

Interactive comment on “Impacts of the 2014–2015 Holuhraun eruption on the UK atmosphere” by Marsailidh M. Twigg et al.

Anonymous Referee #2

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General Comments:

The paper is an important presentation of near-surface atmospheric composition measured during the passage of the Bardarbunga-Holuhraun volcanic gas cloud over the UK in 2014. This provides new data on the composition and aerosol size of such a cloud and its impact on other atmospheric constituents at very long range from the original eruption site. It raises interesting questions about the wider chemical influence of such a cloud, whilst also confirming that there was a low hazard to human health. This paper fits well with the existing literature on observations of this event, e.g. Schmidt, 2015; Gislason, 2015; Jalongo, 2015; Grahn, 2015, although it is noticeable that the latter two are not referenced.

The paper would benefit from some further discussion of the two main con-

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cepts/theories it suggests and there are certain aspects that could be removed without detriment to the paper (see specific comments). There are also a relatively large number of typos that need correction.

Specific Comments:

The methods section of the report is very dry and is mainly a technical description of the equipment and sensors used. Depending on the editor's preference, there would be scope to move much of this to an appendix or even supplementary material. As it is, I would recommend that each section starts with a sentence summarising what it is that the technique measures. This is done for section 2.1 for example, but not for 2.3.

p2 Lines 15-17: The point the authors are trying to make here is a little unclear. Surely stratospheric measurements of volcanic composition are also only made/studied serendipitously? The fact that there is good satellite data of this eruption means that they cannot be referring to observations made with this type of sensor.

Section 2.2: The size range for Auchencorth Moss is provided, but not that for Harwell. It would be useful if the size range for Harwell could be included too.

Section 2.4 and 3.1: The authors need to provide information on where the GOME2 data has been obtained from and how it has been processed. Even though it is just being used as a qualitative picture, this information is still important. Further references for this would be useful in section 3.1. As the authors are using this data to compare to surface observations, the paper really needs some more information on the vertical sensitivity of the GOME2 instrument to SO₂ and to make it clear to the reader that the satellite data in figure 3 shows SO₂ throughout the atmosphere and not just the surface. SO₂ in the satellite data does not necessarily correspond to increased SO₂ at the surface.

Section 2.7: Given that two of the values generated by the statistical analysis are given in the abstract, I was expecting more substance to this aspect, but it appears

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that it is only covered in section 2.7. A more detailed explanation of the approach is needed here, perhaps with an accompanying figure, to fully explain what has been done. For example, the period over which the AGANET data has been evaluated is not mentioned. In Table 1 it is not clear to me why the first column is average SO₂ concentration. Section 2.3 would imply that the sensors record a monthly total – how has the average been calculated and is this particularly meaningful given that the authors nicely demonstrate elsewhere that the plume passed over in approx 4 days? It would be helpful for the authors to consider whether the concentrations from Sep 2014 are such outliers that actually the calculation of return period is not very robust? Some of these numbers are also referred to on page 8, where the authors refer to Goonhilly, but based on the numbers in Table 1 should this not be Yarnier Wood instead? The phrasing on p8 also implies that these sites had the highest values of any site ever, whereas I think the authors are actually suggesting that these sites had their highest monthly values. I'd suggest making this clearer.

Section 3.1: Figure 2 (and Fig 6) nicely demonstrates that there were three “pulses” of SO₂ observed across the UK from 21-25 Sep 2014 yet the authors make no mention of this. This observation in itself is interesting and the paper would benefit from consideration of this. It may not be possible to determine whether the cause is due to changing omissions at the source (a few days previous) or due to meteorological influences, but consideration of these aspects, and any others, should be included.

Section 3.1: The modelling sentence and Figure 4 are an unnecessary add-on and I recommend that the authors remove this together with section 2.5. The model does not add anything to the paper, but rather raises a whole number of questions that the authors will need to address if they want to include this. If anything, at first glance Figure 4 appears to contradict the other evidence as well as refute the descriptions in the text of northerly flow and that Scotland was worse affected than the south of the UK. There is not enough information on the modelling in the paper to explain what this figure is showing and describe why it does not match Figure 3. In addition, the fact that

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the figure contour scale only goes to 22 ug/m³ but the observations in Fig 2 reach over 60-100 ug/m³ would suggest that the model is not doing a good job at representing the plume. Given the efforts by other authors (e.g. Schmidt et al 2015) to demonstrate that their models provide good agreement with satellite column loading data before applying them to near surface concentrations, the lack of any model validation in this paper is particularly stark.

Section 3.2: This is particularly interesting and one of the main new findings presented in this work. I have a few questions based on the text which the authors could hopefully easily incorporate the answers to. The fact that the difference in the plume aerosol diameter is so pronounced compared to the background is perhaps worth stating more clearly. P6 Line 32 – the text talks about the slow oxidation of SO₂ in the troposphere, are you referring to heterogeneous or homogeneous oxidation here or both? P6 Line 37 – you refer to the plume containing “young SO₄”, I wonder whether it would be more appropriate to say that it is a “young plume”? Do you have any idea of the travel time since emission? Does this fit with your “young” finding? P7 Line 14-16: Is this true? This also sounds as though it needs consideration of the travel time. If the travel time was constant from the source to the observation point, then the particles arriving later in the day would have travelled longer in sunlight and so had a longer time to react. Perhaps it is just the wording of the sentence that needs tightening to make this clear. Should it be “with increasing time *after* sunrise”? And where you refer to “site” on line 16 do you mean the monitoring site or the eruption site?

