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Interactive comment on "Evidence for the predictability of changes in the stratospheric aerosol size following volcanic eruptions of diverse magnitudes using space-based instruments" *by* Larry W. Thomason et al.

Anonymous Referee #2

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General Comments

This paper presents a simple study of the way the 525 nm to 1020 nm-extinction coefficient ratio, a proxy for the particle size, evolves in function of the 1020 nm-extinction coefficient during the early period following the volcanic eruption. This study is based on ten test cases of volcanic eruptions that affected the stratosphere and were measured by eitther SAGE II, or SAGE III on ISS. The author's aim is to show that the joined evolution of these two parameters follows in many cases a simple relationship, regardless of the particle size distribution or the details of the circumstances of the

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Discussion paper

eruptions.

This study definitely lacks precision in my opinion. The whole analysis is based on approximations, quite subjective considerations and fast conclusions that remove convincing ground to the argumentation, yet based on a necessary limited sample of eruptions. Furthermore, since one of the two key dataset (i.e. SAGE III/ISS) shows a bias at one of the considered wavelengths, they interpolate these values using a simple law, while the situation after a major eruption like the ones studied here is expected to be complex. Building on approximate data with coarse approximations and arguments gives results that are sometimes questionable, especially in view of some applications the authors have in mind.

Concerning the text, the authors' formulation is in some cases so general that they make the described concept no more relevant, e.g.: "the stratospheric aerosol optical depth" [at 525 nm or 1020 nm], mentioned as if it was a fixed number. The authors should also be more accurate in their naming of the quantities they consider (e.g.: "[aerosols] extinction coefficient perturbation ratio", "perturbation aerosol extinction coefficient ratio", "perturbation extinction ratio", "perturbation ratio"). Overall, sloppiness brings a lot of confusion in the text and undermines the argumentation.

Concerning the case of the Mt Pinatubo, it is particularly surprising that this case is considered and discussed without any mention of the fact that SAGE II could not appropriately measure the extinction coefficient during several months after the eruption due to the extreme opacity of the volcanic cloud. This is however a major drawback for the present study.

Consequently, using this analysis and the relationship inferred by the authors to validate complex models as suggested by the authors seems far premature and overrated.

Specific comments

L. 15, p. 1: The integral over its whole domain of definition, of a distribution function is equal to 1. The change in multiple orders of magnitude should concern the aerosol size number density, not the aerosol size distribution.

L. 24-25, p. 1: For such general consideration, I would suggest citing Robock, Rev. Geophys., doi: 10.1029/1998RG000054, 2000.

L. 1-2, p. 2: To illustrate the efforts to model the climate impact, I suggest citing some work by Timmreck et al., for instance, Timmreck et al., Geophys. Res. Lett, doi: 10.1002/2015GL067431, 2016.

L. 55-58, p. 2: This sentence is correct, but gives still a biased view of the reality: the SAGE II mission was exceptional in several senses, one of them being that it spanned a period with a particularly high amount of very large volcanic eruption. If it had been launched 10 years later, the situation would have been very different. The authors should be attentive to give a correct view of the reality.

L. 56, p. 2; L. 79, p. 3; L. 360, p. 13; L. 437-438, p. 26; caption Figure 1, p. 15: Kovilakam et al. is not published so far, and mentioning it as if it was the case is not ethical and should not be done. If this paper is not accepted in due time, please refer to another paper, e.g. Thomason et al., 2018.

L. 63, p. 2-L. 65, p. 3; L. 72-75, p. 3: This is not true and should be corrected. The ESA Envisat mission provided three experiments with high interest for aerosol studies: the SCIAMACHY spectrometer measuring in the UV-visible-near-IR range (von Savigny et al., doi:10.5194/amt-8-5223-2015, 2015; Noel et al., Atmos. Meas. Tech, doi:10.5194/amt-2020-113, in review, 2020), the IR limb sounder MIPAS (Griessbach et al., Atmos. Meas. Tech., doi:10.5194/amt-9-4399-2016, 2016), and the UV-visible-near IR stellar occultation instrument GOMOS (Bingen et al., Remote Sensing Env., doi: 10.1016/j.rse.2017.06.002, 2017). Also, the ACE-MAESTRO mission provided aerosol extinction from solar occultation measurements (McElroy et al., Appl. Opt., doi10.1364/AO.46.004341, 2007).

L.103-104, p. 4: In the case of the major eruptions that the authors consider, the situation is definitely more complicated than in this simple monodispersed aerosol model.

L. 106-108, p. 4, Figure 3: “The stratospheric aerosol optical depth” is vague. Please specify. Figure 3: SAGE II was not able to measure correctly the aerosol extinction during the months following the Mt Pinatubo due to the saturation of the atmosphere in aerosols. How do the authors infer the “stratospheric aerosol optical depth” during this period?

