Atmos. Chem. Phys. Discuss., 3, S717–S723, 2003
www.atmos-chem-phys.org/acpd/3/S717/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric impact of the 1783-1784 Laki Eruption: Part II Climatic effect of sulphate aerosol" *by* E.J. Highwood and D.S. Stevenson

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Received and published: 3 June 2003

Atmospheric impact of the 1783-1784 Laki Eruption: Part II Climatic effect of sulphate aerosol; Response to reviewer 1

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This response to the comments made by the anonymous referree are made on behalf of both authors. We thank the reviewer for their helpful comments. Many of the comments and queries have been resolved by clarifying the text of the paper and including additional references. We have made new calculations in response to a few points, and altered some of the figures for clarity. Our responses are given below in considerably more detail.

Response to Referee 1 specific comments

1. Although the authors fully acknowledge that they have not simulated the Laki Craters eruption effects in later sections of the manuscript, in the Title and Introduction, the reader could fail to recognise that the modelling does not take account of contemporary meteorology, and is perhaps better described as a hypothetical scenario for a Laki-like eruption. This does not lessen the value of the manuscript but there is a question of emphasis that could be tackled more directly. For instance, the Title could read something like "Simulation of climate impact of a large effusive eruption: implications for the Laki Craters eruption, 1783-84)"

The reviewer suggests a good alternative title for this paper. Although we have made it clear in the paper that the neglect of contemporary meteorology means the results are not a direct comparison for the 1783-84 eruption, that eruption, and the debate surrounding it's atmospheric impact was our motivation for this research and we believe that it is of value to compare our results against this eruption. Since the companion paper, Part 1 (Stevenson et al, 2003) has already been published in the print version of ACP, I do not feel it is appropriate to change the title of Part 2 at this point. However we have added text to the abstract and introduction to re-inforce the idea of a "Laki-like" eruption, with the specific example of the 1783-84 eruption for some validation.

2. In the abstract, is a peak...mean.. anomaly what the authors intend to say?

It is not clear exactly what the reviewer is referring to here. In the abstract we mention the "peak Northern Hemisphere mean direct radiative forcing is $-5.5Wm^{-2}$" and the "the magnitude of the Northern hemisphere annual mean anomaly for 1783 is -0.21K." The first point is correct. The second phrase is admittedly rather clumsy and has been re-worded to be clearer.

3. In the Introduction, mention could be made of the release of H2S in some volcanic eruptions in comparable or even superior quantities to SO2.

In some eruptions H2S is indeed released. Since this is oxidised rapidly to SO2, H2S can be thought of as an additional source of SO2. In addition, emission data for H2S is less well constrained than that for SO2. Andres and Kasgnoc (1998) mention the "global" volcanic output of H2S, and suggest it is about 20% of the SO2 amount, but this is extrapolated from measurements at a sole volcano! Seinfeld and Pandis (1998) suggest that H2S has a lifetime of around 70 hours, longer than SO2 but still short compared to the timescales on which we are considering radiative forcing and climate impact. We suggest that if we put 20% of Laki emissions as H2S with this lifetime it might make the SO2 a little more dispersed but won't make much difference to the sulphate or forcings. It would make a difference to air quality, however this is beyond the scope of this paper.

4. Also in the introduction, the discussion of the aerosol effect as a greenhouse gas could be elaborated in terms of particle size. I was under the impression that, in general terms, the GH effect becomes important for aerosol radii above 2 micron

We do point out the importance of size with respect to the greenhouse effect of aerosol in the Introduction and include this effect in our simulation. It also depends on the temperature (and therefore vertical location of the aerosol), as is also discussed in the introduction. Our simulations show the greenhouse effect of the Laki aerosol to be generally much smaller than the shortwave effect. Even for previous studies of the Pinatubo aerosol (larger and in the stratosphere), the LW effect was found to be only 10% of the shortwave effect, as stated in the paper. Although it does depend on the size of the aerosol particle, over the range of realistic sizes for volcanic aerosol used here, the longwave extinction co-efficient does not vary greatly.

