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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of the 1783–1784 AD Laki eruption on global aerosol formation processes and cloud condensation nuclei" *by* A. Schmidt et al.

A. Schmidt et al.

ear5as@leeds.ac.uk

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We would like to thank reviewer 2 (Hans-F. Graf) for his helpful comments.

Below are the questions/comments raised by the referee (shown in bold) and our response to each of questions/comments.

Unfortunately, the model system works with monthly prescribed cloud fields and this undermines the relevance of the results substantially since the important feedbacks between cloud microphysics, cloud cover and the additional aerosols produced by the Laki emissions cannot be simulated. Anyway, the work clearly shows the importance of these processes and may count as an important step

in the right direction.

We agree with the referee that our chosen model framework cannot account for feedbacks between the additional aerosol loading and cloud cover; however, our study shows substantial first order effects (i.e. effects that are initially unaffected by any potential feedbacks) on cloud condensation nuclei (CCN). First order effects on CCN have not been taken into account in any previous Laki study and the data set provided will be available to other modelling groups that might wish to study what we think will be second order feedback effects on CCN. It is important to separate the effect of the CCN changes on climate and atmospheric circulation (which may be important as shown by Nober et al., 2003) from the subsequent feedback of dynamics-induced changes on the CCN itself. We expect this feedback on CCN will be quite small but agree that it does need to be calculated in an aerosol-chemistry-climate model. In summary, our study aimed to quantify the first order effect and we are the first study to show that this effect is significant.

As with all chemistry transport models the problem is that circulation (and clouds!) are prescribed.

One advantage of prescribing transport and clouds is that first order effects can be isolated, as we did in our study. The next stage would be to calculate responses of other components of the climate system. As we said above, we don't think the subsequent effects of dynamical changes in CCN changes will be very important.

The authors only present results for the simulations driven by 2003 reanalysis. Only at one point (deposition over Greenland) they briefly mention that using year 2000 reanalysis leads to somewhat different results. The prescribed weather conditions (Certainly massively different from the time of the Laki eruption, but what are the similarities and what the differences?) lead me to suggest to talk about a "Laki-type" eruption rather than a "Laki" eruption.

We agree with the referee that there is no way of replicating the meteorology of 1783-

1784 AD. As already explained in Section 2.1, we chose to force the model with ECMWF reanalyses for the year 2003 as this year features similarities, such as the presence of persistent anticyclones over Western and Central Europe during summer, to what is proposed about the meteorological situation in summer 1783 (see e.g. Thordarson and Self, 2003 and references therein).

We added a sentence at the end of the first paragraph of section 2.1 following the referee's comment that our simulations are more representative of a "Laki-type eruption". None of the previous studies can claim to reproduce the meteorology of 1783, however all used the term "Laki eruption" in their titles so we would like to stick to that title. Overall, similar to all previous studies we do simulate the effect of well characterised emissions, so using the term Laki eruption in the title seems valid to us.

Some more discussion of differences/variability when other years are used to drive the transport model would be appreciated.

Of course, the spatial distribution of CN and CCN changes will differ somewhat when forcing the model with different ECMWF reanalyses, however the impact on monthly mean CN and CCN is of the same magnitude in a different year. In order to prove this we further analysed our Laki simulation forced with year 2000 ECMWF fields and found that, on climatological relevant time-scales, the results are very similar for both, CN and CCN concentrations (which are the main scope of our paper).

For example, June-July-August (JJA) zonal mean CCN number concentrations increase by a factor 74 in the upper troposphere (corresponding to maximum changes of 1470 CCN per cc) when using reanalyses for the year 2000 (this compares to a factor 65, corresponding to maximum changes of 1430 CCN per cc for the year 2003). JJA Northern Hemisphere mean CCN concentrations at low-level cloud altitude reach 270 CCN per cc during the year 2003 and 290 CCN per cc during the year 2000. Both, the year 2003 and the year 2000 simulation show significant increases in CCN number concentrations far away from the eruption site (e.g. North America factor 10 for year

2003, factor 11 for year 2000; Europe factor 6 for year 2003, factor 5.4 for year 2000; Asia factor 14 for year 2003, factor 12 for year 2000).

P3195.25 the authors talk about "“stratospheric volcanism" – this is not existent, some volcanoes just inject material into the stratosphere.

Fully agree. We changed that sentence.

There is no way to "evaluate" the model results against other models since it is not known what is "true".

Fully agree. We changed that sentence.

P3196.21 what is meant by "volcanological point of view"?

In order to clarify our point we changed that sentence.

P3200 1ff discuss the model differences in terms of different processes included to make clear where GLOMAP might be superior.

