Response to Review #2

We thank the reviewer for taking the time and appreciate the helpful comments and suggestions for improving the manuscript given in this review.

The comments will be addressed below with review comments stated first, then the author's response in *italics,* the changes to the text is given in quotations (*""*), also in italics.

Structure and title: although the title of the paper focuses on the air pollution effects of the Holuhraun fissure eruption, the text is unbalanced in this regard, with a lot of description on the comparison of EMEP simulations with satellite and ground-based measurements. The title should be changed accordingly or the text restructured and reduced. A potential title, matching better the content of the paper, could be "A model study of the three months of the Holuhraun volcanic fissure: comparison with satellite and ground-based data and air pollution effects". The same unbalance exists in the, too long (please reduce), abstract. If the title remains the same, then the paper structure should be modified and the sections on the comparison with ground-based and satellite data should be gathered into a specific section that addresses the performance of the model calculations for this event. The results and discussion should then focus solely on the air pollution aspects once the following item is also addressed.

The abstract will be shortened, and the authors agree that the title does not reflect the context of the manuscript, title is changed to:

"A model study of the pollution effects of the first three months of the Holuhraun volcanic fissure: comparison with observations and air pollution effects"

Air pollution effects and chemical transport model results: the results and discussion on the air pollution effects should be further extended. The text is based solely on one model simulation with evident limitations. More discussion should appear on the potential effects of the mentioned limitations in the overall air quality side of the paper. In addition, the authors present wet and dry deposition results of the simulations with no comparison with existing data. Whenever wet scavenging data exists for such episode, it should be used to assess the very important effect of scavenging. The chemical transport model results are presented and discussed but without the required depth: why are there such large differences in the modelling results and the measurements? Is there a problem in the atmospheric mixing of the EMEP model that leads to such poor representation of the ground base measurements? what are the potential causes of not only the magnitude differences of the modelled versus measured peaks but also in their times? Have they tested different meteorological fields? Although it is clear that a thorough analysis would probably be out of the scope of the paper, additional thought should be made and added to the manuscript to help the reader with the questions that will surely appear when looking at Figures 4 to 6.

More on the limitations of the model for not performing better for the high concentration events will be included in the discussion part. Comparison to $PM_{2.5}$ measurements and SO_x wet deposition measurements will be included for stations where it is available. There is no known problem in the atmospheric mixing in the EMEP model. The complex transport to the stations for the first episode with first southerly winds, then northerly caused the SO_2 to stay in the atmosphere longer and increase in concentrations. The uncertainties due to model representations and meteorology errors accumulate and create the discrepancies seen in Figure 4. The comparison is better for the two later periods. The ECMWF meteorology is the best available meteorology for the EMEP model, and the resolution is also high. Schmidt et al. (2015) use another meteorological driver and also find the same *discrepancies over this late September period. The result and discussion part will be extended to include more station comparison data.*

Specific comments

Abstract: the abstract is too long and unfocused. Please highlight the main results according to the title of the paper (see General Comments)

The abstract will be shortened and more focused.

Abstract Line 12 - “lava floated” I would change float by flow.

Changed accordingly.

Line 4 Pag. 4 - The authors stated that this case can be used as a proxy for ash events as well. As the authors state further on (lines 9-10) that might not be the case, as Grimvoetn event showed with significantly different transport patterns for SO2 and ash. In addition the processes occurring for ash (including fine and coarse ash, aggregation, gravitational settling...) and SO2 (gas and aqueous phase chemistry) are different enough to add different uncertainties into the processes. It is indeed true that uncertainties in the source term may dominate, but I would rather suggest the authors erase the sentence “The Holuhraun eruption can also serve as a prototype...”

The authors agree that SO_2 is not a prototype for ash, removed the statement and changed the text to:

“Unlike the two previous big eruptions in Iceland, Eyjafjallajökull in 2010 and Grímsvötn in 2011, this eruption did not emit ash. However, uncertainties in source estimates, time varying emissions from a point source and dependence of transport on initial injection height are similar problems for SO_2 and ash plumes. For eruptions where both ash and SO_2 are emitted, SO_2 can act as a proxy for ash (Thomas and Prata et al, 2011; Sears et al., 2013), however separation can occur both because of different eruption heights within the plume (Moxnes et al., 2014) and density differences after some time. Proven capability of modelling the transport of a volcanic plume can be useful for judging future eruption scenarios where ash may cause a problem.”

Section 2.1 Model description: it would be useful to the reader to have more information on how the chemical module of EMEP/MSC-W works for SO2 since for this event the reactions with both OH and in the aqueous-phase (due to its low altitude pathway towards Europe) are significant.

