The authors would like to take the opportunity to thank the reviewers for their comments and for taking the time to offer them. We believe the manuscript has been improved with the helpful input.

The authors use WACCM to show the tendencies in CO for the winters of 2008/2009 and 2010/2011. As the authors show, using WACCM, vertical advection is not the only important contributor to these tendencies. These model results are generally reasonably discussed, although I do have some concerns about the presentations in some of the figures (as detailed below). But my much more serious concern is that the authors fail to make appropriate use of their measurements.

Before comparing descent rates in the models and measurements, and before addressing the six major processes that govern this overall tendency in the model, the authors should first show a comparison of the overall CO tendencies in measurements and models. Admittedly, the model analysis could continue without such a comparison (as the authors state on page 3), but if the CO tendencies in measurements and models are not similar then why are the measurements included here at all? Such a good comparison of WACCM with the measurement components used in this study would be invaluable in helping to judge the ability of the model to accurately address the issue of descent. However, currently the only figure that shows measured CO is Figure 1, and this is both almost impossible to read and does nothing to help the reader to judge the quality of the data as it relates to this study (I would suggest to remove this figure).

The aim of the paper is to use the individual tendencies of CO in the atmosphere to ascertain whether vertical advection can be considered dominant to such an extent that the atmospheric descent rates can be calculated by observing tracer motion. This cannot be achieved with only measurements, as there is not enough information to separate the contributions to the observed VMRs. A model that simulates accurately the observed evolution of atmospheric CO VMRs is used to separate the contributions, or tendencies.

The comparison of atmospheric CO measured by instruments and modelled with SD-WACCM is vital to determining whether the model accurately represents the CO VMRs that are observed in the atmosphere. Figure 1 has been enlarged, changed to landscape layout, and edited to make the data clearer. Table 2 lists the quantitative results of the comparison and shows the level of agreement between the model and the instruments, which is quite high for daily averages. An in-depth comparison of KIMRA, MLS, and SD-WACCM has been made by Hoffmann et al. (2012a), and the comparison is made in the current manuscript because there have been updates to the model and the datasets. Section 1 now includes this information:

"The values are similar to those found for earlier versions of the model and data (Hoffmann et al., 2012a), with differences mainly due to updates to the modelled CO (Garcia et al., 2014) and the data products (Livesey et al., 2015; Ryan et al., 2017)."

Section 4 has been edited to include the calculations that were made with the modelled CO, instead of the measured CO. We agree that this is more consistent and avoids the differences between the model and the instruments (Table 2). The results led to the same conclusions

because of the level of agreement between the modelled and measured CO, as stated in the original manuscript.
Section 1 has been edited to clarify the above points:
"The aim of this study is to assess the limits of the above assumption when using tracer measurements from remote sounders to derive rates of vertical motion in the middle atmosphere. Measurements alone do not provide enough information to enable separation of the contributions to changes in tracer VMRs, and so an atmospheric model must be employed. The specified dynamics version of the Whole Atmosphere Community Climate Model (SD-WACCM) is used to determine the relative contributions to changes in CO VMRs during polar winter. The results are combined with daily average modelled CO to estimate the error associated with descent rates calculated assuming pure vertical advection of the tracer. Three commonly used representations of the data are assessed: a local area above a specific location (Kiruna, 67.8˚ N, 20.4˚ E, in this case), a zonal mean at a certain latitude (80˚ N is used as an example), and a polar mean (60˚ - 90˚ N). The winters of 2008/2009 and 2010/2011 are used in the study as an example of a winter with a strong SSW and a winter with a relatively stable vortex, respectively. The rate calculations were also performed using CO measurements from the Kiruna Microwave Radiometer (KIMRA) and the Microwave Limb Sounder (MLS) (not shown), and the results lead to the same conclusion. This was expected due to the level of agreement found in a comparison of the modelled and measured CO (Sect. 2.4)."

Table 1 – Please put a "+" in front of any rising vertical motions to assure the reader that the "-" has not just accidentally been left out.
This has been done.

