

# ***Interactive comment on “A model study of the pollution effects of the first three months of the Holuhraun volcanic fissure” by B. M. Steensen et al.***

## **Anonymous Referee #2**

Received and published: 21 February 2016

## **General comments**

The paper “A model study of the pollution effects of the first three months of the Holuhraun volcanic fissure” by Steensen et al. presents an interesting case of a volcanic eruption affecting air quality not only in the vicinity of the eruption area, but also afar, reaching mainland Europe. The paper is in general well written and clear, but it lacks additional in-depth evaluation of the results, specially in relation to the usage of model data to discuss the air quality effects. The text is too descriptive and, even if it states several aspects that may affect the conclusions derived in the study, it does not tackle them nor attempts to describe their potential effects in the air quality extrapolation made in the Results and Discussion sections of the paper.

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The authors should address the following general aspects before publication in ACP:

- **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.
- **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

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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.

### 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)

**Abstract Line 12** - "lava floated" I would change float by flow.

**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 SO<sub>2</sub> and ash. In addition the processes occurring for ash (including fine and coarse ash, aggregation, gravitational settling...) and SO<sub>2</sub> (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..."

**Section 2.1 Model description:** it would be useful to the reader to have more information on how the chemical module of EMEP/MS-CW works for SO<sub>2</sub> since for this event the reactions with both OH and in the aqueous-phase (due to its low altitude pathway towards Europe) are significant.

**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?

**Line 16 Pag. 5** - Can the authors state (and even better reference) why they are finally using a constant 750 kg/s SO<sub>2</sub> flux? They could have easily implemented a variable

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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.

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.

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

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 ...”.

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.

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-907, 2016.

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