Extended the model description to:

“SO_2 is oxidized to sulphate in both gas and aqueous phase with assumed equilibrium. In gas phase the oxidation is initiated by Hydroxide (OH), OH is labelled “short lived” and is controlled by local chemistry. In aqueous phase the oxidants ozone, hydrogen peroxide and oxygen catalysed by metal ions contribute to oxidation.”

Line 12 Pag. 4 - The authors should rewrite this paragraph in order to make it clearer to the reader what are they actually aiming at. What is the MAIN aim? and to achieve such aim what are the SECONDARY milestones or aspects that are addressed?

The aim is to study the perturbed sulphur budget due to the volcanic emission, both observed and modelled. The second aim is investigate the impact of the eruption on European pollution levels. This is also made more clear in the manuscript.

Line 16 Pag. 5 - Can the authors state (and even better reference) why they are finally using a constant 750 kg/s SO2 flux? They could have easily implemented a variable emission or taken a “worst case scenario” with the maximum flux of 120kt/day. This affects the discussion on the air pollution section and therefore should be clarified and its implications on the air quality results clearly discussed.

A worst case scenario with a emission of 1400 kg/s (max_hol) and a time varying emission given in Thordarson and Hartley (2015)(Thor_hol) is also studied, but the results were not better compared to observations. As shown in the Figure , 1for concentration comparison at the Manchester station in September where it is shown that an increase in emission gives an almost linear increase in concentrations of SO_2 and $PM_{2.5}$ (and deposition, not shown). Figure 2 show the satellite comparison for the hol_Thor simulation, same as Figure 2b in the manuscript.

