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Re: Responses to comments on "*Aerosol specification in single-column CAM5"* by B. Lebassi-Habtezion and P. Caldwell

Dear Dr. Neale:

We hope that our response letter and revised manuscript have answered all of the points raised by the two anonymous referees and yourself, which we believe has resulted in an improved paper.

Please contact me if you require additional information.

Thank you for your continued interest in this paper.

Sincerely yours,

Bereket Habtezion

B. Responses to comments to referee #1

Anonymous Referee #1 (Comments to Authors):

1.GENERAL COMMENTS

This study takes a close look at the role of aerosol in single column model experiments with CAM5. By default, the model initializes the aerosol fields to zero, which is interpreted as being incorrect. Three alternatives are explored: specifying climatological aerosol, specifying observed aerosol, and fixing the droplet and ice numbers. Several typical SCM cases are used: marine stratocumulus (DYCOMS), Arctic stratus (MPACE), shallow convection (RICO), and deep convection (ARMSGP). Several interesting points emerge through the study. The microphysics desiccates the atmosphere and has a very strong impact on the LWP. This effect is deleterious in mixed phase clouds and is controlled by the Myers formulation for ice nucleation. There is a physical inconsistency associated with the microphysics removing so much water because cloud fraction is determined before this process, so CAM5 does not completely get rid of the old "empty cloud" problem previously reported for CAM3/4. Convective cloud regimes are relatively insensitive to aerosol effects because of the simpler microphysics in the convection schemes, but near cloud-base activation still dominates the determination of droplet number so aerosol matters there while detrainment dominates the determination of droplet number at higher levels. Although I appreciate the general approach of the study, I believe there are several major issues that should be addressed before it is suitable for publication. One is the framing of the problem.

The whole study seems to hinge on the initialization of aerosol to zero in the default model being wrong. It is not a priori wrong to take this approach, and one could probably argue that it is as valid as any of the alternative approaches presented in this paper. The results show that there are probably ways to make the SCM better capture the observed cloud properties, perhaps supporting adoption of another aerosol specification. On the other hand, which of the approaches best matches the results from the full 3D model? The answer is not clear in this study, but probably should be considered as central in defining what the SCM should do. The first sentences of both the abstract and introduction indicate that SCMs are useful for model improvements, and therefore must (before all else) be representative of the full 3D model. Whether any of the aerosol specifications discussed here come closer to the full model is unclear.

We thank the reviewer for their thoughtful comments. We agree that the point of single column modeling is to improve the GCM and that our previous draft did not make a clear case for how our proposed fixes contributed to that goal. We now mention in the text that using aerosol specified from previous GCM runs (the PrescAero method) is the best way to match the *typical* behavior of the 3d model. In order to identify the source of *problems* in the GCM, however, it is best to perform sensitivity studies where quantities typically predicted by the model are prescribed instead. Idealized experiments of this sort are also extremely useful for optimizing a parameterization of interest without while avoiding compensating errors from other schemes. It is for this purpose that we propose the FixHydro and ObsAero methods.

We strongly disagree that initializing aerosol to zero is as valid as specifying aerosol or droplet and crystal number. Clouds respond very strongly to cloud number concentrations, so using a number concentration which is unrealistic (compared to observations and/or typical model values) will produce very unrealistic and unuseful output. In this sense using zero aerosol is like initializing temperature to zero Kelvin - the planet you are simulating is not Earth and the SCM results will bear little resemblance to a column from the 3d model. You are asking the physical parameterizations to act far outside of the conditions they were designed for and any results you get are unlikely to be relevant for guiding GCM development.

All of this was poorly explained in the previous version of the paper - we have revised the paper to make these points more clearly.

A second major issue is that there is a bit of a false dichotomy being presented in the comparison of the default model and the alternatives, and it comes down to the difference between the way the default model is initialized versus how aerosol is specified throughout the integration in the alternatives. The default model initializes the aerosol to zero and is subsequently driven by surface emissions, so the aerosol field (if I understand correctly) remains prognostic, but is erroneous because the only source is at the surface and vertical transport is the only way to populate the upper levels. In the alternative approaches, the initialization of the aerosol is likely to be inconsequent for the result. Instead it is the specification of aerosol fields through the column through the integration that matters. Connecting to my first point, it seems like the prognostic aerosol approach is most consistent with the 3D model, but the SCM would then require aerosol as part of the large-scale forcing, and how to construct an appropriate aerosol forcing may be ambiguous. This distinction between the initialization problem and the specification problem may seem nit-picky, but I think it is fundamental to the study, and the issues are confused throughout the text.

