

Interactive comment on “Tracking far-range air pollution induced by the 2014–15 Bárðarbunga fissure eruption (Iceland)” by Marie Boichu et al.

Anonymous Referee #2

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General Comments

The paper ‘Tracking far-range air pollution induced by the 2014–15 Bárðarbunga fissure eruption (Iceland)’ describes a modelling exercise based on this particular eruption complemented by a large range of measurements. The paper is well written and well-structured and does a good job of highlighting notable and challenging aspects associated with this work, although there are a number of issues relating to the modelling aspect of the work that would need to be addressed before publication.

Specific comments

Discrepancies between models and observations are discussed and a number of reasons have been assigned to this. Possible explanations for these differences include:

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Flux emission and altitude of injection 'This discrepancy can result from a limited knowledge of SO₂ emission parameters (flux and altitude of injection) which initialize the chemistry-transport model.' It is also stated that 'Inception time and altitude of emissions are found by trial and error so as to reproduce first-order features of satellite and ground-level SO₂ observations'. As the authors state model inversions can help with the refinement of this source term and help to further understand this discrepancy. This is clearly outside the scope of the work presented here although other possible reasons for the discrepancy may warrant further clarification. I think a more full discussion of SO₂ oxidation and its possible contribution of the discrepancy should be included (after all it is a CTM). It is stated 'However, the conversion of SO₂ to sulphate aerosols is not implemented in this study to avoid uncontrolled influence of uncertainties on the numerous factors governing this process in a volcanic cloud'. This is a reasonable approach although ground based measurements of sulphate aerosols suggest a fairly significant conversion which is not reflected in the source term. The inclusion of these interactions in future model iterations would clearly represent an improvement. Observations of the boundary layer heights compared to model simulations show a very large underestimation with the largest differences being observed at night time. The authors suggest that this is a ubiquitous feature of WRF. I would recommend confirming the influence of the boundary layer parameterisations by running WRF simulations using a number of parameterisations. This would confirm the influence of boundary layer height on the results presented here and may help to understand its contribution to model/observation mismatch. It is suggested that higher model resolution (temporal and spatial) may help elucidate further the source of observation/model differences and this has both further time and computational costs. This is a perfectly reasonable argument. However I do not think it would be not beyond the scope of this study to perform some test simulations at a higher resolution in order to shed light on this point.

In short I would suggest that perhaps a small effort in performing some simulations using a selection of boundary later parameterisations in WRF. Higher resolution simulations, if possible, would also help to strengthen (or at least clarify) some of the ideas

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presented here. A more complete discussion of the SO₂ oxidation should be also included. Exploring some other locations to confirm the model performance in other regions and add more credence to discussion and conclusions should be considered. Perhaps the authors might outline a possible framework for a set of simulations that might elucidate these uncertainties. The conclusion reiterates the issue surrounding the boundary layer in the model but this should be contextualised within the framework of the other possible reasons for model-observation mismatch.

Technical corrections

Page 1 Line 1 'has emitted'- is 'has' necessary?

Page 1 Line 3 'chemistry – transport' –model should be included after this for clarification

Page 2 Line 13 'triggered a volcanogenic air pollution unprecedented'. Either 'a' should be removed or a descriptor after 'air pollution' should be included.

Page 4 Line 10 Do you need three references from the same author here?

Page 4 Line 12 This sentence regarding the omission of the SO₂ chemistry could be improved. This will clearly lead to large uncertainties when comparing to SO₂ mixing ratios. The measurements of the sulphate aerosols provide some information regarding the magnitude of the conversion process and should be included here

Page 5 Line 14 What was the spin time up on the WRF simulations?

Section 2.2 Line 24 What is the justification for choosing a Gaussian profile?

Section 3.1 Line 10 perhaps 'hitting' could be replaced with reaching

Figures

Figure 1 – It is hard to see how figure 1 is directly related to the text provided.

Figure 6c- Why might there a time shift between gas and aerosol?

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Figure 9 – What would be an estimate of the uncertainty on the model boundary layer simulation?

Discussion - In the discussion the phrase ‘finding optimum configuration’ is used. This is something that could be undertaken or considered with the boundary layer parameterisation within WRF. This work would certainly strengthen some of the conclusions presented in this work.

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