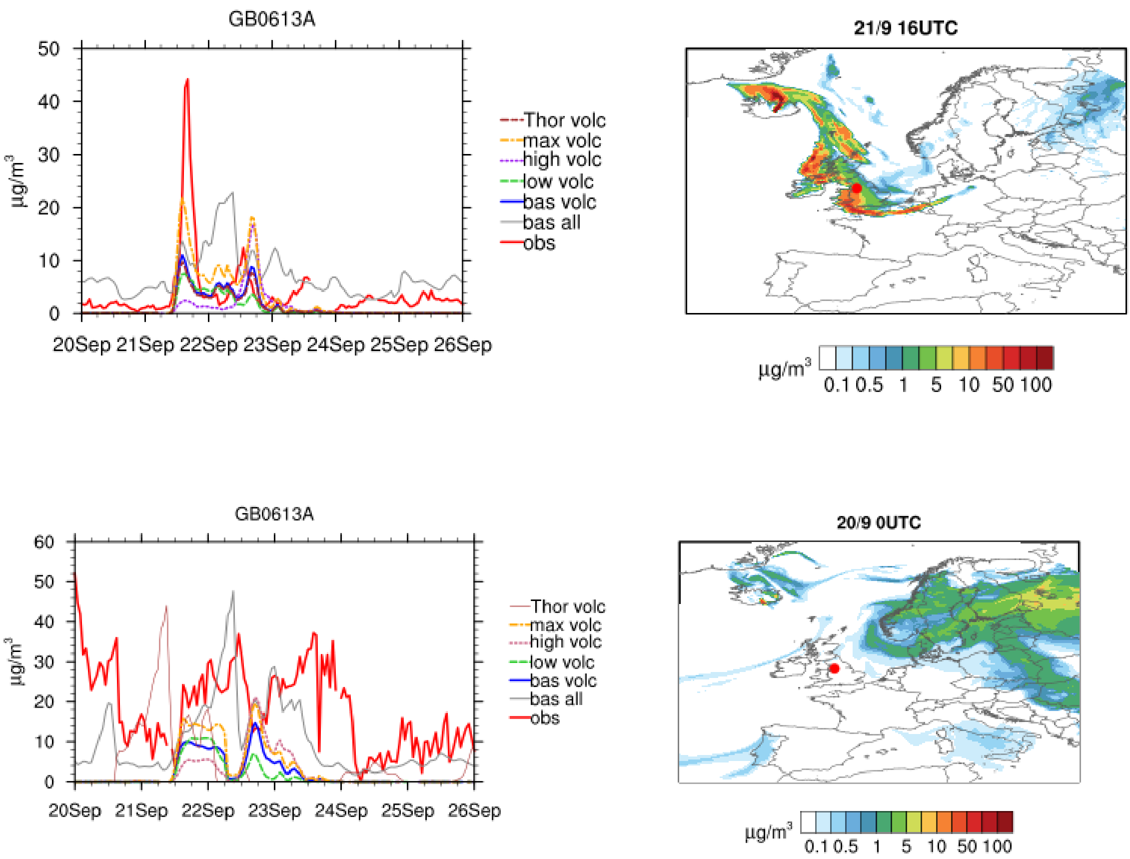

Figure 1. measured and modelled comcentration at GB0613A station in Manchester, Great Brittain (red dots on the map). The timeseries above show SO_2 concentrations and below for $PM_{2.5}$ for observed (red) and five different model simulations, bas all show all sources for SO_2 and $PM_{2.5}$, while the other volc lines only show values due to the volcanic eruption. The time of the map plot is the time of maximum oberved concentration.

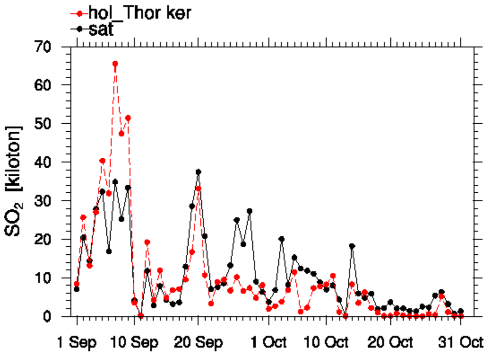

Figure 2. Daily time series of mass burdens from satellite data (black dots) and from model run with Thorarson and Hartley (2015) emission (red dots) with averaging kernel applied.

The height of the emission is seen to be more important, and therefore these two simulations are not included in the manuscript. This discussion is included in the manuscript, and the number behind the emission is added in the text:

"Emission from the Holuhraun fissure is set to a constant 750 kg/s SO_2 (65 kt/d) for the entire simulation from the total 2.0 ± 0.6 Tg SO_2 emitted in September estimated in Schmidt et al. (2015). "

Line 21-23 Pag. 5 - if the authors explain what the control run consists of, also the low and high runs should be explained in addition to the reference of table 1.

Will include more description.

Line 4 Pag. 8 - The measurements were regridded? following what method?

The sentence is changed to:

"Model data to represent the station values are picked from hourly data at model surface level in the gridpoint where the station is located."

Line 10-12 Pag. 12 - It is not entirely clear how the gross numbers in Table 2 are obtained. Is it for the 31 countries but the text states "only grid cells covering ONE ...".

Thank you for pointing out that this it is not clear. The sentence is changed to:

"Grid cells covered by the countries mentioned are used for calculating the results shown in the table,"

Section 3.3 "Effects of the eruption on European pollution". As stated in the general comments, this section should be extended. In addition, the authors should be careful with too general statements when their conclusions are based solely in one small set of simulations which, from the previous sections, do not prove to be very representative of the concentrations at ground level. Also, please try to add comparisons, whenever possible, with wet deposition measurement data.

The section will be extended to include more comparison to the station data observations, and rewritten so the statements better reflect the uncertainty that comes from a single model study.

References:

Gíslason, S.R., Stefánsdóttir, G., Pfeffer, M.A., Barsotti, S., Jóhannsson, Th., Galeczka, I., Bali, E., Sigmarsson, O., Stefánsson, A., Keller, N.S., Sigurdsson, Á., Bergsson, B., Galle, B., Jacobo, V.C., Arellano, S., Aiuppa, A., Jónasdóttir, E.B., Eiríksdóttir, E.S., Jakobsson, S., Guðfinnsson, G.H., Halldórsson, S.A., Gunnarsson, H., Haddadi, B., Jónsdóttir, I., Thordarson, Th., Riishuus, M., Högnadóttir, Th., Dürig, T., Pedersen, G.B.M., Höskuldsson, Á., Gudmundsson, M.T. (2015) Environmental pressure from the 2014–15 eruption of Bárðarbunga volcano, Iceland. Geochem. Persp. Let. 1, 84-93.

Schmidt, A., S. Leadbetter, N. Theys, E. Carboni, C. S. Witham, J. A. Stevenson, C. E. Birch, T. Thordarson, S. Turnock, S. Barsotti, et al. (2015), Satellite detection, long-range transport, and air quality impacts of volcanic sulfur dioxide from the 2014–2015 flood lava eruption at Bárðarbunga (Iceland), J. Geophys. Res. Atmos., 120, 9739–9757, doi:10.1002/2015JD023638.

Thordarson, T. & Hartley, M. (2015): Atmospheric sulfur loading by the ongoing Nornahraun eruption, North Iceland. , 17, 10708.