

Review of "SALSA2.0: The sectional aerosol module of the aerosol-chemistry-climate model ECHAM6.3.0-HAM2.3-MOZ1.0 by Harri Kokkola, Thomas Kuehn, Anton Laakso et al.,

At the end of May 2018, I reviewed the GMD-Discussions version of this manuscript, which describes the implementation of a sectional aerosol microphysics module (SALSA2.0) within the composition-climate model ECHAM-HAMMOZ (ECHAM6.3.0-HAM2.3-MOZ1.0), as an alternative to the existing modal aerosol microphysics scheme "M7".

My review identified the paper certainly within scope of GMD and found valuable the evaluation of aerosol optical properties, aerosol mass and particle size distribution simulated by ECHAM-HAMMOZ-SALSA, comparing to observations and parallel similar simulations with the existing aerosol scheme (ECHAM-HAMMOZ-M7).

However, I contended that some of the principal statements made in the Abstract were not supported by the results presented in the paper, and that more care needed to be taken in the statements interpreting potential reasons for differences between the sectional and modal schemes.

In particular, the authors claimed, and continue to claim in this revised version, that the size distribution comparisons shown in Figure 4, shown for locations where AOD difference is largest, are caused by different microphysical processing in the modal and sectional schemes, and that this is the reason why the two aerosol schemes predict different extinction/AOD in these regions.

My review pointed out that, as currently worded, readers of the paper might, not unreasonably, infer from this statement (and the associated results) that applying modal aerosol microphysics schemes in global models might have some large systematic error compared to sectional schemes which could be considered a benchmark test for the model.

This potential general inference therefore requires the authors to make sure their statements are fully supported by the results.

In my review of the GMD-D paper, I pointed out that the two cases shown in Figure 4 (which are in locations where primary aerosol emissions are very high, of predominantly biomass burning emissions and of anthropogenic emissions), the size distribution of the black carbon (the black bars in the stacked bar chart) are very different between the SALSA and M7 simulations at both locations.

And that, to me, this clearly suggested that there was likely a systematic difference in the size at which primary carbonaceous aerosol particles are emitted, which could explain the reason for the difference. For example at the China site, the M7 run has about half of the BC in particles larger than 200nm, whereas for the SALSA run this is only about 10%. The same is true for the Russia site, indicating that there seems to be a systematic difference between the two schemes in the sizes assumed for primary emitted carbonaceous particles.

The authors replied to this as follows:

**“It is true and a good point that differences in emission sizes could explain the differences over areas with high anthropogenic emissions. However, in our simulations this is not the case since for offline anthropogenic emissions, we use identical emission size distributions for SALSA and M7 (see Page 7 in the manuscript). There will be some difference resulting from remapping modal emissions to SALSA size classes, however the emission masses and numbers and their size distributions are identical for M7 and SALSA.”**

So the authors agree that if there were differences in the primary emitted size distributions (i.e. differences before the aerosol schemes transported the particles) then this could explain the differences seen.

But they explain that the assumptions for the emissions are identical, so unless there is a systematic difference introduced when mapping modal emissions to SALSA size classes, this is not the cause of the difference.

However, in their reply to my review, the authors go on to clarify that they are actually attributing the cause of the differences to differences in the way secondary organic aerosol is treated in the model – and that that is what they mean by “differences in microphysical processing”.

Specifically they explain:

**“The significance of microphysics calculation over the chosen areas can be demonstrated by two SALSA runs: one where condensing organics are treated either assuming them to be non-volatile and one where they are assumed to be semi-volatile, however so that the resulting secondary organic aerosol yield is approximately the same.”**

This information is extremely interesting in that it clearly demonstrates that the way models represent the size-resolved partitioning of organic aerosol has a major impact on simulated aerosol optical properties.

However, again the authors are making an incorrect statement in contending that this demonstrates a difference arising from the modal or sectional aerosol microphysics (the aerosol dynamics approach).

Certainly this shows the major significance/influence from the way gas to particle partitioning is applied in the model, but it does not give any information about any apparent difference between modal and sectional aerosol microphysics schemes.

In fact this statement then strengthens the points I made in my original review, that the differences between the ECHAM-HAM and ECHAM-SALSA are not caused by differences between the modal and sectional schemes, but by differences in the treatment of organic aerosol.

The additional Figure 1 added in the reply to reviewers is very interesting as it shows that treating the semi-volatile nature of the organic aerosol

has a major control on simulated aerosol optical properties.

I agree completely with the authors' statement in the text just above that Figure 1 when they explain:

**From the figure, we can see that although everything else except for the microphysical treatment of OA is exactly the same, there is a large difference in simulate AOD's**

And I also agree that this also highlights the importance of aerosol microphysical processes.

But in the revised manuscript, the Abstract still states:

**The largest differences between SALSA2.0 and M7 are evident over regions where the aerosol size distribution is heavily modified by the microphysical processing of aerosol particles**

I am still not content to approve this manuscript until the text clearly explains this is not a difference between the modal and sectional aerosol schemes, but a difference in the treatment of organic aerosol.

As currently worded, there is still a danger that a time-pressed reader, who may (not unreasonably) have to speed-read the manuscript for its main points, may incorrectly infer that the "difference in microphysical processing" between the ECHAM-SALSA and ECHAM-M7 simulations is due to a difference arising from the parameterized (modal) aerosol dynamics in M7 compared to the sectional aerosol dynamics applied in SALSA.

In their reply to reviewers the authors have shown that although it is correct to say that there is a difference in microphysical processing between the ECHAM-SALSA and ECHAM-M7 runs, their reply clarifies that the difference in simulated aerosol properties arise from the different treatments of organic aerosol between the two aerosol schemes, not from their different aerosol dynamics schemes.

To protect against this incorrect inference, it may simply be a case of revising the statements that attribute the cause of the differences to specifically identify the reason for the different simulated aerosol properties as the gas-particle partitioning of the organic aerosol (rather than the modal/sectional aerosol dynamics).

I request that the authors go through the manuscript and make sure the text cannot be misinterpreted by the time-pressed reader.

I can confirm that I am fully prepared to review the manuscript again once the text has been revised to clarify the attribution of the differences in simulated aerosol properties between the ECHAM-SALSA and ECHAM-M7 runs.

Finally, please note that whereas I am again classifying this required change as a major revision in my reviewers report, I am fully supportive of the manuscript being published in GMD once the required changes have been made. Furthermore I am confident the authors can make the changes

fairly quickly so am confident this additional revision will not introduce a significant delay in the review process.