This paper describes the recovery and retrieval of middle atmospheric CO data from the SMR instrument onboard the Odin satellite. It presents a valuable new dataset for the analysis of mesospheric and upper stratospheric transport processes and dynamics. The paper is well written and the figures and tables are general clear and effective at conveying the essential points. There is one major issue on the recovery method that ought to be addressed, along with a number of more minor comments and suggestions. The paper is highly suitable for publication in AMT provided that these issues are addressed.

Major

There seems to be two major obstacles to overcome during periods when the PLL was malfunctioning: frequency shifting and line broadening. The approaches taken to overcome these obstacles seem reasonable, but more information must be provided in the paper in order to assess whether this is the case.

1. The paper is not entirely clear about this, but my impression is that from 2001-2003, the PLL was malfunctioning but only a frequency correction was need. In 2003-2004, the PLL was working and no corrections were applied. Then from 2004 onward, the PLL was malfunctioning but both the frequency and broadening corrections were needed. My naive assumption was that the broadening was a result of the frequency shift occurring during the time of a single spectrum, but this seems inconsistent with how the corrections were applied. In any event, the paper needs to have more information to better clarify the malfunctions of the PLL during the pre 2003 and post 2004 time periods.

2. For the frequency correction, a "basic" correction applied to all spectra in a scan is derived using differences in vertical gradients of brightness temperature (T_b) between O3 and CO. This is a novel idea and it seems to work well. However, Fig 3 compares T_b profiles for global averages, and one might expect to see differences according to latitude or season since vertical gradients in O3 and CO mixing ratios do change with season and latitude. Whether seasonal or latitudinal variations matter for this "gradient threshold" method is something that needs to be demonstrated. There should also be bars or shading to show the sigma in Fig 3.

3. A second, single-altitude frequency correction seems to be applied using Gaussian fits, but it is not discussed in sufficient detail at the top of p. 6. Why is a Gaussian thought to be the appropriate lineshape (later, thermal broadening is mentioned but what about pressure broadening at the bottom of a scan)? Was the central frequency and half-width fitted independently using some least-squares method? If so, how do the fitted widths compare with those expected? Was this second stage of frequency correction required for both 2001-2003 and post 2004 periods, or only post 2004 as for the broadening correction?

4. Figures 2 and 4 should have color bar legends to show the approximate range of altitudes covered.

5. It is important to show how the frequency shift and broadening varied with time, to give an idea of the magnitudes and temporal variation of both corrections. For example, monthly means of both quantities such as in Fig 7, used for the total number scans, would give the reader this kind of information.

6. Related to number 5 above, in the dataset there should be quality or error flags (or raw information such as mean scan frequency shift and broadening correction) so that users can further filter or weight these data for scientific study. The paper does not discuss any error estimates in the dataset.

Minor

1. abstract line 7 "The much of the..." -> "Much of the..."

2. p. 2 l. 3 "Schumann-Runge *bands* and continuum..."

3. p. 2. l. 27 "This is the first data being part of the..." is confusing

4. caption of Fig 3 "...and in the right place." -> "...and at the expected frequency."

5. p. 5 l. 13 "...a value higher than -0.0045 K/m..." Since all slopes are negative, this is confusing. I think what is meant is a slope with a magnitude less than 0.0045 K/m. Clearly, O3 has a larger magnitude in the vertical gradient of T_b, and CO a much smaller vertical gradient in T_b. Referring to slopes in Fig 3 is confusing because CO has a steeper slope, but a smaller vertical gradient.

6. p. 6 l. 7 define ARTS at first use

7. p. 7 l. 10 "Despite *the fact that* spectra above 40 km tangent altitude are considered..."

8. p. 8 l. 6 Is the given sigma FWHM or HWHM?

9. p. 10 l. 1 High CO vmr due to enhanced descent at the stratopause following an SSW was clearly demonstrated by Manney et al (Atmos. Chem. Phys., 9, 4775–4795, 2009).

10. p. 12, l. 5-10 Not enough attention is given to possible biases due to differences in vertical resolution. There can be large vertical gradients in CO that depend on altitude and season. Between 70km and 90 km, where some biases are shown, CO increases by an order of magnitude over 20 km and differences in vertical averaging could be a factor. The mean averaging kernel widths between the various instruments should be given. For example, it is possible that ACE has at least a factor or 2 higher vertical resolution than SMR.