Response to reviewer

I am very grateful for this insightful and constructive review of my paper and the reviewer has raised some very interesting points which I address below. There are four main aspects of my paper that the reviewer questions. These are:

1. **Terminology.** The terminology used to describe the measurements. I concur with the reviewer that scattering is the correct terminology and as pointed out I used the term “reflectance” as this is commonly used in reference to shortwave radiation reflected from the surface, clouds and atmosphere and recorded by the Himawari-8 shortwave detectors. Consequently, I have altered the text from “reflectance” to “scattered light” in the appropriate places and added a footnote sentence to convey this aspect. The changes are highlighted in coloured text.
2. **Radiative Transfer.** It is correct to say that the paper would benefit from a detailed radiative transfer analysis and this was stated in the paper. I was not aware of the SASKTRAN code and I have thus added a reference to this code (Bourassa et al., 2008) as a means for performing a more detailed theoretical analysis. However, I maintain that this is beyond the scope of this “introductory” paper, which introduces a new and unintended use of Himawari measurements.
3. **Composition.** The reviewer suggests that somehow I think the composition of the stratospheric aerosol is a “mystery”. It was not my intention to address the question of the exact composition of the aerosol. From a scientific methodology standpoint, in my view, it is not a good approach to assume one knows the result before performing the analysis. Sure, there is evidence that the aerosol is predominantly sulfate but why should I assume that for my new data analysis? A better approach (in my view) is to assume one does not know a priori the composition of the aerosol and then provide evidence one way or the other. In any case, I think the reviewer has misunderstood the point of the 1.61/2.25 ratio. This was intended to discriminate ice and water, since it is possible that some of the scattered light analysed could be from ice cloud rather than from the liquid water content of the stratospheric aerosol. High level ice cloud can penetrate the tropopause and so scattered light from these clouds may be misinterpreted as being from the volcanic aerosol. I don’t think using refractive index data for sulfate provides any more information – as the plot below (Fig.1R) shows, but it does suggest that a sulfate aerosol would give a similarly high value for the ratio.

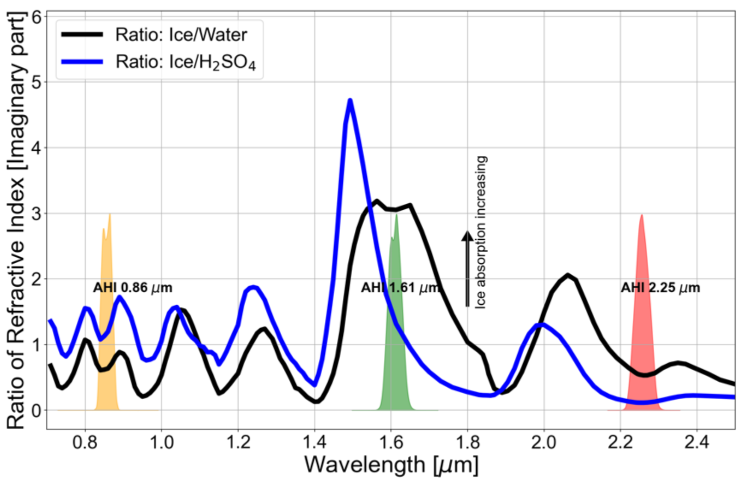

Fig. 1R. Ratio of the imaginary parts of the refractive index of Ice/Water and Ice/sulfate

Fig. 1R is the same as the m/s Fig. 4 but now including a ratio using refractive index data (from the Oxford U. ARIA data base, which uses the Palmer and Williams, 1975 measurements) for sulfate and with an expanded abscissa. The same (qualitative) conclusion can be made about the ice content of the aerosol based on the 1.61/2.25 µm ratio for sulfate rather than pure water. There may, admittedly, be quantitative differences, but as noted this could only be properly assessed using a radiative transfer approach. I have added a new sentence to convey this finding.

Not all volcanic eruptions create sulfate aerosols, the eruption of Chaiten in Chile is a good example (Prata et al., 2015). The material, which reached as far as Australia was predominantly particulate fine ash, probably andesitic in composition. Likewise, the Australian bushfires of 2019/2020 created a stratospheric aerosol (Khaykin et al., 2020) that contained particulates and likely water/ice and oxides of C and N. As part of this research, I analysed copious amounts of data, not just for Hunga but for some other eruptions and for other aerosol types (smoke form fires). I also investigated different channel combinations and indeed developed some interesting metrics by combining 3 channels. These analyses and others were not included in this paper for the sake of brevity; I was predominantly concerned that the ratio might ve seeing some other aerosol or ice clouds that had penetrated the tropopause. To show that other aerosols are indeed detected, I include a new figure here for the 2019/2020 Australian bushfires using a different channel ratio – clearly much more work could be done with these data.

