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Atmospheric Measurement Techniques Discussions

Interactive comment on "The identification and tracking of volcanic ash using the Meteosat Second Generation (MSG) Spinning Enhanced Visible and Infra-Red Imager (SEVIRI)" *by* A. R. Naeger and S. A. Christopher

Anonymous Referee #2

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General Comments:

The paper presents an interesting extension to conventional ash detection methods by including textural and temporal threshold tests. The method is very complex due to the large number of tests and at times difficult to follow. Much of the discussion is based on sampling four areas (clear ocean, water cloud, ice cloud and ash cloud) from a single image and discussing the spectral, spatial and temporal properties of the one sample. The thresholds derived and conclusions would be much more robust and convincing

had a large number of samples been used from a variety of cases. I see this as a major flaw in the results and conclusions presented.

A major limitation of this method is that it can only be used in daytime. This is a particular problem for high latitude eruptions in the winter where there are few daylight hours. Given the operational need for constant ash observations I think that this limitation needs to be highlighted and options for nighttime ash detection presented. There is the assumption, when comparing the SEVIRI detection with the validation data, that areas of elevated AOT are expected to correspond with areas of ash (as detected by SEVIRI), however other forms of aerosol may be present that would not be detected by SEVIRI. There needs to be a careful distinction between ash aerosol and other aerosol in the paper.

Methods of ash detection have been established over many years and it would have been interesting to compare (or at least discuss) the method and results presented here with those established by Francis et al. (2012) or Pavolonis et al. (2006). I found the paper quite long and repetitive in places. I think it is unnecessary to repeat the contents of the image captions in the main text.

In reading the paper I lost the reason for carrying out the three separate classification steps – what is the point in having a separate "feature tests" and "initial cloud tests"? Can these not be done in one step? There is a further cloud detection step, so the feature tests also contain clouds. The strategy for this approach could be explained more clearly.

Finally, the paper presents some interesting ideas about using temporal information and I think it would have been interesting to focus on this a little more, e.g. show a series of 3 pixels for different situations. The paper would also be more valid for operational situations in a VAAC if it used past data rather than temporal information or reference information before and after the time in question – this leads to a delay in the resulting classification of the image.

Specific comments

1. p. 5578, line 25: How can these references classify the size of the 2010 Eyjafjallajökull eruption in 2004 and 2009? Suggest this needs a different reference or re-wording.

2. p. 5579, line 6: Be good to have a reference for the 1 billion US dollars losses. The following sentence ("Also, the damaging effects of . . ..") needs re-wording because it is why airspace was closed not an additional effect.

3. p. 5580, line 2: I would suggest that the common tool for mapping ash is the use of the BT10.8 – BT12.0 method rather than RGB images. RGB images are often used to subjectively view ash dispersion in animations, but not to map ash. (Also use lower-case in red, green, blue)

4. p. 5587, lines 10-12: By using a two-week period surrounding the time of interest makes this method invalid for use in an operational situation where image classification would need to be done in near-real time. If the purpose of this method is for use in VAACs then this needs to be changed; if not the purpose is of the ash detection method needs to be stated (post-event analysis?).

5. p. 5587, lines 15-17: Does the highest BT10.8 value always correspond to the minimum 0.6 um reflectance? I wouldn't have thought this was correct. Surely, hot desert surfaces have high 0.6 um reflectance – perhaps I have miss-understood this point.

6. p. 5588, lines 5-7: Again, this cannot be done in near real-time – limitation in application of the method.

7. p. 5588, line 14 (and many places elsewhere): What type of RGB is this? There are many ways of creating a RGB. Is it the traditionally used "dust" RGB as developed by EUMETSAT? Please can you explain what the RGB is and how it's created (i.e. what channels).

8. p. 5588, lines 27-29: I would question whether the majority of the ash particles were smaller than 1.6 um. There are limitations on the upper size that can be measured by aircraft instrumentation, which means that many large ash particles get missed. Much of the ash mass is contained in the ash particles beyond the viewing capability of infrared sensors. However, in terms of number there are probably large numbers of small particles even if the bulk of the mass is in the large (> 10 um) particles.

