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.

**Response to Reviewer 4**

**First review of Ryan et al. entitled "Assessing the ability to derive rates of polar middle-atmospheric descent using trace gas measurements from remote sensors" for publication in ACP. The authors use the SD-WACCM model to argue that processes other than vertical advection of CO are important in the calculation of polar winter descent rates in the upper stratosphere and mesosphere. The paper is well written and the results will be of interest to the scientific community. I recommend publication after the following revisions.**

**—General comments— I echo here a comment made by another reviewer that the authors need to first show consistency between the CO measurements and the evolution of CO in SD-WACCM. If the CO tendencies between the obs and the model do not agree then it's not appropriate to use the obs to calculate w-corrected.**

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 he modelled and measured CO, as stated in the original and edited manuscript.

**Overall, both in the abstract (maybe even the title), throughout the paper, and in the conclusions, the authors need to emphasize that these results are based on SDWACCM. Report quantitative error estimates to vertical motions derived from tracers instead of using provocative language like "found to be invalid".**

The referenced sentence, from the conclusion, has been edited to emphasise SD-WACCM and to clarify that the conclusion is indicated from the results:

*"An assessment using SD-WACCM indicates that a commonly used approximation of the vertical mean velocity of the atmosphere,* $\overline{w*}$ *, using tracer (CO in this case) isolines is not valid, and an alternative interpretation of the rates derived from trace gas measurements is suggested: an effective rate of vertical transport for the given trace gas."*

The second line of the abstract states that SD-WACCM is what is used to assess the assumption of dominant vertical advection:

*"Using output from the Specified Dynamics Whole Atmosphere Community Climate Model (SD-WACCM) between 2008 and 2014, tendencies of carbon monoxide (CO) volume mixing ratios (VMRs) are used to assess a common assumption of dominant vertical advection of tracers during polar winter."*

The edit to use the SD-WACCM CO profiles instead of the measurements for calculating descent rates is now also mentioned in the abstract:
*"SD-WACCM CO profiles are combined with the CO tendencies to estimate errors involved in calculating the mean descent of the atmosphere from remote sensing measurements."*

The calculation of w*_corrected is somewhat crude (as mentioned in the paper and now expanded upon in the new manuscript), as it combines tendencies in the TEM formalism with values derived using CO VMRs in the atmosphere. These points are now emphasized in Section 4 and it is made clearer that the goal of w*_corrected is to get a qualitative estimate of the errors that may be incurred by assuming pure vertical advection when using tracers to calculate w*:
*"The resulting rate is called* $w_{CO}\ corrected$*. This could be considered a crude approach, combining daily averaged CO output with CO tendencies calculated using the TEM formalism, but the aim here is to provide an estimate of the errors that may be incurred by neglecting influences on CO other than vertical advection. In any case, the results involving* $w_{CO}\ corrected$ *are discussed in a qualitative manner, instead of for quantitative error analysis."*

The conclusion contains the qualitative information from Section 4 about the estimated errors that are found using w_CO_corrected:
*"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 corrected rates more closely match the TEM vertical wind velocity from SD-WACCM, but both results using CO show smaller magnitudes relative to the TEM vertical wind, in agreement with the work of Hoffmann (2012b). 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."*

A more quantitative analysis of the relative influence of tendencies is now included in Section 5 offering a clearer picture of the results as they relate to an approximation of pure vertical advection when deriving descent rates. The reader is also reminded of the daily tendencies showing significant variation on time scales of a week:
*"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).*"

**Figures 1, 3, and 4 are too small, bordering on illegible. In many cases all of the figure panels shown are neither introduced nor discussed. Please reduce the content of each of these figures to simply support the key point being made in the text.**
Figure 1 has been enlarged, changed to landscape layout, and edited to make the data clearer.

Figure 3 and 4 have been separated into three figures, according to the different scenarios assessed: above Kiruna, 80N zonal mean, and north polar average). The panels have also now been made larger.

