist – so when you include both traditional and non-traditional pathways AND the deposition pathways, it would be the best evaluation yet of how well we can model SOA – and it would tell us how far off we are and potentially give insight into what really is missing.

We completely agree with the reviewer that a combination of new formation pathways plus our new understanding of how SOA is removed in the atmosphere would be the most up to date modeling system and would show our current skill in SOA prediction. This is indeed what we are working on, and we will include this in upcoming publications. There are some intricacies in how to include these "new" formation pathways (namely formation in the aqueous-phase, and the correct description of S/IVOC emissions as well as semi-volatile POA) together with the removal pathways in a consistent framework, which need to be

solved first before we can add these processes. This description will require a manuscript on itself, and is unfortunately out of scope for the work presented here.

Minor edits:
R2.5 Page 13734, line 14 – please define VBS here as this is the first use

changed

R2.6 Page 13741, line 13, I do not believe that the deposition cases has removed the local maximum. I believe it is still observed in Figure 4, left hand side.

The local maximum is removed, this becomes clearer with the revised figure in which the bars are colored differently.

R2.7 Page 13742, line 8. Strongly overestimating in summer? Strongly is probably too emphatic.

“strongly” has been removed.

R2.8 Page 13744, line 6, add “to” between “relative” and “simulations”; next line use “employ” rather than “employs”

fixed

R2.9 Page 13746, line 11 – change “and neither in the effect on” to “or in”

fixed

R2.10 Page 13747, line 1 – study should be studies

fixed

R2.11 Table 3, it could be my printed version, but it is difficult to see anything in bold?

fixed

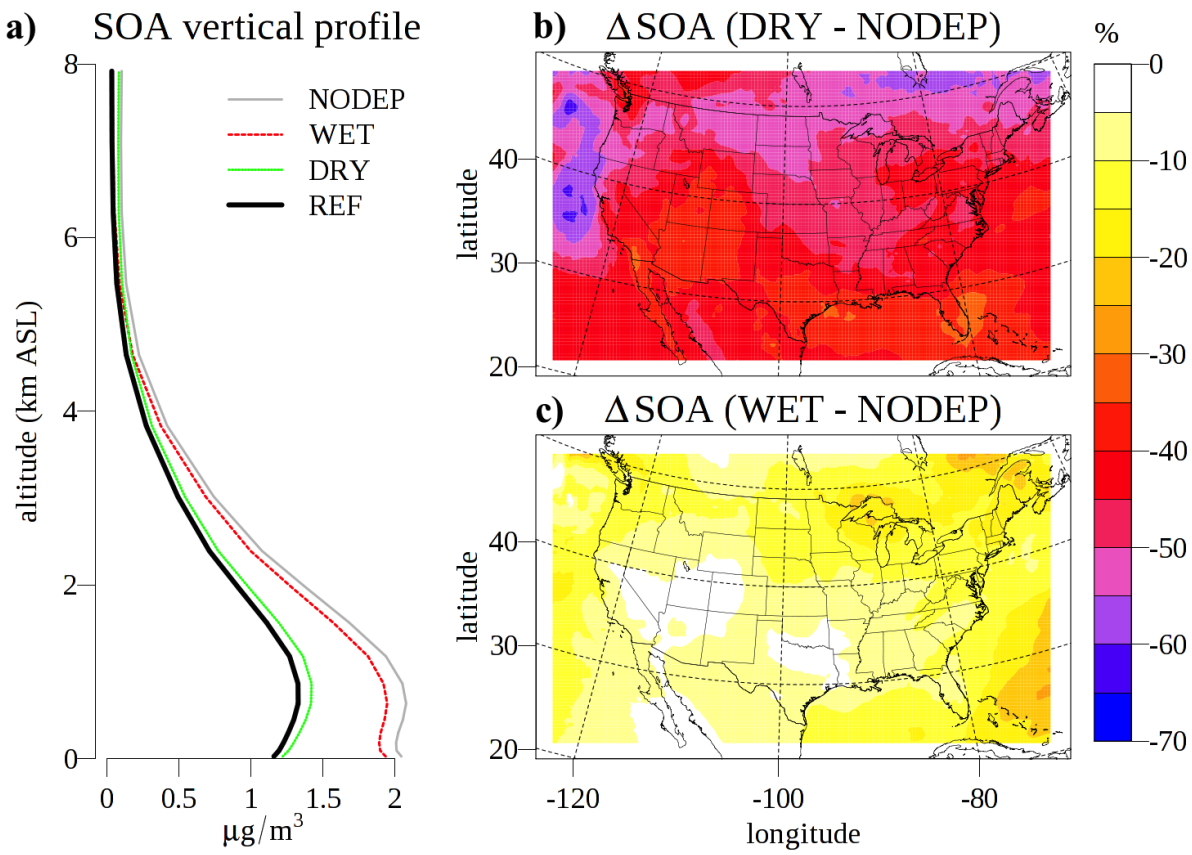
R2.12 Figure 2, update caption as there are no “left” and “right”

