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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.

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All author responses are in *italics*.

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 in- formation 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 reproduction of global mean surface temperature anomalies after Pinatubo. With the new aerosol prescription, the global mean temperature anomaly is shown to be around 0.2âŮęC, and they compare this to time series 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 0.14âŮęC (Canty et al., 2013), to around 0.4âŮęC (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.

We agree with all the issues raised by the reviewer in this comment and have reduced our claims (throughout the text) that suggest the significant improvement of the CESM's global mean temperature response to the the stratospheric forcing. This includes use of the two references provided by the reviewer to help frame the uncertainty in the observed global mean temperature response. Though this work is still motivated by the desire to improve CESM's fidelity, demonstrating a significant improvement in the response of CESM's global mean temperature to the Pinatubo eruptions is, as mentioned by the reviewer, extremely difficult. The difficulty in this task is mainly due to the observed and simulated climate variability. Other biases between observations and the

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model (such as differences in the simulate versus observed ENSO state) as inhibit a clean comparison. Thus, fully demonstrating an improvement in the global mean temperature response of CESM1 to a colossal volcanic is beyond the current scope of this work. This is in part due to the computing necessary to create an ensemble of runs (perhaps >40 of each simulation given the variability seen in CESM's Large Ensemble (Kay et al. 2015)) to accurately estimate the variability of the model. Most notably changes in response to this comment were made to the Abstract, the results described in Section 6 (Page 11 line 22, page 12 line 1 to line 23) and Summary.

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.

We agree that due to the complex nature of the CESM family of model's and the historical differences in the various flavors of CESM's approach to prescribing stratospheric aerosol, Section 6 appears to apply to only CAM5. To clarify that we have improved all of the model's within the CESM1 family, we have now included a new Figure 3 which demonstrates the improvements to the zonal mean, monthly mean SAOD in CAM4 (and CAM4-chem WACCM4 as they all three utilize CAMRT) and CAM5. To further show the specific improvements to CESM1 in both CAM4 (and all other models that use CAMRT) and CAM5 we have also included a new section in the supplement (S3) which compares the specific contributions of the improvements to the forcing file and optical description of the aerosol. We disagree with the point that this work is mislead-

ing with regards to the unification of the the treatment of stratospheric aerosol within CESM1. In this work, we have unified CESM1's prescription of stratospheric aerosol by creating a framework that allows all configurations of the model to use the same forcing file and have laid out a single set of assumptions to allow this forcing to be utilized in both CAMRT and RRTMG as well as the chemistry module. Due to differences in the radiative codes used by the various configurations of CESM1 (most notably the spectral bands used in the radiative transfer calculations), some differences exist in the exact prescription but, now that the setup of these prescriptions is derived from a singular set of forcing and assumptions there minimal differences in the resulting SAOD simulated by the configurations of CESM1. This is explicitly shown in the new Figure 3 and is also now summarized in Table 1.

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. 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.

We agree that it is beneficial for the community to see the differences in the improvements to CESM1's representation of stratospheric aerosol created by the new forcing file versus those improvements due to the radiative/optical assumptions and treatment of the information contained in the forcing file. To this end we have included a new Figure 3 that compares the new and old parameterizations for CAM4 and CAM5 in the main text of this work. We have also included 4 new figures in the Supplement (S3) that parse out the contributions of the new aerosol mass forcing and the changes to the optical and radiative assumptions for both CAM4 and CAM5.

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$_4 l forcing files.$

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The aim of this work is to describe the new unified prescription of stratospheric aerosol across all configurations of CESM1. As this work notably includes the description of a new forcing file that specifies both aerosol mass and SAD we have included this within the title. As changes to the chemistry parameterization of CESM1 are beyond the scope of this work, we have not included any results of this manuscript. For a description of these results, please see Tilmes et al. [2015] as suggested in the summary.

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.

We have now included Table 1 to summarize the main similarities and differences of the main features of all the old and new parameterizations. Unfortunately, a simplification of all the CESM1 model configurations referred to is not possible. A pdf of the new table is attached to this response as well.

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.

We agree and have rephrased references to "most" CMIP5 models throughout the text to this to focus solely on CESM1.

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.

We disagree as the ability of the model's parameterization to utilize the information within a forcing database is linked to the development of databases that contain the *necessary information for the model to appropriately ingest. The GISS/Sato database has indeed included an effective radius for some time but did not include aerosol mass or SAD. As such, the new database utilized here is the first database to allow for both variation of both mass and effective radius in stratospheric aerosol within the CESM1 framework.*

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

This has been rephrased to the following: "Volcanic perturbations to stratospheric aerosol are an often ill represented forcing in the climate system (Solomon et al., 2011; Driscoll et al., 2012; Knutson et al., 2013)."