We agree that the default model is fundamentally different from our proposed fixes because it prognoses aerosol while the other fixes just specify aerosol or cloud number densities. This means that simulations using our proposed fixes have less opportunity to go wrong. As noted above, constraints of this type are acceptable or even preferable when the goal is to optimize some aspect of the GCM unrelated to aerosol. It is true that aerosol activation can't be studied when cloud number densities are prescribed, and that's why we developed the PrescAero and ObsAero methods. It is also true that none of our methods can be used to study cloud/aerosol interaction. We have tried to clarify the tradeoffs between our proposed approaches to aerosol treatment in the SCM in the revised paper.

Third, the text presents the results of the default and alternatives, but never makes any recommendation for what would be the best method. From my vantage point, this lack of a clear recommendation is rooted in the previous points regarding how to think about and frame the problem of aerosol in SCMs.

We agree that framing for our aerosol treatments was lacking. Hopefully with that in place it is clear that there is no single best approach. If one is interested in the impact of their changes to cloud physics, using observed droplet/crystal concentrations is optimal (if they are available). If one is interested in testing changes to the aerosol activation scheme, using observed aerosol is probably best. And if one wants to know how biases in modeled aerosol concentrations impact cloud and thermodynamic behavior, they should compare specAero against obsAero runs. If one is interested in interactions between cloud and aerosol, the 3d model is needed (or better initialization and

specification of horizontal advective tendencies is needed). We have included this explanation in the new revision.

Finally, and related to the others, the text needs a substantial editing for grammatical errors, clarity, and concision.

We agree that the previous draft was sloppy and apologize for that. We've tried to clean up the new version.

SPECIFIC COMMENTS

1. The abstract is overly long and does not highlight the main results very well.

We agree and have completely rewritten the abstract to address your concerns.

2. The first paragraph (pg 3-4) is a little hard to parse. The points get lost in all the call outs to the SCM studies. I think the paragraph could be cleaned up substantially by focusing on the themes that have emerged from these studies, rather than the specific conclusions from each one. It seems unnecessary to establish these results except to introduce the cases to be used later.

Good point. We have moved discussion of previous GCSS/GASS results into the sections devoted to each case study and instead highlight the importance of these case studies and of idealizations as a means to improve model behavior.

3. pg 4, line 19-21: What does it mean for aerosol to be handled "appropriately" in an SCM. This is not established, but would be a useful discussion for this paper. It should also be explained (here or in Section 2) what the default model actually does (initialize to zero and then use surface emissions in MAM).

We have tried to explain this better.

4. The use of the word "fixes" for the alternative aerosol specifications seems informal on the one hand and misleading on the other. If these "fixes" actually fix the issue, then the study should determine what the default model behavior should be and make a recommendation. As mentioned above, there is also this issue about the difference between incorrect specification of the aerosol forcing (in the sense of the specified aerosol transport) versus initialization and actual physics. This comes back on page 5, lines 22-24: "As mentioned previously, this prognostic aerosol model in SCAM5 mode initializes the mass-mixing ratio of the different aerosol species to zero. Hence we test other fixes to solve this problem as described below." This statement must be interpreted as one of an initialization problem, but none of the "fixes" is focused on initialization (and in fact, if the aerosol are still initialized to zero, it probably would not make any difference after the first time step).

We see the 'problem' to be that aerosol and number concentration are so low that SCM runs are simulating an environment that would never happen in CAM or in the real world. In this sense our 'fixes' really are fixes in the sense that they solve the problem. We have tried to clarify this usage in the last sentence of the introduction and have replaced 'fixes' with 'solutions' to be less colloquial.

5. pg 7, line 2 overstates the breadth of the cases. These cases are appropriate for the study, but do not cover the "full range of cloud types."

Agreed. We have changed this text to read 'a range of climatologically-important cloud regimes'

6. Section 2 could probably be streamlined by constructing a table with all the forcings and then the text could focus on the big picture of each case and any caveats (which are already there, e.g., the change in w for the MPACE case).