Figure 3 – The main point of this figure seems to be that "CO VMRs cannot be attributed solely to vertical advection." To make that point the authors need to put all of the contour plots on the same scale. As is, I'm not even convinced that "Tendencies due to resolved eddy diffusion (›˙I´S´ N›˙I ´ SŠ.˙I´S´ S›˙I´SS´) are the most variable", since I can't compare Xedd plots with the advection plots. If as a result of using consistent scales some plots are left blank, then it's certainly fine to reduce the number of panels and mention the negligible effect of certain terms in the text. And, as mentioned above, there should, in addition to the current 6 panels, be a panel showing "total CO tendency" from both measurement and model.
This sentence was supposed to refer to the fact that Xedd varies the most between positive and negative values. It has been edited in the new manuscript to correct this:
"Tendencies due to resolved eddy diffusion ($Xedd$) show the most variation between positive and negative values, …"

Figure 3 has been separated, by the locations (i.e., 67N, 80N, and polar average), into three figures so that the panels can be made larger and more easily read.
In originally making the figures, it was decided not to use the same colour range for each tendency because too much of the information is lost from the plots. Because of the varying values of the tendencies (within a winter and from year to year), there are times, for example, when all tendencies show relatively low values. With the same colour bar for each panel, the

information about relative influence is then lost. There are too many instances of this nature to choose a single colour range that includes the relevant information. Instead, it was decided to use both labelled contours and colour bars. While it is admittedly not easy to quickly determine the relative magnitudes of the tendencies from a glance, relevant information is not omitted from the figure. The separation of the Figure 3 into three, makes it easier for the reader to attain this information for each scenario in the edited manuscript.
Section 3.3 now emphasises that there are different colour bars and added contours:
"Note that the tendencies are plotted with individual colour scales to retain relevant information when there are low magnitudes, and labelled contours are added."

The chemistry and the two advection terms in Figure 3 generally seem to peak at 80km. Is there a physical reason for this (if so please explain) or is this related to changes in the model that occur near this level? In the text there is a comment about a chemical sink layer near this altitude. Secondly, please more explicitly explain the normalization applied to Figure 5.
The peak around 80 km in the chemical tendency is due to a layer of nighttime OH at this altitude, which acts as a chemical sink for CO. This is stated at the following points of the original manuscript:
Abstract. Page 1, line 16:
"It was also found that CO chemistry cannot be ignored in the mesosphere due to the night-time layer of OH at approximately 80 km altitude."
Section 3.3. Page 7, line 3-8:
"Changes in CO due to chemistry ($chem$) are small below approximately 70 km, but all cases show a sustained sink for CO during the winter in a layer at around 80 km altitude. The layer coincides with the location of a night- time layer of hydroxyl (OH) around 82 km altitude (Brinksma et al., 1998, Pickett et al., 2006, Damiani et al., 2010). OH is known as the dominant chemical sink for middle-atmospheric CO (Solomon et al, 1985)."
Conclusion: Page 11, line 9-12:
"The results also show a chemical sink for CO, present throughout polar night, due to the layer of night-time OH at approximately 80 km."

The higher values of the advection tendencies around 70 to 80 km are mainly due to two points: the first is larger magnitudes of the TEM circulation compared to lower altitudes before there is a turnaround in the direction of the circulation at higher altitudes. The circulation changes from *poleward and downward* to *poleward and upward*. The turnaround point is at approximately 95 km in WACCM.
The second reason is that the vertical gradient of CO, which is proportional to the TEM vertical advection, generally increases with altitude.
The following information is included in the edited manuscript:
"The advection tendencies show maximum values around 70 – 80 km for two main reasons. The first is the larger magnitude of the TEM circulation, compared to lower altitudes, before there is a turnaround in the direction of the circulation at higher altitudes, at which point the circulation changes from poleward and downward to poleward and upward (e.g., Lieberman et al., 2000;

Smith et al., 2011). The turnaround point is at approximately 95 km in WACCM (Smith et al., 2011). The second is the generally increasing vertical gradient of CO with altitude (see Eq. 1)."

The description of the normalisation has been clarified and now reads:
"For a given tendency, the daily values are separated by calendar month and averaged, to give a monthly mean tendency. The daily sums of the absolute values of all tendencies are also separated by month and averaged, to give a monthly mean total absolute tendency. The monthly mean tendencies are then normalised by the monthly mean total absolute tendency, and will be referred to here as relative strengths. Using absolute values for normalisation retains the sign of the individual tendencies and avoids a large spread in the results when there is a small denominator (i.e., when the tendencies cancel each other and their sum is near zero)."

