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Atmospheric Measurement Techniques Discussions

Interactive comment on "Observation of volcanic ash from Puyehue-Cordón Caulle with IASI" *by* L. Klüser et al.

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This paper describes an interesting approach to the retrieval of volcanic ash from high spectral resolution infrared satellite measurements using IASI. The approach seems to be based on some earlier work devoted to studies of windblown desert dust and that work has been published and the theoretical framework has been established. This paper extends that approach to volcanic ash and provides a nice example from the recent Puyehue Cordon Caulle (PCC) eruption in southern Chile. A second part of the paper discusses the use of a dispersion model to understand the transport of the ash and, in part, verify the emission height. It is stated in the Abstract that the method has advantages over backscatter methods (presumably those based on OMI and GOME-2

retrievals) as solar illumination is not needed. As a contribution to the existing literature on this subject, this paper adds not much. I am not aware that volcanic ash retrievals have been made from solar backscatter measurements; certainly SO2 retrievals are routinely made, but this is not volcanic ash. Furthermore, the retrieval theory for using measurements in the 8-12 micron window is quite well established. The use of transport models to both corroborate satellite retrievals and perform inversions is also well established and there are several recent papers that describe the techniques in some detail. Thus, the main two additions to the literature are the use of the SVD approach with IASI measurements and its applications to the PCC eruption and any inferences drawn.

Because of these two potentially new and interesting aspects of the paper I can recommend that it be published, but I would request that the authors consider the points below in a revision.

Major comments.

1. The SVD methodology is not explained well in this paper - at least I had difficulty understanding the details. Since this has already been published I would recommend that the authors consider a much more concise explanation giving only the idea of the method, its advantages and pitfalls. Presumably, the finer details can be found in their earlier papers. Also, the authors should perhaps explain the equivalence of SVD and Principle Components Analysis (PCA) which is widely used in studies involving high spectral resolution IR measurements.

2. There is little or no discussion of the effect of size distribution in this method. This is rather crucial.

3. Equation (2) and the discussion about the complex dielectric constant seems out of place. It is well known that ash particles (or more likely ash aggregates) are non-spherical but the received wisdom is that errors due to the effects of particle shape are much less than those due to inaccurate knowledge of the refractive indices of ash or

those due to lack of information on the size distribution. This makes the discussion in the paper unbalanced.

4. It has been established that the main component of ash affecting the differential absorption and scattering of IR radiation is due to the silicate content. The list of minerals given in Table 1 are not the same as those used by previous authors and at least some explanation is needed. Perhaps, these are better but the authors need to provide reasons and evidence if they want to introduce a different set of mineral components to those previously used.

5. Gangale et al (2010) is not referenced and has shown that the retrieval of ash microphysics from high spectral resolution IR measurements (AIRS) depends on optical depth, composition and effective particle size (radius). The authors do not discuss the effect of particle size on their retrievals.

6. There is little discussion about the validation of their retrievals. Admittedly this is challenging but there are sufficient cases now published (e.g. see the ACP and JGR special issues on Eyjafjallajökull) that a side-by-side comparison could have been done. The source-receptor results for the emission heights could have been validated against CALIOP data. The authors might find some good corroboration if they do this. Finally, although it is true that there is not much published on PCC, several researchers have looked at this eruption using satellite (and other) techniques and I believe that many satellite examples have been made available on the internet. A comparison between, for example, MODIS, OMI, AIRS, MSG/GOES/GMS could have been made.

Minor comments.

7. Pg. 4251, Line 1. A reference is needed for the statement that more than 90 aircraft have been damaged.

8. Pg. 4251, Line 4. The eruption started in March 2010 and the most severe airspace closures happened in April.

9. Pg. 4251, Line 9. It is not clear what this paragraph is trying to say. SO2 is used as a proxy but IR data are used operationally to detect volcanic ash. So if the sense is that "mainly SO2 observations are used to detect ash" then that is incorrect.

10. Pg. 4251, Line 23. Prata and Bernardo (2009) is not relevant here.

11. Pg. 4252, Line 3-4. The ashfall was in Chile and Argentina, in the British Overseas Territory of the Falkland Islands, and also over the south Atlantic. Is this widespread? It is doubtful that any ash actually fell over Australia, New Zealand or South Africa (which also experienced airspace closures).

12. Pg. 4252, Section 2. I think more discussion on the use of IASI for volcanic studies is needed here. The researchers at ULB have published many papers on this subject and their work deserves more detailed discussion.

13. Pg. 4253, Line 8. Use of the word "kindly" is nice but inappropriate.

14. Pg. 4254, Line 7. Sentence starting "By the projection..." Sloppy sentence that makes the meaning incomprehensible.

15. Pg. 4255. Line 5. I think I would disagree with this statement. It is also speculative and there is nothing in this paper to support such a statement (of course, it could still be true).

16. Pg. 4255, Line 22. The size parameter is usually dimensionless, being a ratio of some characteristic particle length and the wavelength of light.

17. Pg. 4256. Line 8 onwards. The authors should at least review the ash minerals used by other authors (e.g. Prata, 1989; Wen and Rose, 1996; Clarisse et al., 2010; Prata and Prata, 2012 among others).

18. Pg. 4256, Line 27. Many authors assume the density of ash to be that of basalt or andesite or rhyolite (all well-known volcanic rocks) - how do the authors' densities compare to the densities (DRE) of these volcanic rocks?

19. Pg. 4257, Line 12. Sentence starting "Retrieved ice cloud ..." does not make sense.

20. Pg. 4258, Line 7. Sentence starting "If the .." needs re-writing.

21. Pg. 4260, Line 5. "up to" or "over" not both.

22. Pg. 4266. Line 15. The authors should refer to the work of Gangale et al. (2010) here. Particle size (and size distribution) as well as composition certainly have a controlling influence on IR spectral AOD. It is not clear that iron content for volcanic ash is so important and this paper has not demonstrated that either.

23. Pg. 4267, Line 4. A reference is needed here. I strongly suggest the authors look at CALIOP data as this might re-enforce their results.

24. Pg. 4267, Line 14. I think some independent (not IASI) observations are available now and the paper would be improved if some of this information were included.

Additional references:

Clarisse et al., 2010. Appl. Opt., 49, 3713-3722.

Gangale et al., 2010. Rem. Sensing Environ., 114, 414-425.

Prata, 1989. GRL, 16, 1293-1296.

Prata and Prata, JGR (Atmospheres), 117, D00U23, doi:10.1029/2011JD016800

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