We thank the referee for insightful comment. We have added a new table (table 1) and edited the text accordingly.

7. On pg 12, line 24, the 3D model result is referenced and is very different from the SCM result. What does this mean for interpreting the SCM as a cheap version of the full model? Could the difference in this case be due to sampling? Specifically, is the diurnal cycle in the long 3D run biasing the mean profile compared to the DYCOMS result?

This is a good point. Yes, including daylight hours is undoubtedly causing the BL depth to decrease in the GCM (which we now note in the text). The fact that the SCM runs are a short case study forced by observations and the GCM is a long-term climatology is undoubtedly also playing a role. As noted above, SCM case studies are typically used for testing how the model would respond if it was given realistic forcings rather than trying to replicate the bias of the full model.

8. Pg 13, line 10 blames the initialization of aerosol, but this is after hours of simulation. Is the problem that there isn't enough vertical transport of aerosol from the surface emissions?

This is an interesting point. Schubert et al (1979) identify the turbulent mixing timescale of the stratocumulus-topped boundary layer as being ~1 day. Since this is longer than our DYCOMS and MPACE case studies, it is reasonable to blame initialization for some underprediction of aerosol. We see this empirically as well - it tends to take a couple of days for aerosol to equilibrate.

9. Pg 15: "empty clouds" have been pointed out in previous versions of CAM. Are these empty clouds conceptually similar, or is the different microphysics responsible for a new kind of empty cloud error?

Good catch. Yes, these 'empty clouds' are similar to those that plagued CAM3. The occurrence of empty clouds in CAM5 have been greatly reduced by adding checks in the macrophysics scheme (=cloud fraction + condensation/evaporation) which zero out cloud fraction if condensate is zero (or vice versa). Thus we were surprised to see empty clouds in this study. As explained in the text, these clouds are emptied by microphysics acting after all the macrophysical checks have been performed. We've included a discussion of this in the most recent draft.

10. Pg 15-16: The three paragraphs ending this section should be combined and reduced. The third paragraph contains most of the useful information, so the other two should be turned into one or two supporting sentences in the third.

Agreed. We ended up totally rewriting this section to address the reviewer's concern. .

11. In the RICO case, how can the surface fluxes be so far off if the surface temperature and wind are prescribed?

This is a very good point. To a reasonable level of approximation, surface fluxes depend on SST, wind speed, air temperature, and near-surface humidity, As the reviewer notes, wind speed and SST are fixed in these simulations. The source of surface flux error seems to be a drift towards colder and dryer conditions in the atmosphere. We could have worked harder to improve these simulations (e.g. by nudging the free troposphere or calculating winds from geostrophic values) but our main point with RICO is that aerosol doesn't matter (so model skill is independent of aerosol treatment and thus outside the direct scope of the paper).

12. I was surprised there was no discussion of precipitation in the RICO case. The SCM results must be precipitating, right?

Response: Yes the RICO case is a precipitating case. However, since the case is convective we didn't see much precipitation difference for the different aerosol specification cases.

TECHNICAL CORRECTIONS

pg 3: First sentence of the paper is incomplete: insert "for" between tool and efficient. Also, it is the Community Atmosphere Model, not "Atmospheric." (http://www.cesm.ucar.edu/models/cesm1.2/cam/)

Fixed, thanks.

pg 3, sentence starting at line 13 is grammatically wrong. Perhaps it should just be "In another SCM intercomparison, simulations ... "

Completely rewrote this section.

pg 3, the next sentence (line 16) is also wrong. Perhaps "The SCM intercomparison of ... "

Completely rewrote this section.

pg 4, line 17: There is a problem with the tense. Maybe it should read: "As a result, developing aerosol parameterizations has become a high priority in the climate modeling community."

Corrected as suggested

pg 4, line 18: This sentence reads awkwardly. First because it sounds like it is in the wrong tense ("had"), and second because the use of "break-through" is a bit aggrandizing of the aerosol model. It is a major development and adds capability, but for most applications it isn't a game-changer.

Deleted this sentence

pg 4, line 20: The SCM is referred to as CAM5-SCM here, but as SCAM5 later. Choose one and be consistent throughout.

Corrected as suggested and consistency check made throughout text

pg 5, line 14: "Brethorton" -> Bretherton

Corrected as suggested

pg 5, line 25 versus pg 6 line 3, and also throughout the paper there is a lot of switching between tenses. It's distracting to the (or at least this) reader.