fixed

R2.13 Figure 3, small size makes it difficult to assess measurements versus model output in right hand side

The size of the figure will be increased in the published version.

R2.14 Figure 4, please use color in left hand side as the different lines are currently difficult to distinguish

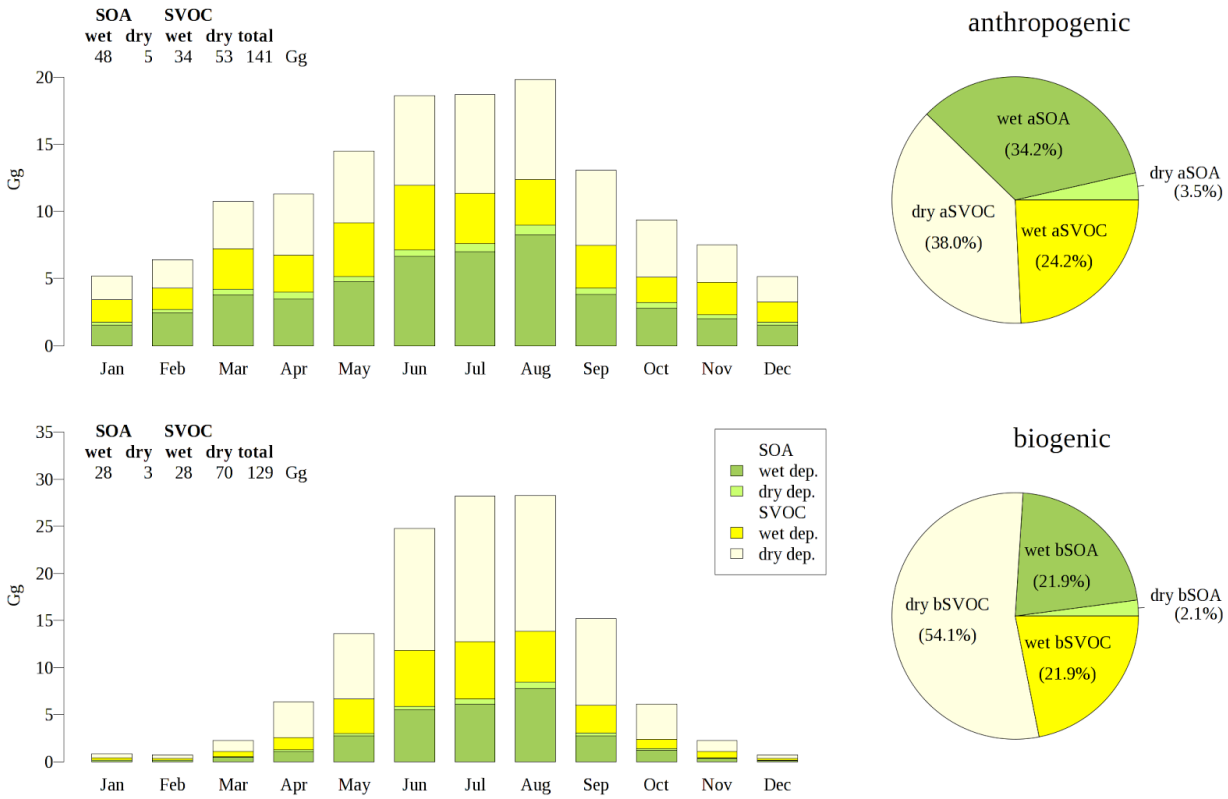


R2.15 Figure 5, same comment as Figure 3 for the top.

The size of the figure will be increased in the published version.

R2.16 Figure 6, dark font inside dark green pie slice is difficult to read.

The figure has been updated and the colors lightened:



R2.17 Should the figures associated with the Appendix be numbered differently?

The numbering has been changed to A1 and A2.