

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

Anonymous Referee #1

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General comments

The paper by Boichu et al. presents a study on the far-range air pollution caused by the Bárðarbunga fissure eruption. The authors gather an extensive, and very useful, set of various type of measurements to complement and compare with modelling results. The paper is however rather descriptive and additional in depth evaluation of such an interesting dataset would be desirable. The authors encounter several problems in the modelling results that are not tackled. Although it is hard to tackle all of them, I would encourage the authors to suggest a roadmap on how to identify the main factors leading to a poor representation of the ground-level concentrations by the model. In addition, given that they have access to a full chemistry model, it would be worth to try to include SO₂ chemistry in the simulations.

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Given the significance of the event, both in terms of air quality and also on volcanic emission forecasts and potential impacts, the paper shall be revised and considered for publication once the main aspects stated before and below are addressed.

Specific comments

Introduction:

- Page 2 Line 10-11 “ Even so, the Bardarbunga eruption only weakly disturbed air traf- fic...” is unnecessary since aviation implications are not the topic of the paper and they are mostly significant for ash-rich eruptions. If the authors want to still keep the refer- ence to aviation, they may state that “Whereas the Bardarbunga eruption SO₂ emis- sions were very large, but not constant, the ash emissions were limited and therefore no affectation in air traffic occurred, unlike on other occasions such as the Eyjafjalla- jokul eruption”.

- Page 2 Line 12-13 “Nevertheless, Bardarbunga triggered a volcanogenic air pollu- tion unprecedented in Europe. . . which necessitated locally exceptional civil protection measures”. The sentence and posterior reference to Gisalson et al. 2015 (which only addresses the environmental stress in Iceland) is misleading since it reads as if excep- tional civil protection measures were taken also in areas of Europe other than Iceland, please rephrase.

- Page 2 Line 17 “The Bardarbunga cloud travelled most often . . . toward high lati- tudes”. Please add reference, even if it is, for instance, Figure 4 of the manuscript.

- Page 2 Line 18 “peculiar meteorological conditions”. They were not that peculiar given that, for instance, the Eyja event suffered similar transport conditions transporting the ash plume rapidly over mainland Europe. I would suggest “favourable conditions”

- Page 2 Lines 25 onwards until the end of the paragraph “ Here, we use a wealth . . . “ is ambitious given that the paper is so far more descriptive and does not go in depth into the characterization of SO₂, the derived sulfates and the dynamics of the ABL

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leading to such unusual concentrations at ground level. Please revisit this sentence after addressing the comments here presented and rephrase if needed. In addition, although the comparisons are indeed quantitatively, they require further analysis and better description in the text to be stated as it is now.

Methodology:

- Page 3 Line 8 “Given the low injection height” needs a reference.
- Page 3 Line 10 “The center of mass of the SO₂ cloud is assumed to be within the PBL”. What is this important statement based upon?
- Page 3 Line 20 The reference to the figure 1 should be complemented with additional explanation of the figure in the text. How this figure relates to the event the authors are examining? Are they suggesting that these low tropospheric aerosols are partly due to the event? What is the relation of the figure 1 with the topic of the paper?
- Page 3 Line 4 As in the previous comment, reference to figure 2. The figure is presented but all the information one can extract from the figure is not written in the text. Please do so and clearly stated how the figure relates with the influence of the volcanic eruption.
- Page 4 line 10 of “Chemistry-transport model”. The authors state that the conversion from SO₂ to SO₄ is not implemented to avoid uncontrolled influence of uncertainties on the numerous factors governing this process in a volcanic cloud. It is unfortunate that the authors decided not to study the conversions since then the comparison with the aerosol measurements would have been more interesting. Given the characteristics of the eruption, with such a low height emission and transport, the conversions from SO₂ to SO₄ may be significant and one would hope that the CTM would at least reflect part of it. Have the authors at least tried to include the conversions? Given that the authors use a CTM, I would encourage them to add discussions on this and, if possible, an additional test with the conversion activated. Otherwise one may wonder why using

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this model and not something closer to a Lagrangian particle dispersion model.

