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Discussions

Interactive comment on "Retrieval of sulphur dioxide from a ground-based thermal infrared imaging camera" *by* A. J. Prata and C. Bernardo

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Response to anonymous reviewer #2

We thank the reviewer for the careful reading of, and comments on our paper. The reviewer has provided some very helpful remarks that we address below.

General comments.

1. Short discussion and inter-comparison. The purpose of the paper was not aimed at doing inter-comparisons and to do this justice we really need to attempt a more systematic approach. An inter-comparison at an industrial stack would be of benefit but this does need careful planning and the UV cameras themselves are only now being

validated and inter-compared. We have added some sentences describing this and the relevant references have been added, but we feel we do not have enough scientific analysis of the retrievals to warrant a discussion on this aspect. 2. The algorithm. This also addresses some comments in 1. Our approach is to try to use the data in the imagery as far as possible and avoid excessive modeling. This approach does have its drawbacks but we feel it is better than a modeling approach (via simulations) as models also require some observations for input (e.g. MODTRAN needs a model or observed atmosphere). We have added some comments on the radiation absorption and emission properties to help explain the physics better.

The reason that we do not use all the filters is because the camera was designed to measure SO2 and ash particles and these are better discriminated at channels located near 10, 11 and 12 μm. We have added an atmospheric transmission spectrum and the filter profiles onto Figure 3. Here you can see the effect the reviewer refers to regarding the 10 μm filter (C4) and the ozone feature at $\sim$9.6 μm. We show two transmission curves: one for a plume 6 km from the camera and one for a plume 38 km distant. At the longer distance, the transmission is reduced because of ozone absorption but the effect is much smaller at 6 km. In fact the effect varies with elevation angle as the camera views higher into the atmosphere. The peak of the O3 layer is $\sim$23 km so for the majority of viewing conditions this should not be a problem. We have added a note on this in the text and elaborated on the filter selection as requested by the reviewer.

We have not attempted a full radiative transfer simulation of the imagery but this may be worthwhile under controlled conditions. The problem with modeling is that the scenes can be very complex, containing multiple sources with different properties (emissivities), emitting at different temperatures. It is a challenge to model these scenes effectively and such work requires a systematic methodology starting with simple emitting targets before attempting complex scenes with multiple emitters. 3. This is difficult to do for the reasons stated above. 4. Yes we do take it into account, but admit that it is a source of error. We don't think CH4 and N2O matter too much as these are not

generally large sources near volcanoes (unless it is a mud volcano!) but our sensitivity to these gases is very small. H2O, of course is a problem. In plume "H2O" is a major source of uncertainty and this has been stated in the paper. We rely on the H2O in the foreground to not vary too much, but admit this is an assumption. 5. Yes, good point we have done that. 6. We can't use saturated water lines (although they certainly exist – see the transmission spectra added to Fig. 3). Our filters are too broad and designed to view in the "window" region. 7. The plume temperature is determined from the 10 μm channel because we have found this to be the most transparent while little affected by the SO2 absorption. After some time (minutes), plumes usually reach ambient temperature so a radiosonde could be useful. We have assigned a large error (10-14%) in our error budget to plume temperature estimation and we suspect that a radiosonde estimate might not reduce this at all. Essentially, our estimate is based on a channel that is least sensitive to water vapour absorption (re-emission) so that the loss between the camera and the plume is smallest, although this still could be significant. It also depends on distance from the plume as shown in Fig. 3. At Etna the camera was ∼17 km from the plume so it is possible that O3 is influencing the results. We have added a comment on this in the paper and done some additional Modtran calculations to assess the effect of ozone on this channel. 8. There are many comments here so we will try to address them in sequence. Yes 5 filters = 5 pieces of information (assuming the measurements are uncorrelated) so in principle the reviewer is right. As stated earlier we use the longer wavelength channels (two) to infer ash particles, the 8.6 μm channel for SO2, the 10 μm channel for plume temperature and the fifth channel is either a broadband (7–14 μm) or the 7.3 μm channel, which is useless at ground level. The broadband channel gives much nicer (lower noise) imagery and we often reserve this for display and feature identification. We have added a sentence at the start of the paper to explain this. We had not considered the notation ambiguity for emissivity here – in fact the reviewer makes a good point, because this should have subscripts (i,j). We have amended this in Eq. (2) and (22) and also included a definition in the List of Symbols. In terms of changing the notation, we would prefer not to do this. Although,

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we accept that it might be easier to associate the symbols with the absorbers better by using SO2, H2O etc., some notational clarity is lost. We have accepted the reviewer's comment concerning notation for temperatures and used TB for brightness temperature and T for thermodynamic temperatures. We don't think it is necessary to include (26) after (1) but agree that a better description of the radiation terms, along the lines the reviewer has suggested, is needed here and we have made appropriate changes. The 7.3 μm channel unfortunately cannot be used to estimate plume temperatures. At ground level it is opaque – see Fig. 3. The purpose of this filter was for use at higher levels (above the water vapour) and we have used it successfully from a helicopter. It has a much stronger SO2 absorption feature, so even if it were more transparent, interference from SO2 would be a problem. We think the reviewer is referring to the fact that the measurements are not simultaneous. This does, of course, induce errors but these are not significant because the time difference is quite small (<1 minute). Again, we have added more explanation on the radiation terms.

Plume temperature: the size of the atmospheric correction for the 10 μm channel is illustrated in a new Figure (Fig. 8) which shows the variation of the correction for three plume temperatures as a function of the slant range. The calculations were performed using MODTRAN-4. This figures shows that the foreground radiation sometimes adds to the sensed temperature (because the atmosphere is warmer than the plume and emits) and sometimes removes radiation (due to water vapour absorption). The correction is based on a standard atmosphere–in the case where detailed temperature and humidity profiles are available better corrections can be made. This was not done for these data.

The absorption coefficients used and the transmission model are adequately described in Davis and Viezee (1964) and they also provide a table with the absorption coefficients. We did not see a need to repeat these values in our paper. The water vapour coefficients at 12 and 8.6 μm are ∼0.135 cm^-1 and ∼0.120 cm^-1, respectively, so they are quite close.

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Minor corrections.

1. We have added detail to Figure 3 to address the reviewer's concerns, including a calculation of the atmospheric transmission at two different ranges (6 and 38 km). 2. Caption corrected in Table 2.