L. 125-126, p. 4: The authors should be specific: Which value of the 525 nm and 1020 nm aerosol extinction coefficient do they choose to infer “the impact of these eruptions”? The conversion of sulfur gases to sulfuric acid is a process that requires several weeks, and the presence of ashes may significantly influence the aerosol population in the early phase after the eruption, as mentioned before by the authors.

L. 128-129, p. 5: This is a serious setback for this study! A “simple Angstrom coefficient” interpolation states that the aerosol population has a simple structure, which is probably not the case in the post-eruption period where different aerosol modes (thin particles, ashes, aged aerosols) coexist. The interval [448 nm, 756 nm] is quite large and inferring the extinctin coefficient value at 521 nm is uncertain. Furthermore, while implementing an interpolation, why don’t the authors interpolate at 525 nm, the wavelength effectively used in the paper?

L. 132-133, p. 5: How do the authors assess the difference between the interpolated value and a good approximate of the true value, if the SAGE III/ISS are biased?

Figure 4: Please indicate the Julian day corresponding to 1 January 1985. Is it Day 200?

L. 148-149, p. 5: Without indication of which points correspond to volcanically pertubed observations, and which ones to unperturbed periods, it is not possible to assess the pertinence of this sentence.

L. 149-150, p. 5: What is a “low extinction coefficient ratio”?

L. 150, p. 5: Was this hypothesis checked in some way?

L. 152-155, p. 5: If the authors clearly excluded any considerations about fire and pyrocumulus events, what do they mention them here? Or conversely, why did they reject such events in L. 69-70, p.3, if they use them here?

L. 158, p. 5: Since the time between two visits is about a few weeks to months, the observed evolution potentially starts at very different stages of plume development, possibly characterized by very different aerosol population features (importance of the ash fraction, development of the aerosol microphysics, mono/multimodal character of the size distribution, etc.).

L. 160, p.5: How do the authors select “the maximum value”? As shown in Figure 4, such situation is rich in outliers. Do the authors consider the highest outlier? Which is the relevance of such a choice, in a situation where the instrument measures locally, at a single time occuring after “a few weeks to months”, more or less close to the eruption?

L. 161, p. 5 and Figure 4: What do the authors mean by a “9-point window”? Is each point in Figure 4 such a “maximum” in a 9-point window?

L. 162, p.6: “main aerosol layer” vs “entire layer”: do the authors refer to the volcanic cloud, as opposed, say, to the combination of the Junge layer and the volcanic cloud?

L. 165, p. 6: What are the authors speaking about? “To produce a mean value” of what? “we required a minimum of 6 points”: which kind of points and over which selection?

Figure 7: Same question as for Figure 3. Furthermore, the authors should add the Ruiz case to ease the comparison.

L. 172-178, p.6: This classification is poorly convincing. Concerning the events with “a rapid increase in aerosol extinction coefficient and ratio following the eruption", this cannot characterize Ambae for which the increase in extinction ratio mainly spans about the 120 days preceding the eruption (as indicated by the anthors), saturating at the moment of the start of the eruption while the extinction coefficient just starts to increase. The authors find the cases of Ruang and Kelut as "following their own way" while these cases have many similarities with Raikoke that they see in another category. Clearly, the categorization is subject to interpretations, what makes the classification over an 8-eruption sample poorly convincing.

Figure 8a: The authors should show in some way, for each eruption, where is the "before" and where is the "after".

L. 179-181, p. 6: How do the authors define "the first data point"? Is it the extinction ratio value corresponding to the earliest time plotted in Figure 7? In Figure 7, the time interval considered between this first data point and the eruption start time is different in each case and the choice of this time interval obviously weights on the initial value of the extinction coefficient ratio, and hence of the "extinction coefficient perturbation ratio" shown in Figure 8b. Furthermore, in at least 2 of the 8 considered cases (Cerro Hudson and Manam), there is no clear indication at all that the start of the curve corresponds to any "baseline", and in several other ones (Kelut, Pinatubo, Ruang, Ulawun, Raikoke), the presence of such "baseline" is very uncertain. Finally, the scales used for both the extinction coefficient and the extinction coefficient ratio are different and differently related for all cases, making the establishment of parallel behaviour uncertain. Hence, drawing any conclusion on these values, taking into account the high uncertainty on the value of the maximum extinction [ratio] (See comment on L. 160, p.5) looks particularly hazardous.

