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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Nabro volcano aerosol in the stratosphere over Georgia, South Caucasus from ground-based spectrometry of twilight sky brightness" *by* N. Mateshvili et al.

Anonymous Referee #2

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This paper presents an analysis of twilight sky brightness measurements before and after the Nabro and Pinatubo volcanic eruptions, and describes how these measurements can be used to derive an aerosol extinction profile, at 780 nm, showing the influences of recent volcanic aerosol and demonstrating the usefulness of such twilight measurements. If the authors stuck to their task, and really demonstrated how well their measurements, and their retrievals, did in comparison to independent measurements, then this could be a useful contribution to the literature. Unfortunately the paper does not do that. Not until the penultimate figure are any independent measurements of aerosol extinction presented, in spite of the abundance of independent measure-

ments available, and then they are included on a postage stamp sized figure with very poor altitude resolution. Instead of sticking to the main task, the paper wanders and the discussion is often superficial.

For examples:

In the introduction the discussion wanders from the source of stratospheric aerosol, to their importance for global warming, to observations of minor eruptions by satellite, lidar, and aircraft instruments, with references for many of the recent minor eruptions, finally settling on Nabro, but not including a balanced discussion of the differing opinions on the height of the initial volcanic effluent, then steps back to an overview of remote sensing, before finally settling on ground based spectral measurements, which is the subject of this paper. This wandering and superficiality continues into the body of the paper.

Section 2 is 10 lines long.

Section 3, on equipment, broken into 2 subsections is 24 lines. All the information in this section could be encapsulated in one nice table. The authors artificially create datasets I and II in this section, with dataset II measured in 1991 and dataset I in 2009-2011.

Section 4 jumps directly to measurements, and a lengthy discussion of dataset I, including more unbalanced discussion of the plume height of the Nabro eruption, while section 4.3 on dataset II, has one sentence, but a whole subsection (4.4), for Pinatubo.

Finally the authors get to the meat of the paper which is the retrieval algorithm and procedure, sections 5 and 6, before application of these procedures to measurements. The authors, however, then fail to compare their retrievals with satellite measurements which would be available very near to their wavelength of 780 nm, and their latitude. OSIRIS measurements on the Odin satellite (Bourassa et al., 2010) at 750 nm spring immediately to mind. It should be straight forward to include these reference measurements in Figures 10-13, along with a discussion of their comparisons. In this way a reader may gain confidence in the twilight measurements, and in their retrieval. With the current submission there is no information on how well the authors are doing in their stated purpose of retrieving such aerosol profiles, and most readers would not be confident in these measurements and retrievals.

The paper can not be accepted in its present form. If the authors were to focus the paper on what they know, that is their measurements and the retrievals, compare their retrieved profiles with independent measurements, and eliminate the unnecessary material and speculation, then perhaps it could be published. If the authors choose not to follow this guidance in a major revision, then I recommend rejection, and would not be interested in seeing a revised copy.

The following are my suggestions for the authors to modify this paper into an acceptable contribution. The paper must focus and present, in a logical flow, their technique and their measurements, with the measurements following the equipment and the retrievals. It must include comparisons with independent measurements for the retrieved profiles, and discussions of differences. It must be shortened by eliminating unnecessary discussion, speculation, misleading statements, sections, and figures.

The following are candidates for removal/shortening.

Much of the current introduction could be removed, keeping a bare minimum on the importance of stratospheric aerosol, but adding more on the measurement of stratospheric aerosol from the ground, using techniques similar to those employed here, thus a survey of contributions by photometers, pyrheliometers, and twilight sky brightness.

Combine sections 2 and 3 and use a table for the differences in the photodetectors used. Eliminate the artificial dataset I/II. Just refer to the data by the time period for their measurement, then the reader will know what volcanic period is involved.

Eliminate section 4 entirely. It doesn't add to the paper, and the retrieval technique

should be discussed before specific measurements are discussed.

Eliminate section 7.4. This has nothing to do with the measurements presented here, nor their analysis.

Eliminate Figures 4-6, 15

Fig. 4. There are many examples of this figure when the measurements are discussed.

Fig. 5. There are no measurements at Tbilisi during the time period of this trajectory. Three days after the Nabro eruption is to soon to see the formation of stratospheric aerosol. These are tropospheric trajectories.