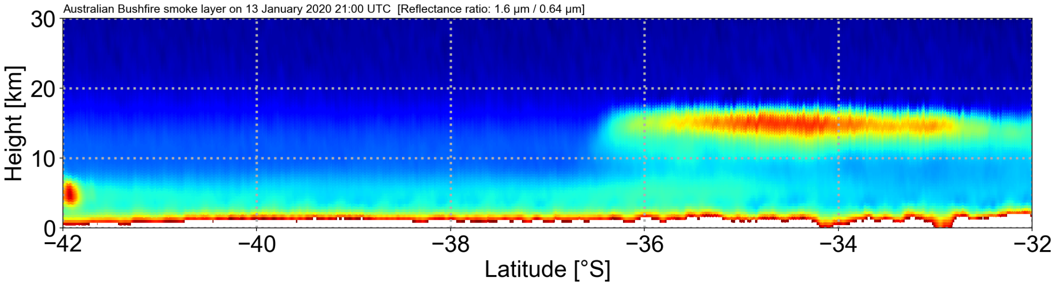

Fig. 2R. Detection of aerosol layer due to bushfire smoke on 13 January 2020. Note that a different channel ratio 1.61/0.64 µm is used rather than 1.61/2.25 µm.

4. **Ice detection and vertical resolution**. I think the reviewer is essentially stating (perhaps more elegantly) what I intended that the ratio is less sensitive to ice and so is detecting the liquid water content of the aerosol. In my analyses low values of the ratio are either because there is nothing scattering the light back or it is ice doing the scattering, since the ratio of imaginary parts of the refractive indices (related to absorption or 1-scattering, in simplified terms) is a measure of the liquid water content. I didn't suggest that the ratio is sensitive to sulfate; although it likely is. The ratio is only semi-quantitative; radiative transfer calculations using limb geometry might provide a lower limit for the ratio, below which the composition could be inferred to be ice rather than liquid water (or sulfate). I made a plot of the ratio of 2.25/0.86 µm for 7 Feb 2022 and this is shown below.

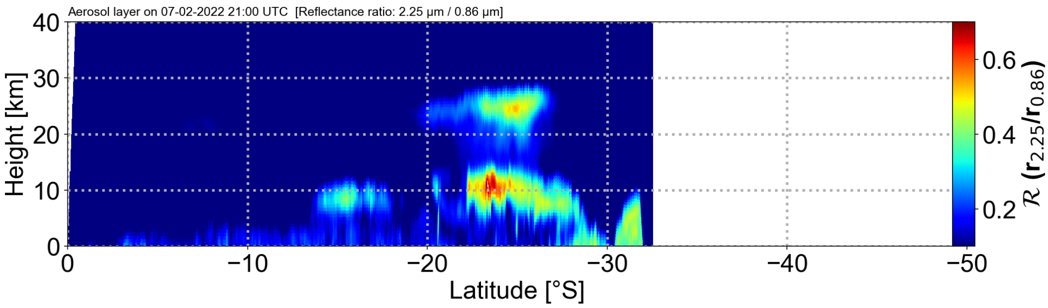

Figure 3R. Ratio of 2.25/0.86 μm reflectances for 7 Feb. 2022

Some features have changed – the suspect ice cloud between 10-14 km is clearly discernible. This presumably corresponds to the Caliop feature between 16-17 km. But it's not clear to me how this helps? According to this ratio a high contrast is also expected for water clouds. The idea of the ratioing was to find a measure that enhanced the water signature while diminishing the ice signature (it does not seem possible to discriminate a sulfate signature without resorting to the use of the thermal channels and here it is difficult because the measurement would be one of emission against a cold background and consequently poor SNR). Nevertheless, I am not arguing that this is an optimal ratio or even the most appropriate, only that it seems to be able to detect the aerosol quite well.

The vertical resolution of the limb measurements is poor and I accept the reviewer's contention that some of the signal is an artefact of the analysis. It's certainly an interesting idea to try to sharpen the measurements, but I'm not sure the methodology suggested would work since the Earth and satellite rotate at the same rate. Some kind of 'dithering' may be possible using the intrinsic wobble of the satellite platform together with the high temporal sampling (10 minutes). I believe satellite attitude data are available.

Again, I thank the reviewer for his very interesting comments and I suspect he knows that more research is required (by me, or hopefully someone else!) in order to give this new use of the Himawari data more 'legs'.

References

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