

Interactive comment on “Validation of satellite SO₂ observations in northern Finland during the Icelandic Holuhraun fissure eruption” by I. Ialongo et al.

Anonymous Referee #3

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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

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to be published at AMT after minor revision.

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

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?

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.

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.

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

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.

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”.

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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.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 599, 2015.

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