In the conclusion, and elsewhere, the authors declare that using tracer isolines is "invalid". Yet, if I understand Figures 5 and 6 correctly, there are several months and altitude ranges (e.g. near the winter solstice in the lower mesosphere) where w* does seem to be the dominant term. A more nuanced conclusion would therefore seem to be in order.

Section 5 has been edited to provide more quantitative information that qualifies the statement in the first sentence of the paragraph, and elsewhere. The variation in the relative strength is discussed and the changes in tendencies with time from earlier is also emphasised:
"There are no months where the relative strength of other processes can be considered negligible compared to the relative strength of adv_w*. The closest approximations of this situation are at 50 km altitude in October and at 46 km altitude in November, when other processes contributes 13.7 % and 9.6 % of adv_w*, respectively. These percentages then vary significantly with altitude. For October, the value increases to 18.6 % at 46 km, 22.5 % at 60 km, and is 61.13 % at 80 km. For November, the value increases to 34.4 % at 54 km, and is 70.8 % at 80 km.
The results for the south polar average, in Fig. 6, are qualitatively similar to those for the north. The relative strength of adv_w* shows a maximum of ~0.8. Both hemispheres show a peak in chem at 80 km for most of winter (see Sect. 3.3). The relative strength of $Xedd$ is not as prominent at the south as the north, likely due to the higher stability of the southern polar vortex. The points at which the relative strength of other processes is smallest compared to adv_w* are at 56 km in April (8.3 %) and at 46 km in May (6.8 %). For April, the value increases to 22.5 % at 46 km and 21.5 % at 66 km, and is 56.9 % at 80 km. In May, the value increases to 16 % at 54 km, and is 69.1 % at 80 km.
For the 10 days directly before and after SSWs, in Fig. 7, the relative strength of adv_w* is less than 0.5 at all altitudes. $Xedd$ is strong below 60 km, such that the relative strength of other processes has a larger magnitude than that of adv_w* at many altitudes. The relative strength of adv_w* shows a more oscillatory structure with altitude, and there is a local minimum at about 70 km in the data for 10 days after SSWs. There is also a positive peak in the relative strength of Xk_{zz} after SSWs at this altitude.
Aside from considering what value would classify as negligible, the significant variation in strength of other processes compared to adv_w*, over altitude, adds complexity to the method of following a tracer over an altitude range to determine the descent rate. One must also

consider that while this section discusses monthly averaged data, tracers are often followed for several days to determine the changes in altitude over that time, and that the magnitudes of each tendency can vary significantly over this time scale (see Figures 3, 4, and 5)."

The referred to statement in Section 6 has been edited to emphasise that the conclusion is indicated by the results, from all sections, using SD-WACCM.

Section 6:
"The results of the previous sections, using SD-WACCM, are clear on one indication, that the assumption of observed changes in CO VMRs being solely due to vertical advection is not a valid one."

The referenced sentence in the conclusion has been edited to emphasise that this conclusion is indicated by the results from all sections. Statements of the main results have been added/edited in the conclusion as follows:
- *"The results show that dynamical processes other than vertical advection cause non-negligible changes in CO VMRs during winter, and particularly directly before and after sudden stratospheric warmings when eddy transport can become dominant."*
- *"Significant changes in CO tendencies from SD-WACCM occur on the order of days. The results also show a chemical sink for CO, present throughout polar night, due to the layer of night-time OH at approximately 80 km."*
- *"Rates of atmospheric motion were calculated when assuming only vertical advection, and corrected rates were calculated by including tendency information for all processes. The differences between the two results are of the same order as the calculated rates, and the rates are prone to showing opposite directions for the mean vertical wind."*
- *"The "true" rate of atmospheric descent appears to be masked by sinks of CO, and by transport processes that oppose the tendency due to vertical advection"*
- *"Monthly mean relative tendencies for CO show that the summed magnitude of processes other than vertical advection can constitute a large fraction of the changes in CO VMR. For a given month, the summed magnitude of the other processes, relative to vertical advection, changes by several tens of percent over the altitude range under investigation."*
- *"The results suggest that there are no months during polar winter when vertical mean advection dominates the budget of CO to such an extent that vertical mean velocity can be accurately derived within the altitude range."*