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Atmospheric Measurement Techniques Discussions

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

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article graphicx

Authors' Response to Reviewer 2

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Referee comments in italics.

General comments

The authors present a new algorithm for the retrieval of volcanic SO_2 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 SO_2. Indeed, as stated in the paper, long-term and systematic monitoring of volcanic SO_2 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 SO_2 retrievals on HIRS/2 measurements, an instrument originally not devised for that purpose. Although a method to derive SO_2 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.

NOAA-9, 10, 11 and 12 were in orbit and operational at the time of the Cerro Hudson eruption. NOAA-11 was selected to pilot the technique principally because of its simple channel configuration. Using the 8.6 micron channel, which is potentially sensitive to both ash and SO_2, would be considered to be an extension to this work since it would require the forward model to additionally be able to simulate ash, and proof of concept with just a single channel sensitive to SO_2 was the first goal. Furthermore, NOAA11 benefits from an extra window channel (which on the other instruments is the 8.6 micron channel) that can be used for offline detection by means of BT difference or ratio flags should further diagnostics be required. This use was explored but is considered to be beyond the scope of the work, since the alternative and more reliable simulations using a cloud and aerosol model were perused to investigate the limitations of the retrieval under cloudy or ash filled FOVs.
We have modified the text as follows:
"This instrument was selected to demonstrate the capability of this version of the instrument with only one channel that is sensitive to SO_2 and two window channels that have some potential to be used to flag cloud and under some circumstances ash if required."

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 SO_2 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.

It would be beneficial to train the model for higher amounts than 300DU to accommodate the larger and more intense eruptions. The limit was chosen in this case to be appropriate for the case study eruption, where a priori knowledge existed (e.g. from TOMS) to suggest that in nearly all instances this would be sufficient. The training limit is very important due to the way in which RTTOV calculates layer transmittances for gases because some species require higher order terms in their predictor coefficients that are challenging to characterise. To train the model for higher SO_2 column amounts is probably possible, but would be the subject of further, future work as non-linearities in the behaviour of RTTOV at very high SO_2 loadings, sensitivity to profile shape and saturation effects would all have to be adequately examined.

In response to a comment from Reviewer 1, a new figure has been added to the manuscript as Figure 1, showing spectral transmittances for water vapour and two amounts of SO_2 (1DU and 300 DU) around the 7.3 micron channel. The reference to figure what is now figure 2 discussed her ehas been amended to reflect that it will be discussed in detail later on.

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 SO_2 profile may show multiple peaks at different altitudes. Knowing this, the assumption of a triangular shape is as good as any other.

A broadly triangular profile may be considered a better representation than any other for a short eruption where material could be expected to gather at a height of neutral buoyancy in the stratosphere where vertical sheer in the advection profile is more limited than in the troposphere over a short altitude range.

In tandem with a point from Reviewer 1, the text has been modified to elaborate on the profile used and the motivation for its selection. It is very probable that multiple peaks in the SO_2 profile may exist. This wasn't explicitly tested. Specifically in this case however, the profile used is considered to be a reasonable representation of the plume observed in the case study. As figure 3 demonstrates, there is some sensitivity to the thickness of the modelled plume – modelling it to be too thick introduces more error than under-estimating its thickness in the case tested here, although this is small compared to errors in height assignment. Often (e.g. for IASI and GOME-2) retrievals are performed assuming the material is at 3 altitudes, and the result which best fits the measurements is generally considered to be the 'best', but some human judgement (often based on ancillary information) is also required when looking at results for specific eruptions from these instruments or when considering total erupted mass. There is more information for the retrieval of altitude with spectrometers but when this is done the error on the amount of SO2 retrieved increases very substantially. In summary, an effective profile is often the best that can be done in the absence of any other information, but there may be some errors associated from getting it wrong – some of which needed to be explored here. As such, every effort must be made to make the profile and height as realistic as possible. More work was done than has been stated to identify the plume altitude from Hudson, and some of it bears repeating to explain the origin of the SO_2 profile used in the model, before it is stated. This is discussed in response to a point below, under Case Study.

That being said, we appreciate the point the reviewer is making here. Section 2.2. has had the following added to the start:
"In the absence of any further information, an effective SO_2 profile must be represented in the forward model."

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?

This section now reads (referring to what is now Figure 3):
"Measurements were simulated for a plume at a range of altitudes from 8-18 km. Figure 3 shows the impact on the retrieved SO_2 column at a specified, fixed altitude of 12 km as a fraction of the true column at these altitudes. Errors range from typically $\pm$0-30 % for most column amounts up to 100 DU an increase for larger amounts, and for particular altitudes. While the specific error may be state dependent (upon meteorological conditions, specifically the water vapour profile), these simulations do give a general indication as to the magnitude of error that can result from incorrect height assignment of the volcanic plume in the forward model. This is the largest source of error in the OE column retrieval (and the Prata-fit method) and is made more challenging because there is a dependency of the error on column amount. Since height assignment errors cannot be known such simulations can at least give a general indication of potential uncertainty of retrieved amounts, depending on the quality of information available regarding altitude of volcanic SO_2. It is clear therefore that good prior knowledge of the SO_2 plume altitude is necessary for accurate retrieval or fit of SO_2 column amounts from HIRS/2."