For Figure 1: Section 2.4 indicates that Table 2 shows the correlation and regression coefficient for each of the panels in Figure 1. The caption for Figure 1 states that there are four altitudes shown (one for each panel), and indicates that more information can be found in Section 2.4 and in Table 2:
*Figure 1: Comparisons of daily CO VMRs from KIMRA, MLS, and SD-WACCM above Kiruna for 2008 through 2014. Values are displayed at 46, 56, 66, 76, and 86 km altitude. Correlation and regression coefficients for the datasets are given in Table 2. See Sect. 2.4 for details."*

For Figure 3: The caption incorrectly stated that the tendencies shown in the panels are described in Section 2.4. This should have said Section 3.1 and has been fixed:
*Figure 3: 11-day running mean tendencies of CO (in ppmv/day), calculated using daily averaged SD-WACCM output. Tendencies shown are for 67° N for the winters of 2008/2009 and 2010/2011. See section 3.1 for a description of the tendencies which are represented in the TEM continuity equation."*
Section 3.3 discusses each of the tendencies that are plotted in Figure 3. Each panel is not sequentially discussed, but rather the relevant points, as well as similarities and differences between the scenarios.

For Figure 4 (original manuscript): w_CO_corrected and the difference between that and w_CO were not mentioned in the caption. This has now been fixed:

*"Figure 6: Rates of vertical motion, in km/day, calculated by tracking CO VMRs over time. $w_{CO}$ is calculated using SD-WACCM CO profiles. $w_{CO}\ corrected$ is calculated using a combination of SD-WACCM CO profiles and TEM tendencies (see Sect. 4 for details). The difference between the two rates of descent is also shown. The results plotted are for above Kiruna for the winters of 2008/2009 and 2010/2011. Contour lines are spaced by 0.2 km/day. Areas with tightly packed contours (black areas) occur when there are very low CO VMRs and the calculation method is unreliable. White areas are where a CO VMR could not be tracked within the shown altitude range. The start date of the SSW on January 28$^{th}$, 2009, is shown with a vertical green dashed line."*

**Figures 3, 4, and 8: swap the color bar to be blue for negative values and red for positives. Whenever possible, hold fixed the color bar range so that comparisons between winters and between tendency terms can be made.**
As the swapping of colour placement is a subjective matter, we will keep the current colour choice.

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 and Figure 4 into three, makes it easier for the reader to attain this information for each scenario in the edited manuscript.

**—Line-by-line comments—**

**Abstract Line 16 - "the relative importance of vertical advection is lessened: : :" – by how much? Give %**
The sentence has been edited to read
*"The relative importance of vertical advection is lessened, and exceeded by other processes, during periods directly before and after a sudden stratospheric warming, mainly due to an increase in eddy transport."*
The magnitudes of the processes are variable and to come up with a percentage would require choosing a single reference point with which to compare. The point being made here is that vertical advection cannot be considered as dominant, which is indicated by the fact that other processes become more important.

**1 Introduction Page 2, lines 25-35: ": : :defining the edges of the polar vortex is not straightforward." – cite Harvey et al. (2009) and Harvey et al. (2015)**
These references have been included in the edited manuscript.

**2.1 KIMRA, Page 3, line 29: does "average precision" mean daily average?**
Average precision refers to the fact that the precision varies somewhat from profile to profile. The sentence has been edited to say this and to give the approximate time resolution of a measurement:
*"The average precision (values can vary from one profile to another) of wintertime KIMRA CO VMRs range from 0.06 ppm at 46 km altitude to 2.7 ppm at 86 km. The average time resolution of a CO measurement is around 2 hours. KIMRA CO data presented in this work have been averaged to give daily profiles."*

**2.2. MLS, Page 4 line 7 - Does "highest pressure level" refer to the pressure level at the highest altitude?**
The sentence has been edited to clarify that is it is the pressure level at the highest altitude:
*"… and have a maximum (largest) precision of 11 ppm at the highest (in altitude) pressure level."*

**Line 8 - Does "averaged to produce daily profiles" in some spatial region?**
Yes, the information was originally in Section 2.4 but has been moved to Section 2.2:
*"MLS data presented here are within ± 2˚ latitude and ± 10˚ longitude of Kiruna, and have been averaged to produce daily profiles."*

**2.3 SD-WACCM – Given the fallibility in MERRA winds (mentioned in the intro) in the upper stratosphere and their inability to properly model the elevated stratopause in February of 2009, what (if any) impact does this have on SD-WACCM and the conclusions?**
Insofar as the actual winds are not well known in the upper stratosphere, then SD-WACCM may not reproduce the precise evolution of a given elevated stratopause event. However, WACCM can, and does, produce realistic elevated stratopause events (de la Torre at al., JGR, 2012; Chendran et al., JGAR, 2013), which is what counts as regards evaluating the effects of such variability on the behavior of CO.

