Answer to the comment from referee #3 on "Validation of satellite SO2 observations in northern Finland during the Icelandic Holuhraun fissure eruption"
By I. Ialongo et al.

The authors thank the referee for the useful comments. The following text includes the point-to-point answer to the referee's comments. The referee's comments are in italics while the authors' answers are in roman.

The paper discusses SO2 measurements from three sources: satellite vertical column density retrievals, ground-based vertical column density measurements by the Brewer spectrophotometer, and in-situ measurements. Such comparisons are rare and therefore results are interesting. The fact that volcanic SO2 was detected near the ground and very far from the source is also very interesting because it may have practical air quality implications. The paper is well written and organized. The presented results demonstrate that all three types of measurements agree well qualitatively, although quantitative characteristics that is typically the main objective of a validation study, are presented in a very limited form. Nevertheless I think this study is interesting enough to be published at AMT after minor revision.

We add now two new figures in the supplementary material similar to Fig. 2 including more detailed information on the comparison (they include all overpasses within 60 km from Sodankylä). Also we include a new section in the revised about the analysis of the uncertainties, which gives more detailed analysis of the comparison results.

Specific comments: P. 602, l. 2. Acronyms such as SNPP, NRT, SACS, etc., are introduced but not used in the text.

We remove the SNPP and NRT acronyms as suggested but we leave SACS as is because this service is often known by its acronym more than from the extended name.

p. 603, l. 17. Row-anomaly appeared in the first time on June 25th of 2007, not in 2009. Did you exclude row-anomaly affected pixels from the analysis?

Yes, we refer to the large expansion of the row anomaly here. We correct now with 2007 in the revised manuscript. Yes, we removed the corrupted pixels from the analysis: we mention in the revised manuscript that the data corresponding to OMI pixels number from 23 to 56 are excluded from the analysis.

p. 604, l. 10. The authors should have some discussion, perhaps with a figure, about the difference between PBL, TRL, and STL OMI data product in terms of absolute SO2 values. For example, if STL SO2=1 DU, what would be the corresponding PBL and TRL values.

This difference varies case by case as highlighted in Fig. 2. This depends for example on the actual SO2 profile. As an example, we provide an averaging

kernel for TRL product in the supplement (Fig. S5). We mention also in Sect. 3.3 of the revised manuscript that "For instance, for an SO2 layer centered at 1 km, the LF TRL retrieval underestimates the column amount: the actual SO2 column is twice as large as TRL columns for a cloud-free scene."

p. 605. Section 2.2. Brewer SO2 measurements depend on DS irradiance at 306 nm. As the single monochromator MKII Brewer was used, measurements at 306 nm at high zenith angles are affected by stray light. The authors should comment on that. Brewer SO2 measurements on a sunny day with no SO2 could be used as an illustration of the stray light problem. Please also provide some information of the Brewer DS calibration for SO2.

Stray light problem has been addressed during comparison campaigns and also during Brewer calibration. We found that there is an impact for high air mass values, therefore the direct sun measurements corresponding high air mass values (after 14:20 UT) are not shown here. The calibrations have been performed on regular basis. During the calibration an extraterrestrial constant (ETC) is determined using the Langley extrapolation method as described by Redondas (2007).

Redondas, A. (2007): Ozone absolute Langley calibration. Edited by C. T. McElroy and E. W. Hare. The Tenth Biennial WMO Consultation on Brewer Ozone and UV Spectrophotometer Operation, Calibration and Data Reporting, GAW Report No.176 (WMO TD No. 1420), 12 -14.

This discussion will be added in section 2.2.

p. 606, l.2. What is the range of industrial SO2 emissions from the Kola Peninsula mentioned here?

The emissions from Kola Peninsula are about 10-15 kt for 2012 (the last year reported in EMEP database, www.ceip.at). We add this information in section 2.2.

p. 607. l. 28. OMPS observations are just briefly mentioned in the paper. Could you demonstrate the performance of similar OMI and OMPS data products to support this statement? For example, could you show OMPS maps in addition to OMI in Figure 1?
It would be interesting since OMPS is a relatively new instrument.

We add now in the supplement figure S1 (similar to Fig. 1) including maps of OMPS TRL product as an example.

p. 608, l. 6 "small OMI pixel (number 16)" and several other places below. You mentioned that OMI pixel sizes are different, but did not discussed the relationship between the pixel number and its size. Also p. 609, l. 14 "overpass corresponds to a very large OMPS pixel (number 1)" The only information about OMPS pixel sizes in the paper was that "its pixel size (50km×50km at nadir)", How large is "very large".

For the OMPS Nadir Mapper the off-nadir pixel at the edge is ~ 190 km x 50 km.
The close-to-nadir pixels (5--55 for OMI and 4-33 for OMPS) are considered as the "small" pixel.
We add this information in the text.

p.611, l.14. Make some reasonable assumptions about the SO2 vertical profile and estimate surface concentration from the total column. This would help to compare the Brewer results with in-situ data.

Unfortunately, this is not feasible in this case, since it would require very accurate profile information. For example, it is clear that the profile shape was not the same in the beginning and in the end of September: in the beginning the surface concentrations are much higher than in the end, although, vice versa is true for the total columns. Note also the answer to Ref. #2:

> "*Figure 3: it is not clear what the figure brings to the validation exercise.*
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> Despite satellite vertical columns and ground-based surface concentrations are not quantitatively comparable, the observed spatio-temporal link between high SO2 concentration values at surface and large total columns from satellite adds confidence in satellite-based observations for volcanic emission monitoring also at surface levels. In particular, satellite instruments show their capability to detect the position of the volcanic plume as compared to independent ground-based observations.
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> We add this discussion in the text."