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Interactive Comment

Interactive comment on "A Consistent Prescription of Stratospheric Aerosol for Both Radiation and Chemistry in the Community Earth System Model (CESM1)" *by* R. R. Neely III et al.

Anonymous Referee #2

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Overview

This manuscript describes methods used to implement volcanic stratospheric aerosol radiative forcing in different versions and flavors of NCAR climate models. This information has until now been difficult to find in the literature, and therefore the paper will be of definite utility to users of the NCAR models, or to readers looking to implement volcanic forcing in other models. I therefore find it to be appropriate in focus for publication in GMD.

General Comments

1. The authors make the claim that the new stratospheric aerosol scheme improves the

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Interactive Discussion

Discussion Paper

reproduction of global mean surface temperature anomalies after Pinatubo. With the new aerosol prescription, the global mean temperature anomaly is shown to be around 0.2°C, and they compare this to timeseries of observed temperatures. The observed global mean temperature of 1991 however contains a strong ENSO signal, which the model ensemble should not reproduce. Studies that have attempted to isolate the pure volcanic surface cooling signal from other sources of variability (including ENSO) result in estimates of maximum cooling ranging from 0.14°C (Canty et al., 2013), to around 0.4°C (Thompson et al., 2009). It is therefore clear that the model results from both aerosol prescriptions presented in the present work are within the uncertainty range of observation-based estimates of Pinatubo surface cooling. Secondly, it is not apparently clear that the difference in global mean temperature simulated using the two schemes is significant: the error bars in Figs 3 and 4 overlap considerably, implying that the difference might be purely due to natural variability. Therefore, more work would be needed to show that the temperature responses are different, and showing that one is more realistic than the other (in terms of global mean surface temperature) would be very difficult.

2. It appears the results shown in Section 6 apply only to CAM5 using the new aerosol prescription, with updates of both the shortwave and longwave parameterization. It is stated that only the shortwave prescription is modified for CAM4-chem and WACCM4. Assuming that the stratospheric aerosol heating is mostly due to long wave aerosol properties, this means that the most obvious improvement of the new prescription, namely the improvement in stratospheric temperature anomalies as shown in Fig 5, will not apply to the CAM4-chem and WACCM4 models. This fact and its repercussions (for example on surface temperatures and dynamical effects) should be discussed. It is also arguably misleading to state that this work has “unified” the treatment of stratospheric aerosols in the CESM family of models, as the authors claim.

3. In the results shown in Figures 3, 4, and 5, it is not clear what changes are due to changes in implementation, and what are due to changes in the input forcing files.

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Short of performing new simulations with the new implementation and old forcing files (or vice versa), one solution may be to show plots of the global mean AOD (and/or sulfate mass) and Reff for the old and new forcings.

4. Although "Chemistry" figures prominently in the article title, no results are shown in terms of SAD or its effect on model chemistry, and in fact the only advance described by the article is the model's ability to read SAD from the SAGE_4l forcing files.

5. For the reader not familiar with the zoology of NCAR models and model flavors, it can be difficult to follow the flow of the text. A summary table, listing the models involved and the main features of each model's aerosol scheme could greatly help, as could a simplification of the models referred to, if it is possible.

Specific comments

P10712, l4: the fact that "most" models produced a poor response to volcanic forcing is not entirely relevant to the need for a new prescription for one model.

P10712, l11: it's not clear what the connection between time varying mass loading and effective radius has to do with the recent improvements in aerosol databases. The GISS/Sato database has included effective radius for some time. It seems to me that the improvements in the parameterization, and use of new databases, are two separate things.

P10712, l16: Volcanic perturbations to stratospheric aerosol may be important to include in model simulations, but are not, in and of themselves, "essential".

P10712, l25: eruptions->simulations

P10713, l2-4: But the ensemble mean will not include the effect of the particular ENSO state in the real world of 1991/92, so exact agreement is not to be expected.

P10713, l6: The GISS/Sato forcing is provided in terms of AOD, not mass, so this sentence is incorrect.

P10713, l10: “interacts” suggests to me a two-way exchange. In this case, the models ingest the forcing data and the influence is in only one direction.

P10713, l27: CCSM4 was defined above.

P10714, l9: is it not kg/m**3?

P10714, l14: it would be nice to have a clearer, more prominent definition of effective radius.

P10714, l16: again, “interacts” seems a strange word choice.

P10717, l7: should this be a standard deviation of 1.25? Also, values for standard deviations should have units.

P10717, l16: describes->describe, I believe that grammatically, one refers usually to the authors, not to the paper.

P10717, l18: SAD defined already.

P10717, l19: change reference style.

P10717, l21: This section could be improved. I think the point here is that the aerosol mass is derived from the SAD and a set of assumptions about the aerosol size distribution. This needs to be clearer. Also, some details on the parameterization of Tabazadeh et al., 1997 would be very useful, as would a comparison of the derived aerosol mass with that of Amman used in the other simulations.

P10718, l7-9: Are the tags necessary? Also, the tags given for WACCM4 and CAM5 are identical, is this correct?

P10718, l10: file->data

P10718, l13: “unified basis of information” means nothing to the reader. A little more information on the advantage(s) of the CCMI dataset is needed here.

P10719, l18: documentation->specifications

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P10719, l21: again, “interact”

P10719, l26: need->needed

P10720, l1-8: this paragraph is not quite clear. What “other parameterizations” are meant here? Temporal interpolation may change the instantaneous fields, but the monthly mean of the instantaneous fields should match the prescribed field in the forcing file, and so it's not clear why this is of special interest.

P10722, l28: insert “simulated” into this sentence

P10722, l28: Is this difference really significant? The 1 sigma ranges overlap.

P10722, l28: It would be helpful to compare these TOA flux anomalies to actual data (e.g., ERBE).

P10723, l5: no apostrophe on forcings

P10724, l4: “unifies” is too strong, see general comments

P10724, l10-12: This is potentially interesting, but since nothing is shown in the results, it does not belong in the Summary.

References:

Canty, T., Mascioli, N. R., Smarte, M. D. and Salawitch, R. J.: An empirical model of global climate – Part 1: A critical evaluation of volcanic cooling, Atmos. Chem. Phys., 13(8), 3997–4031, doi:10.5194/acp-13-3997-2013, 2013.

Thompson, D. W. J., Wallace, J. M., Jones, P. D. and Kennedy, J. J.: Identifying Signatures of Natural Climate Variability in Time Series of Global-Mean Surface Temperature: Methodology and Insights, J. Clim., 22(22), 6120–6141, doi:10.1175/2009JCLI3089.1, 2009.

Interactive comment on Geosci. Model Dev. Discuss., 8, 10711, 2015.