In contrast to all previous studies we simulate (and budget) aerosol microphysical processes. Moreover, we do not prescribe size distribution; Timmreck et al. (2009) simulated the 1258 AD eruption using an Earth System Model and concluded that the temporal evolution of the aerosol size distribution needs to be accounted for when assessing the climatic response of such an eruption. Moreover, in contrast to previous studies we are using a two-moment model aerosol scheme thus we account for both, number concentrations and particle mass. All previous Laki modelling studies relied on the mass of sulphate when calculating the direct radiative forcing. Our dataset can be used to calculate cloud drop number concentrations (and subsequently, the first aerosol indirect effect induced by a Laki-style eruption) using a physically based cloud drop activation scheme. Furthermore, in sections 3.5.1 and 3.5.2 we show the importance of tracking particle mass and number concentrations as our simulations indicate a widespread impact in regions far away from the source.

We amended section 3.3.1 adding a table (comparing our results with previous studies) and explaining where GLOMAP might be superior when compared with previous modelling studies.

9 what is meant by "normalized bias"?

The normalised mean bias is calculated as follows:

$$\frac{\sum_{i=1}^{N}(Mi - Oi)}{\sum_{i=1}^{N} Oi}$$

where Mi represent the modelled values and Oi the observed values.

We amended the sentence referring to Mann et al. (2010) where details such as the normalised mean bias can be found.

Ch 3.3.2: There seems to be much enhanced deposition of S over Greenland, using another year does not help since a factor of 4 is not in the range of model variability. Confidence cannot be gained from modern simulations since as shown chemical pathways are different. Such a high deposition bias severely raises doubts about the usefulness of the model. Here some more discussion is necessary to convince the reader that the model results are useful.

We disagree with the referee as we think that confidence can be gained from comparing the modern-day model simulations to observations (without that, no model prediction would be valuable).

We believe the high deposition bias over Greenland is primarily due to the meteorology used to force the model not being the exact 1783 AD meteorology. Comparing modelled deposition values to Greenland ice-core measurements is certainly one method of testing the model, however, we believe that one would have to force the model with the 1783 AD meteorology in order to make that test conclusive. Stevenson et al.

(2003) already discussed further uncertainties that arise when modelling the deposition to snow. Moreover, a modern analogue of the "meteorology-issue" is the recent eruption at Eyjafjallajökull which occurred during an anomalous meteorological situation and transported volcanic ash over the UK. We simply don't know for sure whether something similar happened during Laki. We would like to note that apart from a recent model evaluation paper by Mann et al. (2010), GLOMAP-mode results have been submitted to the A2-CTRL-2006 control experiment for AEROCOM and the performance of the model against a range of observations can be explored via the AEROCOM webpage for instance against sulphate deposition at:

http://dataipsl.ipsl.jussieu.fr/cgi-bin/AEROCOM/aerocom/surfobs_annualrs.pl

We amended section 3.3.2 accordingly.

We agree with the referee that in our paper some more discussion is necessary, thus we added Table 1 comparing our results to previous modelling studies by Stevenson et al. (2003) and Oman et al. (2006a) and literature-based estimates of several diagnostics. Moreover, we expanded the discussion on this issue under section 3.3.1.

We would like to stress that it is not our aim to simulate point by point accurate aerosol distributions but to calculate the impact on global CCN. Moreover, the high aerosol loading might have suppressed precipitation during 1783 – we cannot account for this feedback, however it might explain the high depostion bias over Greenland.

Despite the high deposition bias we have confidence in our results as other important diagnostics (see Table 1 which has been added under section 3.3.1) compare well to previous modelling studies. For example, our model produces a similar amount of total volcanic sulphate aerosol as Oman et al. (2006a); however our model simulates a lower sulphate burden and a shorter sulphate residence time compared to the Oman et al. (2006a) study. However, both the sulphate residence time and the sulphate burden are comparable with the Stevenson et al. (2003) study. These findings suggest that either, we remove sulphate too quickly (when compared to Oman et al., 2006a) or Oman et al.

(2006a) initialise their model simulation using too small an effective radius. In summary Table 1 shows that key diagnostics such as burden, yield and SO_2 residence times are within the range of uncertainties that arise when comparing different models with each other (e.g. Textor et al., 2006). Uncertainties arise from different parameterisations of certain processes such as wet removal.

Ch 3.4 Here some thoughts on the radiative effects of the changes in aerosol concentration and spectra would be appreciated.

We are currently working on a paper describing the impact on cloud radiation (as already mentioned in Conclusion). We amended the sentence at the end of the paper to make this clearer.

Additional References:

Nober, F. J., Graf, H.-F., and Rosenfeld, D.: Sensitivity of the global circulation to the suppression of precipitation by anthropogenic aerosols, Global Planetary Change, 37, 57–80, 2003.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 3189, 2010.