Fig. 6. This figure is at minimum premature since the technique has not been fully described, and the units are arbitrary, so it is not that helpful. Such information could be included on Fig. 12 by including the pre-Nabro measurements of brightness from Fig. 11a), then the perturbation from Nabro should be clear, and there the units would not be arbitrary.

The following are specific comments made while reading the paper. If the above suggestions are incorporated into a revised copy, then some of what follows no longer applies, but I leave it in here for completeness. Location is page-number.line-number

4404.15 In addition to the comments by Fromm et al. (2013) and Vernier et al. (2013), this manuscript must acknowledge the reply by Bourassa et al. (2013), which shows from MLS data that only a small fraction of SO2 penetrated the tropopause, whereas the majority of SO2 was in the troposphere until its advection to, and interaction with, convection in the Asian monsoon.

4407.10-11 and Fig. 3. It would be clearer if Fig. 3b) was shown just for the pixel ranges available from the measured spectral image, e.g. 670 -800 nm, and if the measurement box (red) was included in both 3a) and 3b). This would indicate there is a feature missing in the observations, near 680 nm. Is there an explanation for this? Also, if the intensity units on the ordinate are arbitrary why shouldn't they be in the same range?

4408.22-4409.6 and Figure 5. What is the point of this discussion and this figure for this paper? The three day advection the authors show is tropospheric, while at 10 days after the eruption, the authors measure background stratospheric aerosol on 24 June 2011, clearly showing, as we all know, that it takes time for the SO2 from a volcano to be converted to aerosol. Thus this whole discussion of the plume height just after Nabro has nothing to do with the authors observations, which show the Nabro plume in the middle of July, after, as Bourassa et al. (2012) show, the SO2 has been lofted into the stratosphere, and converted to aerosol, filling the Asian monsoon region. Thus I see no need to discuss the eruption altitude estimates of Fromm et al. (2013), which use MODIS and are fraught with serious altitude uncertainties. These should either be removed along with the estimates of Vernier et al. (2013), since they do not impact this paper, or the authors must include the additional, in my mind more convincing, evidence shown in Bourassa et al. (2013) for the evolution of the Nabro plume. But is this paper about the evolution of the Nabro plume, or the utility of twilight observations and their use in the case of Nabro? The authors must decide. If it is the former then I would not accept it, because their measurements do not add any useful information. If it is the latter then they should stick to their observations, flesh them out, and convince the reader of their accuracy. For example what other information can be inferred from Figure 6, the altitude, the extinction, how does that compare from satellite extinctions at near the same time? These questions were asked prior to my recommendation, above, to eliminate this figure. I still favor elimination as the comparison can be shown in Fig. 12.

4410.1-5. Two sentences merely conveying the time when the observations were taken in 1991 does not deserve a header and a paragraph. Just include the information at the end of the previous paragraph. This is a useless waste of space.

4411.3. A more correct term would be a difference equation.

Fig. 7a). The ordinate is mislabeled. It should be ratio of intensity from multiple scattering compared to single scattering as indicated in the text. A multiple scattering correction factor is not defined.

Fig. 7b). Where are these aerosol extinction profiles from? It is not clear from the text. It should also be stated in the figure caption.

4412.19-29. The sentences beginning with, “Let us . . .” are not necessary and should be deleted. The language using derivatives is confusing and unnecessary. Eq 1 defines yi, which is a difference of logarithms of intensity, now use it.

Fig. 8. Label the ordinate with what it is Log(Ii+1/Ii)/0.1° SZA. This is true for the SS curves but it is not clear here, nor in the text, how the MS curves are established for this figure. Are these just the logs of “MS correction” from Figure 7. That doesn’t seem to make sense when considering the differences of the logs? It needs to be explained.

4413.11. I don’t understand what the authors intend with the last part of the following sentence, “The center of the Sun corresponds to the actual SZA whereas the upper and lower points – to the SZA±a/2.” The upper and lower points of what?

5.3.2-5.3.4. Does each one of these need a separate section with title, when they are only a few sentences?

4416.0-2. The conclusions here make Fig. 4. seem a little misleading, showing results up to SZAs of 102°. What altitudes do 94 and 93.5° SZAs correspond to in Georgia?