- Page 5 line 17. WRF can work using different PBL schemes. Is there any reason for using YSU in particular? Where there some sensitivity tests behind that suggested this one to be the one giving the best results? Given that the evolution of the PBL is crucial in this event to understand the ground level concentrations, more details on additional sensitivity tests, if done, would be useful and help understand the influence of this very important parameter in the final ground-level concentrations. Although not all the potential tests should be presented, for the sake of keeping the manuscript short, any insight in significant parameters is valuable.

- Page 5 Line 25 “inception time” what does this mean in this context?

- Page 5 Line 25 onwards: as for what I understand, the authors modified the injection height and times trying to match as much as possible the satellite data keeping a gaussian profile. Did they do this automatically or by simple visual inspection? It would be useful to know. It is also important to note that, the coarse assumptions in the source term make an accurate evaluation difficult. It would be good to highlight this in the conclusions section and state that the aim of the paper was not to make an estimate of the source term but to try to accommodate a simple source term that would represent the main features for this far-range study. A plot with the source term (injection height, times, vertical profiles) used in the modelling would be very useful to accompany figure 4 and would help the reader visualise the simulation.

- I would suggest also more description of Figure 4. For instance, we can clearly see from the derived IASI heights that for lower latitudes the heights are constrained to heights mostly below 8km. In addition, over many areas, example 20/09/2014 UK, the cloud is constrained below approximately 5 km a.s.l which will of course favour potential plume ground-touching.

Results:

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As previously stated, if possible, it would be good to include a simulation that accounts for the SO₂ conversions to sulfates.

- Section 3.1 title, Large scale SO₂ dispersal from Iceland toward Europe does not read nice. I would suggest Large scale transport of SO₂ towards Europe

- When looking at Figure 5, one has at least some doubts about the transport towards the Atlantic ocean of the Wave 1 since OMI show some traces that could actually be wave 1 transported further into mainland Europe. What is the opinion of the authors on this?

- Page 5 line 15-16, have the authors tried to gather data from the Scandinavia region to further assess the model behaviour in this region?

- Figure 6 c: why are the ground-level concentrations of SO₂ and particles de-phased with particle concentrations peaking several hours after the passage of the SO₂ plume? Whereas the text states there is coexistence of SO₂ and sulfates, we see a delay in the peaking particle concentrations. We see this behaviour both for the first and second waves. Also, seeing the plots, it would be good to add a discussion of the PBL evolution and how this is may be influencing the concentrations at ground-level.

- Page 7 line 25 “Interestingly...”. Why are the authors surprised about the two cities following a similar pattern? In sections before, the authors describe the transport patterns by explaining two waves coming towards Europe. This, therefore, makes it evident that the temporal patterns of the two locations may undergo a similar signal pattern. And actually the authors stated this right after the “Interestingly... “ sentence. I would rephrase it and start with “ As observed from space and reproduced by the CTM, two waves. ... This is also seen in the measured ground-level concentrations at ...”

- The authors state that the model fails to represent the second wave. Looking at the magnitude of the model at the first wave I am wondering whether what actually happens is that the model is maybe too fast and representing the second wave too

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early. Do the authors have any comments in this regard? Or, if not, do the authors have any suggestion on why the much significant peak is not at all captured by the model? Is it a transport problem? A mixing problem? A combination? Is it due to the assumptions in the source term? Given the discussion further on, it seems that the authors are, understandably, concerned about the representation of the PBL height. Have the authors made any tests in this regard? Also, as stated before, different PBL schemes in WRF can create different output. It would be good to have a clearer opinion of the authors on what factor they consider may be influencing most the poor model performance when representing the ground level concentrations and how would they approach a study to discern what is the main effect and how to compensate it (for example, as they have already suggested, increasing the resolution of the CTM and NWP calculations)

[Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-159, 2016.](#)

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