The model can simulate accurately the statistical properties of elevated stratopause events, even if it does not simulate precisely the weather. The imprecise simulation of a specific elevated stratopause event does not materially change any of the conclusions of the paper.

In the comparison of CO from instruments and models, as in the manuscript, imprecise simulation of a specific elevated stratopause event may cause differences in the measured and modelled CO profiles. Looking at January 2009 in Figure 1 (enlarged version in the edited manuscript), SD-WACCM captures the quick decrease and subsequent increase in CO VMRs seen by both KIMRA and MLS at the time of the SSW. The differences seen between the CO profiles on a daily timescale are reflected in the regression coefficients and correlations listed in Table 2.

**2.4 CO VMR comparison, Page 5 Figure 1 is inadequate. Please compare the model and measured CO in a comprehensive way that convincingly demonstrates that CO**

**tendencies are in agreement.**
As the comment overlaps somewhat with the first general comment, there is some repetition in the answer.

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.

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. A more in-depth comparison of KIMRA, MLS, and SD-WACCM has been made by Hoffmann et al. (2012), 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 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)."*

**Figure 2 – increase panel size and symbol size. Reword last line of the caption to be "Parcel positions on Jan 28th (start of the 2009 SSW) is indicated by black asterisks."**
The panels and symbols have been made larger and the sentence has been edited to above.

**3.3 Tendencies of CO during Arctic winter Page 6, line 28 – ": : :in depth analysis is not made as it is not the focus of the study." – Then can the results be summarized in fewer than 36 panels?**
Thorough analyses of the tendencies trace gases have been made as the sole focus of a paper (e.g., Monier and Weare, ACP, 2011). Such an analysis is not warranted in the current manuscript, but the tendencies are an essential part in understanding the processes that effect CO.

The figure has now been separated into three, according to the different scenarios. The different scenarios are used to reflect the predominant ways that the tracer data are used: a point measurement (from a ground-based instrument), a zonal mean at a specific latitude (most often from satellite data and sometimes from ground-based), and a north polar average or vortex average (from satellite data). The two winters are used as examples of a winter with a SSW and one without.
With the figures separate, the reader can now clearly see the tendencies for both winters of a scenario, and focus on one scenario if they so choose.

**Page 6, line 31 – ": : :decrease in CO VMRs" add "in the upper mesosphere" – Does this mean air is ascending there?**
The sentence has been edited to contain the addition.
While the vertical advection term refers to changes in CO due to ascent and descent of air, the molecular diffusion and eddy diffusion terms are diffusive in nature.
The effect of molecular diffusion on a trace gas can be regarded as the sum of two contributions: diffusion, which is proportional to the second derivative of the mixing ratio, and a vertical drift velocity, which is related to differences in the molecular mass of the trace gas compared to the molecular mass of the background atmosphere. The mass of CO is close to that of the background atmosphere and so CO has a small drift velocity.
The effect of eddy diffusion also depends on the gradient of the mixing ratio as well as the diffusion coefficient. The process is a related to the turbulent diffusion associated with gravity wave breaking.

**Page 7, first paragraph –**
**Mention different color scales. Give relative magnitudes wrt w*, i.e., chem is 10% of w*. Can we interpret negatives = ascent/poleward and positives = descent/equatorward (or is it not that simple)?**

The different colour scales are now mentioned, and there have been additions to the quantitative description of each tendency, and information added about what the sign of the advection tendencies indicates about the direction of air motion:

"*Figure 3 plots the wintertime tendencies of CO (RHS of Eq. (2)) for 2008/2009 and 2010/2011. The zonal mean tendencies are shown for the three scenarios of 67˚ N, 80˚ N, and a north polar average (60˚ – 90˚ N), and are plotted as an 11-day running mean. Note that the tendencies are plotted with individual colour scales to retain relevant information when there are low magnitudes, and labelled contours are added. In the context of a point measurement at Kiruna, a full rotation of the vortex is on the order of 10 days (assuming a zonal wind speed of 20 m/s at 67˚ N). Relevant comments on the results are provided here but an in-depth analysis is not made as it is not the focus of the study. Molecular diffusion ($Xmol$) generally causes negligible changes in CO, compared to other process, below approximately 83 km, and shows little variation between different scenarios and winters. Above that, the magnitudes increase quickly, with tendencies < -0.1 ppm/day in the altitude range shown here. Unresolved eddy transport ($Xk_{zz}$) is also negligible below approximately 75 km, but can show tendencies < -0.2 ppm/day above that altitude for short times (order of a week). Significant variation is seen for the different winters. Both processes tend to cause a decrease in CO VMRs throughout the winter in the upper mesosphere, agreeing with results of Smith et al. (2011). For comparison, vertical advection ($adv\_w*$) at these altitudes shows positive tendencies ranging from < 0.2 to > 1.6 ppm/day. Changes in CO due to chemistry ($chem$) are small below approximately 70 km, but each scenario and year shows 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). $chem$ tendencies are stronger at 80˚ N compared to 67˚ N, with magnitudes reaching more than 0.3 ppm/day in November and December 2010, ranging from approximately 10 % to 50 % of $adv\_w*$ over that time. The results suggest that CO chemistry cannot be ignored in the mesosphere during winter. Tendencies due to resolved eddy diffusion ($Xedd$) show the most variation between positive and negative values, mainly at 67˚ N because of proximity to the edge of the polar vortex. The north polar average shows that $Xedd$ generally reduces CO VMRs during the winter and, above ~ 70 km, has magnitudes greater than 25 % of $adv\_w*$ for time scales of a week. The largest tendency in CO is from $adv\_w*$, and causes an almost constant increase in CO VMRs throughout the winter, before reversing when the TEM vertical wind changes direction in Spring (visible in all $adv\_w*$ plots). The increase is due to the downward motion of air and the positive gradient of CO VMR with altitude. The tendency is stronger at 80˚ N compared to 67˚ N due to a stronger vertical component of the residual circulation at the higher latitude (Smith et al., 2011, and see Figure 2). A signature of the major SSW in 2009 can be seen in the $adv\_w*$ tendency for that year, with a decrease and eventual change to a negative tendency. A negative tendency generally indicates ascent of air at this time. For some time directly afterwards, the tendency has a stronger positive magnitude than before. This agrees with observations of stronger vertical motion above the pole after a SSW (see references in Table 1). There is also a brief change to a negative $adv\_w*$ at 80 N, around 80 km altitude, in early January 2011. This coincides with a relatively strong positive value for $Xedd$ at the same time and location, indicating strong wave activity. The CO tendency from horizontal advection*

*($adv\_v *$) is negative almost everywhere. This is expected, considering the direction of $\overline{v*}$, toward the winter pole, and the low-to-high gradient of CO from lower to higher latitudes in the winter hemisphere. The magnitude of the tendency decreases in spring in each scenario and year. but a change of sign is not obvious by the end of April. 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). At 67˚ N, the magnitudes of $adv\_v *$ are roughly half that of $adv\_w *$, and at 80˚ N they are roughly one fifth. Considering this observation alone, changes in CO VMRs cannot be attributed solely to vertical advection."*

**Page 7, first paragraph, line 7 – ": : :because of proximity to the edge of the polar vortex." – No, both are well inside the vortex core.**

The polar vortex is not a fixed structure in space; it moves and can be distorted.
Kiruna has been shown to be inside, outside, or in the edge of the polar vortex on many occasions (e.g. Kopp et al., JGR, 2003; Ryan et al., AMT, 2016, Ryan et al., ESSD, 2017).
The same has been shown for Eureka, which is at 80N (e.g., Bird et al., JGR, 1997; Batchelor et al., AMT, 2010).
One can expect Kiruna to experience more variability with respect to the edge of the polar vortex, due to planetary scale waves, for instance, because of its location in latitude.
Some of the references in Section 1 of the current manuscript, like from Manney et al., describe the movements and distortions/splitting of the north polar vortex.

**Page 7, line 14 – "There is also a brief change to a positive" – do you mean negative?**

Yes. Thank you. This has been fixed.

**Page 7, lines 19-20 – This is very useful. Please do this for all tendency terms.**

Quantitative information has been added for each tendency term, with the other additions to Section 3.3.

**4 Rates of vertical motion with KIMRA and MLS Page 7, lines 30-31 – ": : :the concentration is adjusted using the tendencies of the continuity equation: : :" add "from WACCM". This section will gain credibility after showing that the model and observed tendencies are in agreement.**

As the comment overlaps somewhat with an earlier comment regarding Section 2.4, there is some repetition in the response.

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.