6.5. The authors here are going to “investigate if local . . .” and suggest that they are carefully considering such problems as indicated here, but then end up just waving their hands and saying that clouds and haze are not a problem. For example, since a local low aerosol layer affects the light passing through it from different stratospheric altitudes in exactly the same way, it can be easily discounted. Does this really require a “fully spherical single scattering model with multiple scattering corrections” to come to this conclusion?

4416.24. What does “these” refer to?

4417.22. “This means . . .” I do not understand the intent of this sentence. Uncertainties are modified before what?

4417.Eq 4. How does Eq. 4 differ from Eq. 1?

4417.Eq5. Are the authors applying the Gaussian law of error propagation? If so I believe the left hand side should be (σ(F)/F)2.

Figs 10-12. c) What is the difference in the altitudes on the two axes? What is being contoured? d) What do the three lines represent? I apologize for my ignorance on the presentation of these averaging kernels.

Fig. 14. This figure needs revised. All the information on c) can be included in a). That would permit b) to be made large enough to be seen. This can all be accomplished with a good choice of colors. Thus the blue line in a) should be paired with a similar blue line in b), not with a black line in b). This would also show that the authors modification of the retrieved aerosol profile is not corroborated by the SAGE II measurements, and there should be some discussion of that. By including the curves in c) on a) it would also clearly show how important multiple scattering is. What is “double scattering,” and how much different is it than multiple scattering in the impact on radiance measurements? Adding a second abscissa scale indicating approximate altitude would also be nice.

4422.5. Finally, the authors compare their derived aerosol extinction profiles with an independent measurement. This is way too late in coming.

4422.18-Fig. 14b). This construction to reproduce the measurements at high SZA is artificial. The modified profile makes no physical sense, and the altitude scale is so coarse that it defies easy consideration of the profile. Is the figure made small to limit careful inspection? It seems something else is wrong. If the measured twilight curve after Pinatubo can be used to derive an aerosol extinction profile, which more or less

agrees with SAGE, then why doesn’t the model of twilight sky intensities reproduce the measurements? If there is a discrepancy, I would suspect the model, not the aerosol profile.

4423.1-3. This point would be much clearer if the curves in c) were included in a).

Section 7.4. I don’t understand the point of this section for this paper, which has been about sky brightness measurements at 780 nm, and a model of the scattering which leads to these measurements. Now the model is transferred to a wavelength that has not been discussed, to a phenomena which could not be observed at 780 nm, and the authors want to make conclusions about the different spectral signatures from noctilucent clouds versus volcanic aerosol, which should be obvious based on the differences in the particle sizes involved. This section does not contribute to this paper.

By the way, I am not Bourassa.

Bourassa, A. E., Degenstein, D. A., Elash, B. J., and Llewellyn, E. J.: Evolution of the stratospheric aerosol enhancement following the eruptions of Okmok and Kasatochi: Odin- OSIRIS measurements, J. Geophys. Res., 115, D2, D00L03, doi:10.1029/2009JD013274, 2010.

Bourassa, A. E., A. Robock, W. J. Randel, T. Deshler, L. A. Rieger, N. D. Lloyd, E. J. Llewellyn, and D. A. Degenstein: Large volcanic aerosol load in the stratosphere linked to asian monsoon transport, Science, 337, 78-81, 2012.

Bourassa, A. E., A. Robock, W. J. Randel, T. Deshler, L. A. Rieger, N. D. Lloyd, E. J. Llewellyn, and D. A. Degenstein: Response to Comments on "Large Volcanic Aerosol Load in the Stratosphere Linked to Asian Monsoon Transport", Science, 339, 647, 2013. DOI: 10.1126/science.1227961, 2013.

Fromm, M., Nedoluha, G., and Charv′ at, Z.: Comment on “large volcanic aerosol load in the stratosphere linked to asian monsoon transport”, Science, 339, 647, doi:10.1126/science.1228605, 2013.

Vernier, J.-P., Thomason, L. W., Fairlie, T. D., Minnis, P., Palikonda, R., and Bedka, K. M.: Comment on “large volcanic aerosol load in the stratosphere linked to asian monsoon transport”, Science, 339, 647, doi:10.1126/science.1227817, 2013.

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