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Atmospheric Chemistry and Physics Discussions

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 #1

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Steensen et al. investigate the effects of the 2014-15 Holuhraun eruption in Iceland on European air quality, SO2 burdens and sulfur deposition using a chemical transport model, satellite data and surface observations of SO2. The study is worth publishing, but in its current form it is not of the scientific standard expected for ACP, mainly because of the very descriptive writing style and lack of detailed comparison to available observations of PM2.5. The abstract should be shortened and throughout the manuscript a much more scientific and quantitative writing style ought to be used. Below I point out some instances that are rather descriptive but this is really a problem throughout most of this manuscript.

Specific comments:

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Page 2, line 1: what do you mean by 'peak type' increases? Give numbers here including that date of the measurement and location.

You report increases in PM2.5 mass concentrations based on your model simulations. There are plenty of PM2.5 monitoring sites across Europe (many more than for SO2), so you ought make an effort to compare the model simulations to these observations.

Are there deposition measurements available that could be used to compare to the model simulations?

Page 2, line 31: state the total amount of lava produced

Page 4, line 1: replace 'on the top' with 'at the top'

Page 4, line 4: I strongly disagree with that statement. I agree uncertainties in the source term affect both volcanic gas clouds and ash clouds, but fundamentally the processes that affect SO2 dispersion and conversion to sulfuric acid aerosol particles are different than those that affect volcanic ash concentrations downwind the source. I would simply say that Holuhraun is an eruption worth studying for gas and aerosol processes and effects.

The aims of the study could be described more clearly and put into context with previous studies (e.g. Schmidt et al., 2015, Gislason et al., 2015).

Model description:

It isn't clear to me why the Holuhraun case is called the 'control' simulation. Would it not be more intuitive to call the no_hol simulation the control simulation?

You run sensitivity simulations changing the emission height, but given that your are making statements about effects on air quality, it would be better to also test the sensitivity to the SO2 flux. I would recommend carrying out one simulation using 120 kt/d. It should also be possible to use a time-varying flux by using the data from Thordarson and Hartley (2015) for example.

Observations:

Page 6, lines 23-24: Schmidt et al. (2015) used IASI to derive plume heights, which indicates that using an a priory plume profile of 7 km is too high indeed.

Page 8, lines 3-4: be more specific and state the dates and significance of the SO2 observations for these episodes

Results

3.1 Comparison to satellite data

Page 9, line 1: state the highest value for both the satellite burden and the modeled burdens.

In particular, the simulated burdens for September 2014 should be compared to those in Schmidt et al. (2015), which should give you an opportunity to compare model performance to that of another model.

Page 10, line 7: here you should perform a sensitivity study using higher SO2 emissions than 65 kt/d and discuss the comparison to the satellite-derived burdens.

3.2 Surface concentrations

Page 10, line 9 onwards: give more detailed information including the locations of the measurement stations, the peak values observed and the date/time period of these observations. Surface SO2 mass concentrations of about 500 ug/m3 have been observed in Ireland on 6 September (when the eruption was most powerful). Why do you not use these data as well?

Page 12, lines 3-4: this is only true for the later period of the eruption. You haven't analysed observational data for the early eruption phase, which should be done and it should be stated more clearly that your results support emissions of about 65 kt/d for the late Sep to Oct period.

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3.3 Effects of the eruption on European pollution

Page 12, lines 6-7: this has also been shown by Gislason et al. (2015) and Schmidt et al. (2015)

Page 12, line 18 onwards: rewrite all paragraphs using less descriptive writing style

The increases in simulated PM2.5 mass concentrations ought to be compared to measurements from across Europe otherwise the discussion is of little scientific value (in particular because the model is not capturing peak SO2 mass concentrations at the ground compared to the observations).

4 Discussion

First paragraph: several aspects of this discussion are too simplistic because there are observations of the plume height (both at the source and in the far-field using IASI for example)

Second paragraph: Unless you carry out a sensitivity study changing the SO2 flux, you must not state that the variations in the source flux explain the differences between the observations and your model results because you haven't demonstrated that.

Page 15, lines 16-26: state the date and station name for each event that you discuss. I struggle to understand why the difference between the modeled and observed concentrations for the 6 Sep 2014 air pollution event cannot be explained by higher emissions fluxes.

Conclusions

All paragraphs need to be rewritten in a less descriptive manner.

Page 16, line 20: 'increase in SO2' what? Is there a word missing? Do you mean burden or surface mass concentrations? Previous studies that came to the same conclusion should be referenced here.

Last sentence: I disagree; the increase in SO2 mass concentrations was significant in several places even though the pollution episodes were transient.

Figure 1: state which model run is shown.

Figure 3: give date range and how does this compare to Schmidt et al. (2015) who I presume used the same satellite data but state much higher burdens than reported here. Is this down to different averaging periods?

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