L. 184-186, p. 6: See previous remark.

L. 197, p. 6-L. 201, p. 7: Beyond the uncertainties related to the values plotted in Figure 8b (See comment on L. 179-181, p. 6), this model is so coarse, ignoring the fact that some eruption produce more sulfur gases and some other ones more ashes, ignoring all effects related to the geolocation and consequent dynamical features of the plume dispersion as well as seasonal effects, and ignoring intrumental limitations (notably SAGE II's "blindness" in the early phase of the Pinatubo eruption), that it seems absolutely premature to draw any conclusions from these 10 cases among which only a handful of the considered ones follow a nice flat curve. Applying such relationship to other data sets from limb sounders where retrieved extinction values depend yet on a set of assumption, is at risk to get quite far removed from the truth. Instead of suggesting using this tool for the evaluation of interactive aerosol modules for GCMs and ESMs, it would be useful, on the contrary, to use GCMs with elaborate aerosol microphysical models to assess if this relationship is sufficiently grounded to be used elsewhere.

L. 206-207, p. 7: If two closely related eruptions from a same volcano, occurring within such a short period of time, have yet to be splitted in a "regular case" and an "outlier", it becomes very hard to find any foundation in the proposed relationship. Even if other observations could bring some evidence that the plume composition, due to this quick succession, was very different in both cases, such argument could not be invoked since this kind of consideration has been excluded in this study.

L. 210-213, p. 7: Choosing the "initial state" of the second eruption of Ambae (with a value of 4.9 for the extinction ratio) is a highly speculative exercise ignoring the fact that the microphysical evolution is highly perturbed by the addition of fresh material. In no way, the situation corresponding to this value of 4.9 for the extinction ratio can be considered as a "baseline". Furthermore, the given values are quite arbitrary: the value of 5 is never reached by the extinction ratio (the maximum is about 4.7), and the extinction ratio decreases far below 4.1, toward a value lower than 3.5 and that cannot be estimated from the limited curve shown in Figure 7. Hence, putting both Ambae eruptions again (and in contradiction to what is observed in L. 206-207, p. 7, see comment) in the category of "regular cases" is particularly dubious.

L. 220-221, p. 7: This looks like a fast conclusion about a situation where microphysics, dynamics, and multiple injections of volcanic matter combine in a complex way, as illustrated in Figure 9.

L. 223, p.7: “the main aerosol layer”, “all parts of the volcanic cloud”: please clarify (See also comment on L. 162, p.6).

L. 226-228, p. 7: What do the authors mean by “the first observations”? Also, the end value is rather 2.9 than 3.

L. 228-229, p.7: Where do the authors find this value of 2.7? Such value is never reached in Figure 7, nor is indicated in Figure 8. Is the “perturbation extinction ratio” another quantity than the ones illustrated in these figures?

L. 230, p. 8: What is a “compact extinction coefficient [ratio]”?

L. 230-235, p. 8: It is surprising to give such an importance to outliers that in other circumnstances would be fastly overlooked.

L. 239, p. 8: What do the authors mean by “the spread of the Nevado del Ruiz, Cerro Hudson and Raikoke eruptions”?

L. 243-251, p. 8: This part of the text looks like a suite of speculations without attempt to analyse them seriously, making this enumeration not very useful.

L. 257-259, p. 8: The sentence is unclear. Please rephrase.

L. 263-266, p. 9: The sentence is unclear. Please rephrase.

L. 286, p. 9: What do the authors mean by “identical perturbations”?

L. 285-293, p. 9 and Figure 11: Basically, the extinction coefficient at 525 nm and 1020 nm and hence they ratio, can be exactly calculated by Mie theory for fixed values of the aerosol volume density and for single-radius particles. Hence, the differences found in Figure 11 for Nevado del Ruiz and Ambae only shows in which extend this simpel model of single-radius particles departs from the reality. Furthermore, the authors mention coagulation effects, but ignore sedimentation which is nevertheless crucial after a large eruption, and high from the early post-eruption period. L. 289, p. 9: There is no single timescale used in Figure 11, nor in Figure 2, and the authors should thus not use this concept alone.

L. 305-307, p. 10: From the simplicity of this model and the fact that important aspects are neglected (e.g. the absence of sedimention which critically influences the extinction and extinction ratio), I do not think that any conclusion can be drawn about inferring primary microphysical effects from these SAGE II and III/ISS observations.

L. 323, p. 10: Which “measurement paradigms” are the authors talking about?

L. 327-328, p. 11: Using a tool to assess the data quality of a data set is only meaningful if this tool is not based on coarser assumptions and approximations as the ones leading to this data set. I am not sure that this is the case in the present work.

L. 331-333, p.11: Among the different applications proposed by the authors, the use of an extinction ratio value inferred from a relation established from the points 6-5-8-7-0-2 in Figure 8b to fix the size parameter used in the OSIRIS retrieval (e.g. through the Angstrom coefficient) seems to be the only one for which this simple model could have a real added value, in my opinion. It is really a pity if this model even cannot be beneficial in this framework.

Technical corrections

Caption Figure 9: It would be useful to indicate here the latitude of the Ambae volcano.

L. 216-217, p. 7 and caption Figure 9: the indications of wavelength and time are not consistent between the text and the caption. Please specify the exact time duration (start and end time).

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