5. A general point: dry fog has been used confusingly in the earlier literature on Laki and other volcanic eruptions, referring both to tropospheric and stratospheric aerosol. Whenever known, the nature of such fogs should be qualified.

We wholeheartedly agree and found that in general the earlier literature was not helpful in indicating the extent of stratospheric and tropospheric components. A sentence has been added to the Introduction (page 1603, line 13) to emphasis the uncertainty surrounding this issue.

6. The authors could mention the Central England temperature dataset which records July 1783 as the hottest July in the record prior to 1981 (I think).

This is indeed true, the CET being $18.8°$C, although 1976 and 1852 (both 18.7) come close and we have added a sentence documenting that. However, we should be wary about linking this directly to the volcanic eruption as there is considerably evidence of anomalous southerly flow bringing warm air across the U.K. as we discuss in section 4.2 of the paper. Virtually the same temperature occurs in several other years throughout the 1700s and 1800s.

7. In section 2, the Mount St. Helens (18 May) eruption is described as mainly tropospheric. Is this the case? It is certainly nothing like the Laki eruption in character; the mushroom cloud reached about 25km, and TOMS picked up 1 Tg if SO2 which I presume was mostly in the stratosphere

The e-folding time reported for Mount St. Helens in Jager and Carnuth (1987) is 3.6 months, much shorter than other eruptions like Pinatubo, suggesting that this eruption put most of its aerosol into the upper troposphere and lower stratosphere. The main blast was lateral and the emissions were relatively sulphur poor. This combination of factors led to no discernible influence on hemispheric or global mean temperature. While we recognise that this is a different type of eruption from that of Laki, if Laki did put material into the upper troposphere and lower stratosphere as suggested by some sources, it is likely that the e-folding time would be similar. Text added to clarify.

8. On page 4, comparison of forcing results with Pinatubo is made. These are not entirely straightforward since the Pinatubo forcings derived from ERBE data (i.e. real measurements that include direct and indirect effects), and are also presumably minimum global forcings because the instrument did not operate polewards of about 40 degrees.

We compare to both the forcings derived from ERBE data which have the caveats mentioned by the referee but also with model simulations using observations of stratospheric optical depth and calculating the true global mean forcing as reaching a peak of around 4 Wm^{-2} (Hansen et al, 1992, Stenchikov, 1998). The point that we emphasise in the paper is the difference in duration of event as well as in the peak.

9. Clearly much of the modelling results depends on the aerosol size distri-

bution and selected optical properties. Some discussion of the sensitivities to these parameters is provided. I was quite surprised at the 0.05 micron mean size chosen for the tropospheric aerosol, though this is balanced with a wide distribution. Where does this put the effective radius of the aerosol? I have always wondered whether the tropospheric aerosol produced could be coagulating relatively close to source so as to produce a greenhouse effect that might provide an explanation of the high July temperature observations in various parts of Europe. I would be interested to see some additional discussion of this possible effect though realise this may be beyond the scope of the present work.

Firstly, it is simplistic to assume that a local positive radiative forcing (or greenhouse gas effect) due to aerosol will necessarily lead to a positive temperature anomaly at that location; it can be seen that the climate response to the localised negative forcing of the aerosol is spatially variable although there appears to be some spatial correspondance between forcing and temperature anomaly during summer 1783. The effective radius for the tropospheric aerosol is 0.12 microns; this size was chosen as being similar to tropospheric sulphates formed from industrial pollution. There is little if any data available concerning the coagulation of tropospheric volcanic aerosols. The greenhouse effect of mainly tropospheric aerosols is expected to be small, even if they are large, since the temperature difference between these aerosols and the surface will be relatively small. Even if the size of the aerosol is increased to the same size as that in the stratosphere the effect is actually to increase the magnitude of negative radiative forcing by less than 5to give a large greenhouse effect. In the context of the other uncertainties discussed in the paper, this sensitivity to size is small.

We thank the reviewer for drawing our attention to the typos, legends and text in figures.

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