

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

Anonymous Referee #2

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This manuscript presents a mesospheric CO dataset covering the period 2008-2015 obtained by means of a ground-based radiometer, KIMRA, installed at Kiruna. The dataset is very valuable and I recommend its publication on ESSD. There are, however, several points I suggest the authors should address before the manuscript is published.

General comments:

1) This manuscript relies on a previous publication (Hoffmann et al., 2011) for what concerns the error analysis description, but the two datasets (the one published in 2011 and the one described here) have important differences (versions of ARTS and QPACK, temperature and pressure profiles, time interval of data used in the comparison, resulting vertical profiles, and more). A description of the uncertainties characterizing the presented dataset should therefore be added in this manuscript, also to

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clarify more precisely which uncertainties and general data characteristics described in Hoffmann et al. apply here as well. This is not clear at the moment.

2) In order to consider this manuscript a validation of this updated and extended dataset, the authors should be more quantitative on the biases with respect to Aura/MLS and show in more details the differences between KIMRA and MLS. It would be important to know and state what the relative difference between the two datasets is, at all (most) altitudes. In the abstract, authors state that the two sets of data have a "high level of agreement", but a possible bias of 10-15% over parts of the vertical coverage does not qualify as "high level of agreement". Figure 4 (b) also shows that in the upper mesosphere (65-85 km) there's a large std dev of the difference (which is not sufficiently stressed in the manuscript) and this weakens the stated agreement between the two datasets.

3) The uncertainties and potential biases of the dataset uploaded in the database are not clear. I agree that it is useful to underline how important it is the use of different T and P vertical profiles in the analysis (and hence the presented study on the two different KIMRA set of retrievals), but eventually potential users/readers are interested in knowing how to characterize/treat/use the data made available, e.g., the retrievals associated to the ECMWF reanalysis. As a start, in the various comparisons discussed in the manuscript the authors should always state if the bias is positive or negative, i.e., which dataset is high or low.

4) The altitude range of the dataset that is recommended for scientific use is not clear, as numbers change in the manuscript and differ from the 40-80 km range (with the interval 70-80 km to be used with care) recommended in Hoffmann et al.

5) Hoffmann et al. (2011) explicitly stated that the use for that dataset was only for comparison and validation with higher vertical resolution datasets, and therefore the error associated with those profiles did not include, for example, the smoothing error. I personally find this approach correct. Here, however, it is a different matter. Authors

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should advise potential users that intend to employ the updated and extended KIMRA dataset as a stand alone product that the uncertainties in the mixing ratio profiles can be much larger due to, for example, the smoothing error. More importantly, I think that in the dataset there should be the information on the estimated total uncertainty, as it is described for example by Hoffmann et al.

Specific comments:

Page 4, Lines 1-2: Is the sinewave removal accounted for in the error estimate and sensitivity of the retrieval? Sinewaves should also be shown in Figure 1 to give the reader an idea of what their amplitude might be compared to the errors listed and the spectral line intensity.

Page 4, Lines 1-2: Same as above.

Page 4, Lines 15: Co-authors should not be cited in personal communication, I think.

Page 4, Lines 27: See general comment #1.

Page 5, Line 10: Looking at the dataset, there are profiles which have a measurement response larger than 0.8 at 93 km altitude. This upper limit for the retrieval is, however, unrealistic. Authors should state explicitly in the manuscript and in the uploaded dataset which is the altitude range where KIMRA profiles are reliable.

Page 5, Line 17: See general comment #1.

Page 5, Line 20: This concept is clear. However, you should also advise potential users that intend to employ your dataset as a stand alone product.

Page 5, Line 31: figure 4e seems to suggest a larger coincidence criteria.

Page 6, Line 15: In the X-title of the plots of figure 2 it says 46-66 km, instead of 40-60. Which is it?

Page 7, Line 4: I am not sure this figure is necessary.

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Page 7, Line 10: Please, explain in more details what is this correlation. If you refer exactly to what has been done by Hoffmann et al., please say so.

Page 7, Line 14: In figure 4, the difference between MLS and MLS smooth is not convincing. Kimra averaging kernels are apparently pushing MLS towards larger CO vmr values with respect to its original values. A good set of AKs should just smooth the original dataset, degrade it, not add positive or negative biases. Furthermore, this plot doesn't illustrate what the actual differences between KIMRA and MLS is. X-scale is maybe too large, and it would be useful also to see the relative difference in percentage.

Page 7, Line 20: I understand that this analysis replaces the older one, but maybe a tentative guess as to what is working now that wasn't working before should be attempted. Also, if the analyses are so different, more needs to be said concerning the error analysis of this latest one (see general comment #1).

Page 7, Line 28: This is true only up to 65 km, above that (a large fraction of the profile) Hoffmann et al. show that the predominant error is due to the doppler broedening, e.g., temperature, and in this case doubling the measurement error is definitely not an overestimation, but again an underestimation.

Page 7, Line 29: It's not clear to me why a slope greater than one means greater variation in KIMRA CO concentrations with respect to MLS. Please, explain.

Page 8, Line 2: In order to consider this manuscript a validation of this dataset, the authors should be more quantitative and show in more details the differences between KIMRA and MLS (see general comment #2).

Page 8, Line 11: This sentence is unclear.

Page 8, Line 21: I understand that MSIS profiles from a different time of the day can be different from one another. Yet, looking at figure 5, their difference at 115 km and their low correlation at 100 km are surprising.

Page 9, Line 10: By using your dataset only, how can you distinguish a SSW from a

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simple shift of the vortex further away from kiruna?

Page 9, line 13: Remove point after "2015").

Page 9, Line 19: As commented earlier, what about users of this dataset as a stand alone? Should you add to the dataset also a column with the total uncertainty?

Page 9, Line 23: Is this the official altitude range? What about the considerations in Hoffman et al. that limit the upper altitude to 80 km? Also, in figure 4 (b), are the Δ V_{MR} below 50 km particularly small because the inversions don't have enough sensitivity below 50 km?

References: Straub et al., 2013, is missing.

Figure 2: Units of the X-axis are missing.

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