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Atmospheric Measurement Techniques Discussions
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Interactive comment on "Retrieval of volcanic SO_2 from HIRS/2 using optimal estimation" *by* Georgina M. Miles et al.

Anonymous Referee #2

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General comments

The authors present a new algorithm for the retrieval of volcanic SO2 total column amount from the HIRS/2 instrument. This paper is well structured and convincingly demonstrates the added value of adding HIRS/2 to the series of instruments used for the retrieval of SO2.

Indeed, as stated in the paper, long-term and systematic monitoring of volcanic SO2 is relevant in relation to climates issues and knowledge on plume evolution. I enjoyed reading the paper and would certainly like to see it published in AMT, after taking into account the remarks below.

1 - Introduction Well written. Clearly indicates the relevance of performing SO2 retrievals on HIRS/2 measurements, an instrument originally not devised for that pur-

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pose. Although a method to derive SO2 from HIRS already exists (the Prata fit method), the paper indicates the shortcoming of that method and outline how the retrieval could be improved my taking multiple HIRS channels into account by using an OE scheme.

2 - Methodology Section 2.1 introduces the HIRS channels to be used in the OE retrieval, as well as the applied RTM, RTTOV.

P4 L17. Pleas mention briefly why NOAA11 was selected.

P5 L19-24: It may be beneficial to train the model for amount larger than 300 DU, as (much) higher values occasionally occur in the most powerful eruptions (e.g. Nabro in 2011). Is there a specific reason to limit the procedure to 300DU? The reference to Figure 1 seems premature, as this figure is discussed only later in the paper and shows total SO2 amounts up to 200 DU only. I suggest not mentioning the figure or to explicitly state that this figure is to be discussed in more detail later in the text.

Section 2.2: P6 L10-12: The later assessment of retrieval sensitivity to uncertainties in plume altitude and thickness is introduced here. The vertical extend of the plume is said to be derived from ancillary information. I think it would be good to state that all parameters involved here are effective values, certainly when using a pre-described triangular profile shape. For example, in reality the SO2 profile may show multiple peaks at different altitudes. Knowing this, the assumption of a triangular shape is as good as any other.

3. Error study. Overall a clear, to the point chapter.

Section 3.2.1 took me a bit longer to understand. P8 L25-28: The texts states that retrievals are performed on simulated spectra, with a fixes plume altitude of 12 km assumed in the retrieval. However , Figure 2 suggests that the peak altitude is fixed in the RFM simulations. Which is correct?

P9 L1-3: What is meant here? Where retrievals also performed using the RFM as forward model, with the conclusion that it performed less well for plumes > 17 km than

RTTOV?

Section 3.2.2 P9 L9-10. Any idea as to why an underestimation of the plume thickness has significantly less impact on the result than an overestimate? Would this depend on the peak altitude of the plume.,bringing it in another temperature/water vapour domain?

Section 3.3: The seem to be some little inconsistencies here. The section states that care should be taken with clouds above 5-6 km. A threshold of 5 km is used further on in the paper, whereas the abstract mentions 6 km. Yet, figure 4 suggests that one can go as far as 9 km without any significant problems.

4. Case study.

Please add a few words on why this particular eruption was selected to demonstrate the new algorithm. Also, this eruption is compared to Kasatochi in the abstract, something that you may want to repeat here.

The assumed plume altitude and thickness of the plume is not mentioned in the text. From the description of previous studies (putting the Cerro Hudson aerosols at 11-13 km) I assume that you used the same 12 km plume as used for Section 3, but this is not clear.

5. Discussion Despite the remaining uncertainties in the new algorithm, the authors manage to demonstrate the added value of the new system in comparison to previous methods. It would be nice to see a short summary here of what drawbacks of the Prata methos have been resolved by the new OE schemes and which issues remain, such as the dependence on plume altitude information.

I very much liked the clear understanding by the authors that the presented work can be seen as a mere first step toward extending and improving the long-term data series of volcanic SO2 measurements from satellite. I certainly hope that (part of) the proposed future work will be realized.

Cosmetics:

P2 L28: TiROS –> TIROS; Or keep TiROS and used this spelling consistently throughout the paper.

P13 L1-5: The wavelength unit (nm) is missing a few times.

P14 Eq 1: minus symbol missing in the exponent.

P23: Caption: 11.5, 12, 13 –> 11.5, 12., 12.5

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