Agreed. We have tried to be more consistent.

pg 5, line 26: "This case is the setup in default" is confusing, perhaps change to "This case is identical to the default"

Agreed. This whole section was confusing and we have rewritten it to (hopefully) improve clarity.

pg 7, line 8 AND EVERY SUB-SECTION TITLE: the letter denoting the subsection is repeated (e.g., a. a. DYCOMS RF02 case)

This issue seems to be related to GMDD's automatic conversion of word documents. We have deleted our lettering in hopes of fixing this problem.

pg 8, line 21: delete "values"

We completely rewrote this section to improve clarity.

pg 10, line 21: "The ARM95 included because" should be "The ARM95 case is included because" (?)

We completely rewrote this section for improved clarity.

pg 11, lines 16-19: grammar fixes: "We also include cloud base, zb, which is computed by interpolating to the level at which cloud fraction first exceeds 0.5 and cloud-top height, zi, which is computed by interpolating to the highest level at which the total water mixing ratio drops below 8gkg-1." -I think that zi is probably the lowest level at which q is below 8 g/kg, right?

Actually we do use interpolation. First we identify the level just below the cldfrac=0.5 or qt=8 g/kg mark and then we do linear interpolation between this level and the level just above to get an interpolated height rather than a layer height. This approach avoids noise due to snapping to model levels and reduces sensitivity to the grid specification. We have tried to explain this better.

pg 14, line 10: "not" -> "no"

Corrected

pg 14, lines 22-24: This sentence reads very poorly, perhaps change to, "In PrescAero and ObsAero, the microphysics removes all the liquid water, but this feedback is removed in the FixHydro case by specifying constant droplet and ice numbers."

Agreed. We have rewritten this section to make more sense.

pg 14, line 28: "consistes" -> consists

Oops, thanks.

pg 15, line 4: "the 10 years October 2004" What is this supposed to mean?

We have corrected this section to be more clear.

pg 16, line 22: the first "LHF" should be "SHF"

Corrected

pg 16, line 24: "compared to LES, (0.19) and (19 g m2), respectively." -> "compared to LES (0.19 and 19 g m-2, respectively)."

We've removed these numbers from the text since they can be easily read from the table.

pg 17, line 4: has -> have

Corrected

pg 17, line 5: "was" probably is not correct tense

Corrected

pg 17, lines 27-28: incomplete sentence (maybe need "is" between overestimation and due?)

Corrected

pg 18, line 16: "every other day" what is meant by this?

Corrected

pg 18, line 21: "Generally, SCAM over estimated LWP at all periods." -> "Generally, SCAM5 overestimated LWP during all periods." (If the past tense is to be used.)

Corrected

pg 19, line 14-15: "formed when you have higher aerosol burden." -> "formed with a higher aerosol burden."

Rewrote this section.

Figure 1: the global run isn't labeled.

Good point. Fixed.

Figure 4 caption: " 3-D CAM values are 10 years July average global CAM extracted at the location of MPACE-B." -> "They cyan line shows the July average from a 10-year integration of the full 3D CAM at the MPACE-B location."

Thanks. Fixed

Figure 5: add legend for the observations

Great idea, done.

Figure 7: "No Aero" is the wrong color in the legend.

This figure seems irrelevant since N_d has no impact on the simulations, so we removed it entirely.

C. Responses to comments from referee #2

Review on "Aerosol specification in single-column CAM5" by B. Lebassi-Habtezion and P. Caldwell

Major comments:

Single-column model (SCM) is an important tool for the climate model developments. This study implements different approaches of aerosol specification for the SCM of the NCAR/DOE CAM5, and examines effects on SCM simulations under several cloud scenarios.

This study is a useful contribution to the global climate model (GCM) community regarding the importance of aerosols for simulation of clouds when GCMs have been implementing the aerosol effects on clouds.

I feel this manuscript in current version was prepared rash and there are many places through the text needing to improve the accuracy of wording. Some important references relevant to this study are missing.

I recommend the publication of this manuscript after my comments are sufficiently addressed.