Section 3.3: This section is also one of the main findings and theories in this paper derived from the observations. It is an interesting conclusion, but also raised a number of questions in my mind that it would be useful for the authors to comment on in the paper. Firstly, whether the displacement has occurred due to the transport of the plume over the sea for such a long distance (and/or time) or whether this is a relatively local affect due to the site being not far inland. Second, if the plume had travelled directly south, it actually would have been over land for many miles before reaching the site, how would

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this fit the proposed mechanism? Third, does such a mechanism require transport to Scotland to have occurred near to the sea surface / within the boundary layer / or more local near-surface transport following above-BL transport over the ocean? Is there any data to support one of these over another?

Conclusions: The first line states that the eruption perturbed *all* aspects of the UK atmosphere. As a first point this should be the atmospheric composition (not atmosphere, see technical corrections), but even so this seems to be rather overstating what has been presented. For instance there is no mention in the paper of changes to oxidant levels, impact on ammonium reactions, etc, which is understandable given the context, but would be necessary to justify the “all aspects” claim. Some minor, but careful, rewording of this sentence would bring it more in line with what has actually been presented.

Technical Corrections:

p1 Line 25: “of the Holuhraun” needs modification to for example “of the Holuhraun fissure” or “at Holuhraun”

p1 Line 29: expand what EMEP stands for, or omit from sentence

p1 Line 34/35: missing “of” – “due to primary emissions of HCl”

p2 Line 4: delete one of the two “were”

p2 Line 8: add “of this type” to the end of the final sentence

p2 Line 22: add “is” to “but there is a very limited”

p2 Line 27 and p9 Line 30: the eruption was within the Bardarbunga volcanic system not the Holuhraun volcanic system. The eruption site and the eruption have been called Holuhraun.

P2 Line 29: recommend making “emission” plural, i.e. emissions

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P2 Line 34: please explain what the EU-28 is/means for an international readership

P3 Line 8: “Northern” should have a lower case “n”

P3 line 26: replace “for” with “of”, i.e. “a detailed description of the instrument”

P3 line 27: please expand the acronyms QA/QC

P3 line 27: replace “are” with “is”, i.e. “by both instruments is given in”

P3 line 28: need to add “the”, i.e. “between the Auchencorth”

P3 line 30: what is “IC”?

P3 line 30: need to add “a”, i.e. “to achieve a lower detection”

P3 line 31: modify to be “therefore has an order of magnitude”

P4 line 26: a word is missing from “Downstream of is a gas”

P4 line 28: remove “this”

P4 line 35: Gome should be capitalised, i.e. GOME

P5 line 16: remove “below”

P5 line 24: in a number of places in the text the authors use “high resolution analysis”, this is not specific enough, I assume that they mean high temporal resolution not spatial? This should be included/made clear.

P6 line 19: there are other references that could be included here for the observation of the plume (see General Comments). It would also be useful for the authors to clarify whether these observations occurred at approximately the same time (i.e. related to the same plume transport) or at different times during the prolonged eruption.

P7 line 4: change to “or ‘banana’ shape in Figure 5, starting with”

P7 line 23-25: change line 23 to be “air quality impact from particulates during” and

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remove “due to particles” from line 25.

P7 line 35-36: The reference list is not needed here as these are already referred to or implied earlier in this sentence.

P7 line 38: However, a reference is definitely needed for the molar ratio of HCl/SO₂ being <1% near source.

P8 line 4: Is HCl correct at the end of this line? Should it be Cl-?

P8 line 31: Add “at”, i.e. “in particular at the sites”, and South West should be lower case

P8 line 32: add “in”, i.e. “whereas in Northern Ireland”, and change “were” to “was”

P8 line 33: add “that”, i.e., “noted, however, that there”

P8 line 37: suggest rephrasing “was not important to” to “was not significantly different to normal” or similar

P9 line 4: change to “the UK atmospheric composition during the latter part. . .”

P9 line 5: change line to “Elevated SO₂ was observed by the networks at both high and low temporal resolution. These observations complement the study by”

P9 line 8: remove the comma and change “to” to “in”

P9 line 10-11: I think we would expect particle formation and growth to be occurring in the plume based on past chemical and physical knowledge, so it would be better to say “. . . from the two EMEP supersites provide observational evidence for new particle formation and growth occurring as the plume. . .”

P9 line 14: add “work”, i.e. “the recent modelling work undertaken”

P9 line 22: add “that” to become “The study has highlighted that even though”

P9 line 25: change “are” to “is”

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P9 Line 26: remove “the” from “concurrently with the SO₂”

Fig 1 caption: Repeat of “sites”

Fig 3 caption: explain what VC SO₂ is and what DU is. Are these images snap-shots or aggregated daily totals or means? This needs to be stated.

Fig 5 caption: explain what the black line is

Fig 7 caption: remove capitalisation from “Sea”. It would be useful to explain the colouration of the dots in the main paper text and what this means for this event.

Fig 9 caption: what are the orange lines?

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-177, 2016.

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