The caption for what is now figure 3 has been amended to read:
"A measurement was simulated for a volcanic plume of triangular profile centred at a range of altitudes, for a range of total column amounts. A retrieval is then performed where the plume is assumed to be at 12 km. The fractional difference, or error, is

plotted."

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?

The text has been modified to make its purpose more clear:
"The performance of the column fit was also directly assessed against a line-by-line model (RFM) for plume altitudes from 8 to 18 km (where the plume height assignment used in the retrieval was the same as that used in the measurement simulated by the RFM) and it was found that…". Specifically, it refers to a test of precision/accuracy of RTTOV vs RFM forward models.

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?

We are not certain, but as suggested it probably has something to do with where it is in the atmosphere and how this relates to the water vapour. It does suggest that so long as the plume is located (and modelled) above most of the atmospheric water vapour, it is better to underestimate the plume thickness or else use a very narrow profile in similar cases.
The text has been modified as follows:
"The retrieval simulations suggest that errors are larger when the plume thickness is overestimated (typically 13 %), with only small inaccuracies introduced when the plume thickness is under-estimated (less than 2 %). The modelled cloud top height was 3 km in all cases. It is therefore possible that an underestimate of plume thickness would result in smaller errors."

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.

We thank the reviewer for pointing out these inconsistencies. The pertinent panel in figure 4 is the bottom right, which shows that deviations (errors) in retrieved water vapour column begin when a cloud is at 6 km. Poor fitting of water vapour leads to errors in the retrieval of SO_2, because the 7.3 micron channel is sensitive to both water vapour and SO_2. A cloud at 5 km shows no perceptible deviation, and it is likely that the threshold is somewhere in between, which is the origin of the 5-6 km warning. In the abstract 6 km was stated as this will definitely contribute an error to the retrieval of SO_2. For clarity this has been corrected to 5 km since it is stated as "…above.." the given level.

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 following text has been added:
"In this sense, as well as being a non-equatorial eruption, it has similarities to the 2008 Kasatochi eruption in the Northern hemisphere. It is selected here as a case study because it was a relatively large eruption that has not been studied exhaustively, and a very good example of an eruption in recent satellite history which only TOMS observed with any significance, that can benefit from application of this technique."

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.
This has now been clearly stated, as well as the reasons for it:
"In addition, contemporary lidar measurements of the Hudson plume were made at the CSIRO (Commonwealth Scientificand Industrial Research Organisation) Division of Atmospheric Research, at Melbourne, Australia (38 S, 145 E) (Young et al., 1992, Barton et al. 1992). These measurements are sensitive to ash, sulphate aerosol and meteorological (water) cloud. The backscatter profiles tend to indicate peaks at around and above 20 km, and frequently at 10-13 km. The higher peak is attributed to aerosol from the Pinatubo eruption. Young et al. (1992) interpret the majority of observations that are thought to include Hudson material as the feature at 12 km in October, with variable cirrus at 10 km. It is reported by the authors that the plume was observed consistently from 28th August until December 1991 between 10 and 13 km, with a decreasing scattering ratio. The relative proportions that contribute to the backscatter measured are expected to be dominated by ash in the first few weeks after the eruption. Little ash is expected to be present after a month beyond the eruption, but by this time the vast majority of the SO_2 will have oxidised into aerosol. Whilst lidar is not sensitive to the presence of gaseous SO_2 inferences can be drawn from the height of the aerosol it eventually becomes. In this case the lidar information is considered to be considered a starting point as a guide for estimating the cloud height of the SO_2, to be considered in the context of other information."
Additionally:
"Using all of this information, the Hudson plume is modelled as a triangular peaked profile with a baseline of 2 km between 11 and 13 km, peaking at 12 km."

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 methods have been resolved by the new OE schemes and which issues remain, such as the

dependence on plume altitude information.

The list has been slightly expanded upon to include some more points:
"They include a quantified error on individual pixel retrieved values, latitudinal variation in accuracy, diagnostic indicators of the retrieval performance and goodness-of-fit and treatment of cloud and water vapour consistent to the retrieval of SO_2. When summing mass over a large number of pixels, the precision that these afford becomes increasingly important. Issues that remain are those endemic to ill posed problems where there is only one piece of information on SO_2 available and only limited information about the height or shape of the profile of a volcanic plume. It is conceivable that further progress might be made by using HIRS/2 aboard NOAA10 and 12 with the addition of the 8.6 μm channel in ash-free pixels."

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 SO_2 measurements from satellite. I certainly hope that (part of) the proposed future work will be realized.

We thank the reviewer for their support of what we consider to be useful work. This work has so far not had the benefit of any direct funding, but it is hoped that it may contribute to a future case for support for funding. We welcome any opportunities for collaboration.

Cosmetics:
P2 L28: TiROS to TIROS; Or keep TiROS and used this spelling consistently throughout the paper.
These have been unified to read TIrOS.

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

This has been corrected.

P14 Eq 1: minus symbol missing in the exponent.
This has been corrected.
P23: Caption: 11.5, 12, 13 to 11.5, 12., 12.5
This has been corrected.