Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-689-RC2, 2016
© Author(s) 2016. CC-BY 3.0 License.

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying the mass loading of particles in an ash cloud remobilised from tephra deposits on Iceland" *by* Frances Beckett et al.

M. Watson (Referee)

matt.watson@bristol.ac.uk

Received and published: 15 December 2016

Review of Beckett et al., ACP. Matthew Watson, Bristol, December 2016

Overview: This paper details satellite observations (VIIRS) and dispersion modelling (NAME) of re-suspended volcanic ash from Iceland during an event in September 2013. It is well written, with very few editorial issues, appears well referenced and does, I think, provide incremental improvement in our understanding of the phenomenon. There are however some serious issues with the paper which will require revisions before the paper can be published.

Major: The per-pixel mass loadings from VIIRS can be worked up into total mass. This

Printer-friendly version

Discussion paper

is not discussed in the text in although appears to have been done (in Table 2, which is also only fleetingly discussed). Are those numbers (on order 10 Gg) comparable with the NAME estimates of 0.2 Tg? Why is there a five order of magnitude difference? It doesn't appear that you can even get to 10 Gg from summing the column loadings in figure 10. (A quick back of envelope calculations using an area 2E9 m2 (much larger than the observed clouds) and the max observed loading gives 6E9 g (i.e. 6 Tg)? This may simple be a typo, but needs resolving.

Following from that, the whole process appears rather circular. It's a somewhat tortuous process to go from column loading, to emission rate, to mass from the NAME model when the VIIRS observations tell you that directly? This entire section is quite confusing (section 4), for example it's not clear what 'un-calibrated really means'. NAME must have been run with some starting conditions with some given units (even if this is unity). Also, the use of the scaling factor is poorly defined (and has a significant impact on the final mass). It appears to be derived from the difference in masses between the column burdens derived from VIIRS and the uncalibrated NAME runs. This requires significant expansion.

The section on water vapour, whilst technically correct, is completely undermined by the final section of 3.1.1 where the discussion grinds to the halt as it is explained that the water vapour correction was not applied. Recast this section to explain what was done (in more detail) rather than a more complex explanation of something that wasn't.

Why does the BTD signal in Figure 8 get stronger at lower mass (for constant particle size). That is opposite to what every paper I've ever read on the subject would suggest.

There may be other reasons for positive BTD. The authors should probably approach them, and rule them out (especially coating of the ash and/or mixing with ice). I accept that this is unlikely but there are precedents in Iceland, though not from re-suspended ash.

Leadbetter et al., 2012 proposed a range of 0.4 – 0.5 for U*t. What difference would

CC BY

using 0.5 make (i.e. how sensitive to the final outcome is that choice)?

What effect does limiting the NAME 1-10 microns have? Given much larger (and more mass bearing) particles have been observed (and discussed later in the paper) this is something the authors will need to further explore.

In summary there are some quite unfathomable things in the paper. I would encourage the authors to work through these and provide explanations. It could be I've simply misunderstood but even that would imply a lack of clarity in the paper.

Editorial (very minor): P1 L19 should be ':' not ';' P2 L10 'random walk' might need further explanation / reference P4 L1-5. Reasons for mass loadings are presented but do not mention reduced availability. This is then discussed later in the paper, foreshadow that discussion, briefly here (it seems to be to be a perfectly reasonable explanation, as, of course, does the location of the OPC) P4 L19 Maybe quote the calculate dBTD cost from water vapour here (for the purists). P7 L1 This is clumsy. Do you mean your doubled the concentration of the lowest layer to preserve constant mass? P11 L3 cite 'Mackie, S., Millington, S. and Watson, I.M., 2014. How assumed composition affects the interpretation of satellite observations of volcanic ash. Meteorological Applications, 21(1), pp.20-29.'? P12 L16 Chronologise reference list Figures look good in colour but are unusable in black and white (no change required unless the paper won't be published in colour)

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-689, 2016.

CC BY