**Page 8, lines 17-20 – please reword. Are derived descent rates stronger because they need to counteract the opposing terms?**

The lines have been reworded, and an additional sentence added for clarity:

*"… the values of $w_{CO}$ are generally of a smaller magnitude than $w_{CO}$ $corrected$ during winter, meaning the calculated rates of descent are stronger if one accounts for CO tendencies other than vertical advection. This makes sense because, as seen in Figure 3, the other transport terms of the continuity equation (and the chemical loss term) tend to oppose the vertical advection term. In other words, the results indicate that the "true" rate of atmospheric descent is masked by sinks of CO, and by transport processes that oppose the tendency due to vertical advection."*

**Page 8,**
**line 23 – ": : :around the time of SSW is decreased: : :" could use an arrow in the figure to highlight this location.**

A vertical dashed line has been added to mark the date. The caption has been edited to reflect this.

**Figure 4 – can any of this information be shown using a scatter plot with w* along one axis and w*-corrected along another? The points could be colored by altitude. It is currently very difficult to look at the panels and understand the comparisons quantitatively.**
**How often would you get points with opposite signs? Is it more likely to get different directions up high or down low?**

As was stated earlier in relation to a previous comment, because the method combines daily averaged CO profiles and TEM tendencies, the aim of the calculations is to provide a qualitative error estimate. This is now emphasised in the text:

*"The resulting rate is called $w_{CO}$ $corrected$. This could be considered a crude approach, combining daily averaged CO output with CO tendencies calculated using the TEM formalism, but the aim here is to provide an estimate of the errors that may be incurred by neglecting influences on CO other than vertical advection. In any case, the results involving $w_{CO}$ $corrected$ are discussed in a qualitative manner, instead of for quantitative error analysis."*

The text, which describes the results of Section 4, now explicitly states that three main qualitative points are made about the results.

*"There are three main qualitative points, common to each scenario and year, that are evident from the results.*
*First, the values of $w_{CO}$ are generally of a smaller magnitude than $w_{CO}$ $corrected$ during winter, meaning the calculated rates of descent are stronger if one accounts for CO tendencies other than vertical advection. This makes sense because, as seen in Figure 3, the other transport terms of the continuity equation (and the chemical loss term) tend to oppose the vertical advection term. In other words, the results indicate that the "true" rate of atmospheric descent is masked by sinks of CO, and by transport processes that oppose the tendency due to vertical advection. Second, the differences between the two rates are often of the same order as $w_{CO}$.*

*Third, the signs of $w_{CO}$ and $w_{CO}$ $corrected$ are often opposite, meaning the calculated direction of air motion is prone to change when accounting for CO tendencies other than vertical advection. In each example for 2008/2009, the magnitude of the positive (upward) motion around the time of SSW is decreased for $w_{CO}$ $corrected$ compared to $w_{CO}$. After the SSW, and into March, the strongest descent values are seen around 70 – 80 km in $w_{CO}$ $corrected$, compared to values of ascent seen in $w_{CO}$ at the same location."*

The panels are now larger because of the separation of the figure into the three scenarios., so the reader can better observe the times/altitudes when the two rates have different signs, which is indicated by them having different colours.

**Figure 5, 6, and Page 9 – refer to 60-90 as the "polar cap" – not the pole.**
"North pole" or "south pole" are not used in the edited manuscript. "… polar average" is used.

**6 Discussion, Page 10 line 2 - ": : :not a valid one." Add "according to SD-WACCM" line**
The line now reads:
*"As the results here using SD-WACCM indicate that the commonly used approximation of $\overline{w*}$ with $w_{\chi}$ (using tracer observations) is not valid, we suggest an alternative interpretation of $w_{\chi}$: as an effective rate of vertical transport for the trace gas $\chi$."*

**30 – Reword ": : :are representative for the complete mesospheric air: : :"**
The line now reads:
*"… but with an assumption that the overall dynamic effects on CO are representative for mesospheric air, and so $w_{CO}$ is representative of $w_{\chi}$ for all tracers."*

**7 Conclusion, Page 11, line 17 – ": : :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." – This statement is too strong. Instead, give error estimates as a function of altitude and time over the winter.**
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."*
- *"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 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."*

Section 5 now contains a quantitative description of the results and a discussion of the results that indicate the conclusion with respect to the monthly data, and also reference the significant changes in tendencies on time scales of several days that is seen in the earlier section:
*"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 that 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.)"*