

Interactive comment on “Atmospheric impact of the 1783-1784 Laki eruption: Part I Chemistry modelling” by D. S. Stevenson et al.

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General comments:

The paper presents a CTM study of the 1783-1784 Laki fissure eruption which was one of the biggest tropospheric pollution events of the last 250 years. There is not much known about the atmospheric effects of a high latitude eruption so the paper contains original and new material and gives valuable information for atmospheric scientists and geologists.

This paper focuses on the chemistry effects of the eruption. An accompanying paper (Highwood and Stevenson in prep) treats the radiative forcing and the climate impact. The authors show that the lifetime of SO₂ is prolonged due to a significant depletion of OH and H₂O₂ so that a large fraction of the SO₂ will be deposited before it will be

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oxidized. The authors suggest that previous estimates of the global aerosol loading associated with Laki are too large in magnitude.

The manuscript is well organized and clearly presented and the subject is well suited for ACP. I recommend publication in ACP after taking the following comments into account.

Specific comments:

1) monthly mean emission fields

The authors are using monthly mean volcanic SO₂ emissions as an approximation instead of short episodic injections. They discuss in the paper that this might not have an large effect on the monthly and seasonal results because of the probably sufficient long lifetimes of SO₂ and SO₄.

I am not completely convinced by the discussion of the authors that this will not alter the results so I would like to see a more detailed discussion of this point. An important aspect of the paper is the depletion of OH and H₂O₂ oxidants due to the higher level of SO₂. Would this effect not even be stronger if one use the episodic data instead of monthly mean values ? Is the effect linear or not ? I suggest to simulate the first three months of the Laki eruption not with monthly mean values but with pulses of SO₂ injection to get an idea if it is relevant or not and how big are the uncertainties.

2) aerosol microphysics or bulk approach

Do you consider an aerosol size distribution or do you treat the sulphate aerosol with a bulk approach ? You listed the gas phase reaction to H₂SO₄ in table 1. How do you treat the conversion from sulphuric acid vapor to sulphate? Do you explicitly calculate microphysical processes such as nucleation and condensation ? If so you have to describe it somewhere if not it should be mention under missing feedbacks and other uncertainties

3) H₂O emission

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Thodarson et al (1996) estimated an H₂O emission of 235 MT due to the Laki eruption. Would the volcanic H₂O emission play a role for the lifetime of the oxidants ?

4) Wet and dry deposition

Wet and dry deposition are central points in your study so I would like to see a more detailed discussion of these processes e.g. reference for dry deposition velocities. How does your model behave in comparison to other sulfur cycle models ?

5) sink process for sulphate

What about sedimentation ? Sedimentation from the UT/LS might be relevant

6) figure 6

Figure 6 compares observed surface concentration and values from the lowest model layer, or? Clearly one can see the interannual variability in the SO₂ and SO₄ concentration is high. How large is the variability with respect to the simulated deposition fluxes ?

7) Comparison of model results with ice core data

How important is the different meteorological situation (wind fields, precipitation) in 1783 compared to the one in 1990 for the aerosol deposition in Greenland? Does it influence your results?

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 551, 2003.

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