Other comments:

1.P7694. Line 25-28. The current statement is a bit confusing and please change the wording here "This finding suggests…". Since ARM95 is a convective case, and CAM5 does not treat the aerosol activation and droplet nucleation for this type of clouds, the underestimation of predicted droplet concentrations suggests that CAM5 needs to include the sophisticated cloud microphysics and aerosol effects for this type of clouds.

We have rewritten this section to improve clarity. The idea that lack of convective aerosol treatment is the source of low aerosol in the SGP region is an interesting idea that should be tested. Our intuition is that convective microphysics would further *reduce* aerosol number due to rain out and combination of aerosol particles when droplets evaporate. Note that convective aerosol *transport* already exists in (in a crude form) in CAM5 so lofting should already be happening.

2.P7696. Line 19. Citation of Abdul-Razzak and Ghan, 2000 is not correct one. Cite Ghan et al. (2012) and put behind Liu et al. (2012).

Oops, thanks. Fixed.

3.P7697. Line 17. Remove "simplified". "Easter et al. 2014" is not a correct one, replaced by "Liu et al., 2012)".

Sorry, fixed.

4.P7702. Line 26. Please give a reference for the "State University of New York (SUNY) objective analysis method".

Done

5.P7703. Line 9. At which vertical level is Nd/Ni in Table 1?

Great question. These quantities are the average over the in-cloud portions of all cloudy levels of the column. We have tried to clarify this in the table captions.

6.P7704. Line 9. "4.45 kgkg-1 s-1" is 8 orders of magnitude higher than other numbers here. Is this a correct value?

Oops, corrected.

7.P7706. Line 10. Change "not" to "no".

Done.

8.P7706. Lines 22-24. This issue is not new and has been identified by earlier studies, e.g., Liu et al. 2011. Please cite this study.

It is true that Liu et al (2011) note low LWP caused by microphysics and suggest that the Meyers nucleation scheme is a cause - we should have (and now do) cite them for this. We find no mention of total depletion in their paper however, so don't cite them for this.

9.P7707. Line 5 and other places. The CAM5 model time step is 30 min not 20 min.

Good point, fixed.

10.P7707. Lines 22-28. Earlier studies have found the overestimation of ice number from Meyers et al. parameterization and also tested several new parameterizations. These studies (e.g., Liu et al. 2011, Xie et al. 2013; English et al. 2014) should be mentioned and discussed.

True. References added.

11.P7708. Line 17. "The Default, PrescAero, and ObsAero cases showed an average Nd value of 51 cm-3". However, it is not 51 cm-3 in Table 3. Please clarify.

This value was left over from an earlier round of model runs. We have deleted these numbers from the body of the text in the new draft because it was redundant - the reader can easily extract such information directly from the tables.

12.P7708. Line 24. Is there a reason why "All the models simulated CLC (0.18), and LWP (19.4 gm-2) very well as compared to LES, (0.19) and (19 gm-2), respectively".

Since the vertically-resolved cloud fraction (Fig. 8) is so different between the LES and CAM5-SCM, we have to conclude that good agreement in cloud cover is (unfortunately) coincidental. This behavior is, however, canonical for the UW ShCu scheme (as noted in Park and Bretherton, 2009).

13.P7709. Line 13. Why does the ObsAero give the lowest aerosol burdens compared to Default case?

This is a good question. There's no rule that Default needs to be the lowest - it builds up aerosol from surface emissions over the 24 hr RICO simulation period so there's no reason it couldn't reach higher N_d levels than ObsAero. What is strange is that N_d is only 14 cm^{-3} in ObsAero even though it uses aerosol specifications which produced reasonable droplet concentrations in LES. We have checked that we implemented the suggested aerosol numbers from VZ11 correctly but otherwise have no explanation.

14.P7709. Lines 23-25. Mass flux figure is shown in Fig.8b not 8a. How do you know "condensate is overpredicted"? Condensate is shown in Fig.8a not in Fig.8b.

The figure numbers are corrected and statement reworded.

15.P7710. Line 27-28. Why the Nd from prescAero is so different from that from the 10-years prescribed climatology (Figure 11)?

The 3d model includes horizontal advection so can feel aerosol emitted in other regions. Also, as mentioned with regard to DYCOMS RF02, N_d can be (and undoubtedly is) created by convective detrainment in these simulations. As a result, N_d wouldn't be the same between prescribed-aerosol GCM and SCM runs unless their convection timeseries were similar (and there's no reason to expect that is the case).