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Atmospheric Measurement Techniques Discussions

Interactive comment on "An adaptation of the CO_2 slicing technique for the Infrared Atmospheric Sounding Interferometer to obtain the height of tropospheric volcanic ash clouds" *by* Isabelle A. Taylor et al.

Anonymous Referee #2

Received and published: 15 February 2019

General comments:

In general, I thought this was a really good paper, extremely well-written, and presents some nice, potentially useful results. My only major concern is that I feel that there is a potentially useful scientific discussion to be had as to why the CO2 slicing approach gives better results than the full OE retrieval - and this discussion wasn't really explored to any significant extent in the present manuscript.

Major comments:

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Discussion paper

1. Abstract and elsewhere: The statement in the abstract reads “Overall, the CO2 slicing tool performs better than the optimal estimation scheme”, which stopped me in my tracks! In common with the report of Referee #1, I was surprised by this – why indeed should the CO2 slicing approach (which is basically a cut-down version of a full OE retrieval) outperform the OE approach? I feel that this should be explored in some more detail, because this (for me) is the main scientific issue in this manuscript. The authors suggest that the prior height constraint is responsible for the low ash height bias shown by the OE retrievals – to me, this suggests that the prior is clearly not appropriate in this case, or is being given far too much weight in the analysis.

2. Section 4.1, paragraph beginning “Figure 3 demonstrates. . .”: I felt that this paragraph didn’t really do justice to the description of Figure 3, and I think it would be good if the reader could be “guided” through the details of this Figure a little bit more. When you say “. . .the best performing channels. . .”, do you mean CO2 channels, reference channels, or both? When you say “As expected, this shifts from lower wavenumbers at lower pressures to higher wavenumbers closer to the surface”, I found it very hard to interpret the intended meaning. Is “this” referring to the “best performing channels”? I really couldn’t reconcile this sentence with what I was seeing in Figure 3.

3. Section 5.2, page 10, 28-29: You suggest that “In future applications of the OE scheme, the CO2 slicing results could be used as the a priori”. I disagree very strongly with this statement! One CANNOT use as prior information a state which has already been influenced by the measurements themselves. You could use the CO2-slicing solution as the first guess in the OE iterative process, but absolutely not as the prior constraint. There’s an equivalent comment in Section 6 (line 16 on page 12).

Minor comments, grammar, typos, suggestions, etc.

1. Section 2, page 4, line 2: “dependant” in this context should, I think, be “dependent”?

2. Section 3, page 5, line 16: Trivial I know, but EUMETSAT usually insist that the satellite name is “Metop” and not “MetOp”!

3. Section 4.1, and elsewhere: I note that you use “mb” for pressure units – I suspect the journal would prefer “hPa”.

4. Section 4.1, page 6, line 9: When you use the phrase “is greater than the CO2 channel”, in what sense is “greater than” meant in this context? Channel number, wavenumber? Best to be absolutely explicit for clarity.

5. Section 4.2, page 6, line 16: When you say “The top two lines show. . .”, I suggest using the word “rows” instead of “lines”.

6. Section 4.2, page 6, lines 26 and 29: You use the phrase “less channels” a couple of times. It should be “fewer channels”.

7. Section 4.2, page 7, lines 16-19: It’s not made clear in the text why you make the distinction between using narrower ranges for the channel selection work (ash optical depths ranging between 5 and 15, ash effective radius between 5 and 10 microns) than for the simulated retrieval work (ash optical depths ranging between 0.5 and 15, ash effective radius between 1 and 10 microns). It would be good for you to be explicit as to exactly why you didn’t use “spectra representative of thinner ash clouds” for the channel selection.

8. Section 5, page 8, line 31: “planck” should have upper-case “P”.

9. Section 5, page 9, line 33: You say that you have defined the tropopause “as the height at which the temperature profile inverts and has a positive gradient”, but it’s confusing that the tropopause dashed lines in Figures 6(a-f) have obviously not used this definition!

10. Section 5, page 9, lines 1-2: When you say “Figure 6 demonstrated that the CO2 slicing method performs poorly where the temperature profile steepens significantly”, can you clarify exactly what “steepens” means in this context – when dealing with (negative) vertical gradients, it’s very easy for the reader to become confused with words such as “steepens”!

11. Section 5.2, page 10, lines 31-32: You say that “Ventress et al. (2016) identified that in some cases the retrieval assumed a lower altitude and a higher ash optical depth in order to fit the spectra”. Lower/higher than what? Just needs to be a little clearer.

12. Figure 4: What is the x-axis here? The y-axis is labelled simply as “Wavenumber”. Presumably they should both be the same as for Figure 3?

13. Figures 10-13: Where does the retrieved ash mass come from – the OE retrievals? In any case, it’s not clear to me exactly why these mass column loadings have been included in the paper, as I don’t believe they are ever referred to. What do they add to the paper?

14. Table 3: Needs units! Presumably the “Channel Ranges” etc. refer to wavenumbers (cmˆ-1)? Are the “Peak Sensitivity Ranges” in mb/hPa?

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