

## ***Interactive comment on “Strato-mesospheric carbon monoxide profiles above Kiruna since 2008” by Niall J. Ryan et al.***

### **Anonymous Referee #3**

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This is an interesting and well written study of CO in the mesosphere above Kiruna. My primary concern with this work is in the CO retrievals. The authors are, apparently for each measurement, fitting 3 sinusoidal baseline waves plus a second order polynomial. This seems to be done as part of a pre-retrieval process. They then go on to do an optimal estimation retrieval, and from that retrieval calculate the “measurement response”. But if the baseline waves removed in the pre-retrieval process affect the retrieved mixing ratio profile then the measurements response calculated in the optimal estimation retrieval no long represents the true measurement response. In fact, having removed all of these waves, the authors need to reassure the reader that significant information remains in the retrieval.

In order to address this issue the authors need to do one of several things. 1) They could show (or simply state) that the waves fitted in the pre-retrieval process do not

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significantly affect the retrieved vertical profile. 2) They could keep the waves constant for all of the measurements – this would at least give them a measure of the variation of CO if not an absolute measure. 3) They could include fit the baseline waves as part of the optimal estimation retrieval.

The other more serious issue regards Page 8 line 22 - “The slopes of the lines of best fit were calculated without assuming uncertainty in the temperature datasets.” I’m not sure what the authors are trying to say here, but any slope fit must always make some assumption about uncertainties. I assume what the authors are doing here is using a generic linear fitting routine, in which case they are implicitly assuming that all of the uncertainty is in the dependent variable. This is almost certainly not appropriate since the comparison is between two types of temperature datasets, neither one of which is perfect. It would be far better to use the same slope fitting routine as was used in the KIMRA v. MLS comparisons and assume that the two temperature datasets have similar precisions.

I do have a few minor comments:

Page 2 line 15 – “offering the advantage of providing measurements during polar night.” Advantage relative to whom? There are certainly other satellite instruments that measure during the night. Page 2 line 25 – “a new CO dataset”. Perhaps a better phrase here would be “a new CO retrieval” so as not to imply that these are new measurements. Page 3 line 11 – “So the pointing angle changes from one measurement to another, meaning that individual spectra cannot be averaged to reduce the SNR.” While one can perhaps understand the author’s reluctance to average together spectra taken at different angles, there are certainly many microwave studies which average together spectra taken at different pointing angles. The elevation angle for the retrieval is then based upon an appropriately weighted average of these pointing angles. Spectral averaging periods can vary from as much as a week [Nedoluha et al., JGR, 834-942, 2013] to an hour [Ohyama et al., Earth Planets, and Space, (2016) 68:34] depending upon species and altitude of the retrieval, whereas pointing angle adjustments may occur

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over timescales of a minute.

Page 5 line 32 – If the data are filtered to satisfy a  $\text{DOF} > 1$  would this result in a high bias since measurements when the CO concentrations are low are more likely to be filtered?

Page 5 line 27 – “In the middle atmosphere the dataset has a positive bias of approximately 20% compared with the ACE satellite, suggested by Livesey et al. (2015) using a validation of the MLS version 2.2 CO data (Pumphrey et al., 2007).” What is not stated here is whether MLS v2.2 CO is biased relative to the MLS v4.2 CO used in this study. Is it?

Page 6 line 32 – “a vertical resolution of more than twice ...” can be a difficult to interpret phrase. Simply adding “...as good as ...” would be helpful, but even better would be some approximate vertical resolution numbers.

Page 9 line 14 – There is a superfluous “.” after a reference.

Figure 1, middle: I assume that this is the spectrum after removal of the 3 sine waves and the second order polynomial described on Page 3. If so, this needs to be clearly stated either in the caption or in the text near Page 5 line 7.

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