P10712, l25: eruptions->simulations

Rather than use the suggestion of the reviewer, this sentence has been changed to "The vertical dashed grey lines note the date of colossal volcanic perturbations accounted for in the forcing files utilized in the CMIP5 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.

We agree and have removed the statement singling out the 1991 Pinatubo eruption due to the complication created by the ENSO state in the observations.

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

This has been corrected to the following: "Stratospheric aerosol is prescribed in several ways with various levels of complexity in global climate models. Most models contributing to CMIP5, including NCAR's Community Climate System Model, version 4 (CCSM4) [Meehl et al. 2012], use a scheme that prescribes a zonal mean, monthly mass, optical depth and or surface area density (SAD) of stratospheric aerosol (using datasets such as created by Ammann et al. [2003] and Sato et al. [1993])."

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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.

We agree and this has now been changed to: “Typically this specification of aerosol is ingested within the model’s 1) radiative transfer parameterization and 2) chemistry parameterization using several underlying assumptions about the size distribution and composition of the aerosol.”

P10713, l27: CCSM4 was defined above.

This has been removed.

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

No, the original CAM4 prescription is based on a layer integrated mass.

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

We disagree as the effective radius should be a well known quantity for the intended audience of this work and is only used here as a reminder for the reader.

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

We agree and have changed the sentence to the following: “In CAM4 the stratospheric aerosol mass is interpreted by the radiative transfer code via the predefined mass-specific extinction, single scattering albedo, and asymmetry parameters.”

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

This has been corrected in the text. As the standard deviation based on the ln(r) it is unitless (See Matta, Massa, Gubskaya Knoll, (2011), Can One Take the Logarithm or the Sine of a Dimensioned Quantity or a Unit? Dimensional Analysis Involving Transcendental Functions, Journal of Chemical Education, Vol 88, No. 1.)

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

This has been corrected

P10717, l18: SAD defined already.

This has been corrected throughout the manuscript.

P10717, l19: change reference style.

We have corrected this.

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.

We agree and we have clarified this section.

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.

As the Tabazadeh et al., (1997) parameterization is not used in the new parameterization described in this work, we do not feel that it is necessary to describe it fully and refer the reader to the original work. A comparison of the resulting new and old stratospheric SAODs for CAM4 and CAM5 is show in Figure 2 and the new Figure 3. Further comparison of the impact of the new aerosol mass versus the changes to the optical assumptions are also now provided in the new supplement section S3. These comparisons are proportional to the derived aerosol masses so a further figure is not warranted.

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

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We believe the tags are necessary to fully document which configurations of the CESM include the changes described in this work due to the complexity of the ever changing CESM1 code base. It is correct that the CESM1 code base for the changes to WACCM4 and CAM5 are identical. This has been clarified in the text.

P10718, l10: file->data

This has been corrected.

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.

We agree and have clarified the sentence to the following: “The main advantage is that the new dataset includes information on the mass, effective radius and surface area density that are all derived from a coherent set of observations and modelling assumptions.”

P10719, l18: documentation->specifications

This has been changed.

P10719, l21: again, “interact” *We agree with this rephrasing and have changed the sentence to the following: “To fully implement the new stratospheric SAD file in CESM1(CAM4-chem-CCMI) and CESM1(WACCM4-CCMI) several modifications were made to the mechanics of how the CESM1 ingests stratospheric aerosol forcing files so that information about the size of the aerosol could be included with the radiation calculation.”*

P10719, l26: need->needed

This has been corrected.

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.

We agree that this paragraph was not clear and have not rephrased this section. It is also not true that the monthly mean of the model interpreted data matches the monthly mean of the forcing file due to the process of linearly interpolating (i.e. smoothing) between the ingested data points. For further explanation of these issues in CESM1, please see Neely et al. [2015].