9. p. 5589, lines 5-6 (and elsewhere): The inclusion of the solar component in the 3.9 um channel makes the data difficult to use; the 3.9 um data value will vary enormously with viewing and solar angles as well as atmospheric content. I would think that a wider study was needed looking at the 3.9 um value over several days, times and situations to gather any conclusions from the use of this channel. Normally, this channel is only used at night, but to the problems in its use during the day.

10. p. 5589, lines 20-21: doesn't this test also detect clear areas that usually have BTD 10.8 – 12.0 of around 0.0-0.5K?

11. p. 5590, line 6: Wouldn't the test BT10.8 < 240 K also detect high altitude ash cloud (particularly thick ash cloud)? Suggest that you also write "BT10.8" or similar rather than "10.8 um".

12. p. 5590, line 15: The BTD 3.9 – 10.8 must vary enormously with solar and viewing angles. Is this threshold robust in other viewing geometries and times of day?

13. p. 5592, lines 25-29: Do you assume that you have the same feature in the same pixel in three successive images? If so, this must place limitations on detecting ash cloud edges and small areas of ash – please discuss this and the consequences of your assumption.

14. p. 5595, line 1: How many case studies? This is key – were a large number of cases analysed to derive all thresholds or most derived from the samples in the single image shown in Fig 1?

15. p. 5595, line 20: Is it good that the method performed well for dust cases? Are you developing a volcanic ash detection method, an ash + mineral dust detection method or an aerosol detection method? Mineral dust was not included in your original samples from Fig 1. Surely you want to be able to identify ash from other aerosols if it is to be useful in VAACs. Please can you clarify the purpose here? If mineral dust (or other aerosol) can be miss-classified as volcanic ash then this should be discussed.

16. p. 5598, line 6: Do these areas contain thicker ash? Perhaps it is more concentrated or the ash particles are smaller? What evidence is there?

17. p. 5598, line 27 (and in following text): Confusing ash with aerosol again – the aerosol may not be ash or are you trying to detect all aerosol?

18. p. 5602, line 13: Is it the intention to detect both dust and ash? Please make this clear and the consequences of the joint ash/dust detection.

19. Figure 1: What type of RGB is this? It does not appear the highlight the ash very well – looks the same as low cloud and clear sky. Is there a better RGB you could use? e.g. Dust RGB (see Francis et al, 2012 for a description: http://onlinelibrary.wiley.com/doi/10.1029/2011JD016788/abstract).

20. Figure 2: These values are hugely dependant on cloud height (not mentioned), viewing angles etc. Dangerous to rely on one sample for each type.

21. Figure 3: The BT8.7 – BT10.8 will vary with ash composition and SO2 amount in the volcanic cloud. I think that the effect of SO2 on the 8.7 um BT should be discussed in the text. The BT10.8 – BT12.0 will also vary with ash composition, particle size, atmospheric water vapour content as well as ash concentration: these points should be mentioned in the text.

22. Figure 4 and 5: The type of RGB needs to be described and a key shown for the MODIS AOD. The images are also quite squashed – is it possible to increase the height of the individual images?

23. Figure 6: Nice plot showing the high AOD with BTD10.8 – BT12.0. Worth mentioning in text that you are comparing a point measurement with pixel averages.

Technical corrections

1. p. 5578, line 7: Suggest add “also”. e.g. “. . . to May but also a . . .”

2. p. 5578, line 10: Suggest add “from”, e.g. “. . . including from the Moderate . . .”

3. p. 5580, line 23: Suggest write “fewer” rather than “less”

4. p. 5581, line 20: Suggest use “are” or “were” rather than “was” when referring to the aircraft measurements.

5. p. 5581, line 23: Change “. . . include the . . .” to “. . . include data from . . .”

6. p. 5587, line 4: “inputs” rather than “input”

7. p. 5599: Need a reference for the FAAM data and flight paths.

8. p. 5601, line 25: I think you mean “< 0.1” rather than “> 0.1”.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 5577, 2013.