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/8/C4369/2016/gmdd-8-C4369-2016-supplement.pdf

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

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Model Version	Radiative Transfer Model	Standard Aerosol Mass Input File	Mass Composiition Assumptions	Optical Properties Look Up Table	Optical Properties Dependencies	Size Distribution Assumptions	SAD File
CCSM4/CESM1(CAM4)	CAMRT	CCSM4_volcanic_1850-2008_prototype1.nc	75% H_2SO_4 + 25% H_2O with ρ =1750 kg/m^3 at 215K	sulfuricacid_cam3_c080918.nc	Spectral Band & Mass	Log-normal with Constant wet r_{eff} = 0.426 µm Constant σ(ln r) = 1.25	N/A
CESM1(WACCM4)	CAMRT	Computed from SAD	Kinnison et al. [2007] & Tabazadeh et al. [1997]	sulfuricacid_cam3_c080918.nc	Spectral Band & Mass	Constant wet reff = 0.5 µm Constant σ(ln r) = 1.25	SAD_SULF_1849-2100_1.9x2.5_c090817.nc
CESM1(CAM5)	RRTMG	CCSM4_volcanic_1850-2008_prototype1.nc	75% H_2SO_4 + 25% H_2O with ρ =1750 kg/m^3 at 215K	rrtmg_Bi_sigma1.8_c100521.nc	Spectral Band, rg & Mass	Log-normal with rg diagnosed from mass density Constant σ(ln r) = 1.8	N/A
CESM1(CAM4-chem-CCMI) and Newer Tags & CESM1(WACCM4-CCMI) and Newer Tags	CAMRT		75% H_2SO_4 + 25% H_2O with ρ =1750 kg/m^3 at 215K	volc_camRT_byradius_sigma1.6_c130724.nc	Spectral Band, r_g & Mass	Log-normal with Varying rg as specifed by input file Constant σg =1.6	Read from Standard Aerosol Mass Input File
CESM1(CAM5) and Newer Tags	RRTMG		75% H_2SO_4 + 25% H_2O with ρ =1750 kg/m^3 at 215K	volc_camRRTMG_byradius_sigma1.6_c130724.nc	Spectral Band, r_g & Mass	Log-normal with Varying r_g as specifed by input file Constant σ_g =1.6	Read from Standard Aerosol Mass Input File

Fig. 1. New Table 1

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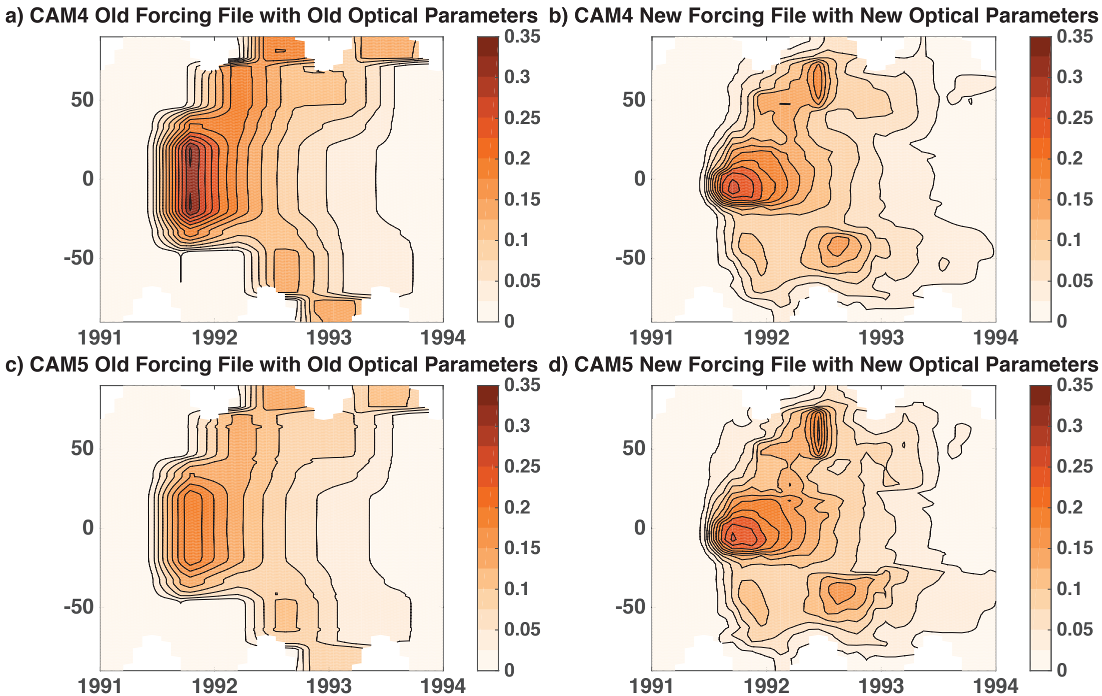

Fig. 2. New Figure 3

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