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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessing the ability to derive rates of polar middle-atmospheric descent using trace gas measurements from remote sensors" *by* Niall J. Ryan et al.

Anonymous Referee #2

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The authors use results from the WACCM model together with observations of CO from two sensors to investigate how well descent rates can be derived from a chemically (nearly) inert tracer with a strong vertical gradient in the altitude range 45-85 km (i.e., the mesosphere). It is found that considering corrections due to horizontal advection, turbulence, and chemical loss can imply differences in the descent rates derived from CO of more than 1 km/day particularly around strong sudden stratospheric warmings. Credibility is provided by a comparison of the modelled CO to the two observation data sets which generally show a good agreement. Considering that descent rates derived from these methods mostly lie in the range of 100-300 m/day, this is quite a large margin of error. Inert tracers are widely used to derive descent rates in the

polar winter middle atmosphere – not only in the mesosphere to estimate the input of thermospheric tracers, but also in the stratosphere to derive chemical ozone loss rates – and the paper provides an important caveat for these methods. I found the paper very clearly structured and well written, and recommend publication in ACP with a few minor changes.

Page 10, line 12: what does it mean if "w* corrected" derived from modeled CO using corrections from the model itself does not provide the model w*? If equation 1 is a correct description of all terms affecting CO in the model, then "w* corrected" should provide w* in a self-consistent way. I would say that this means that the terms in Equation 1 do not reflect what the model does to CO. I would expect that in the model, the resolved eddy term (Xedd) is not treated separately but as part of the advection scheme; in which case it is counted double if subtracted for derivation of "w* corrected". Does this make sense?

As I understand the term, the middle atmosphere comprised the stratosphere and mesosphere. As you really focus on the mesosphere here (the altitude range from 45-85 km) you might want to change the title of your paper to "polar mesospheric descent".

Page 1, lines 16-17: "The relative importance of vertical advection is lessened . . ." that means that other processes become more important, could you add a sentence which? (Turbulence, horizontal advection,. . .?)

Page 1, lines 25 and following: dynamical tracers have also been used (quite extensively) to derive stratospheric descent rates: to distinguish chemical ozone loss from dynamical processes.

Page 2, line 2-3: you could also reference Funke et al, 2014a, b; and Funke et al., 2017.

Page 2, line 16: . . . "and photochemical destruction in the upper mesosphere" limits

the altitudes at which it can be used to the stratosphere and lowermost mesosphere.

Page 4, section 2.2, line 6: can you also state the approximate altitude range of MLS (in km)?

Page 4, section 2.3, line 28: What exactly does daily output mean – once per day at a specific global time (a global snapshot with varying solar zenith angle) or at a specific local time (a global snapshot with nearly fixed solar zenith angle), or output of daily averages? For a dynamical tracer this probably does not make a big difference apart from some impact of the tidal phase in the upper mesosphere.

Page 5, lines 1-2, discussion of Figure 1: Figure 1 is too small – in my A4 one page per page printout each panel is about 1 cm high, making it very hard to distinguish the lines. You could more than double the vertical range of the panels without filling the page. Please do.

Page 5, line 7: "... but a systematic change in the results ... isn't found ..." despite the very cramped figure (see my previous comment) I do see a systematic difference between MLS and WACCM in early and late winter, i.e., in the buildup and decrease of the winter maximum: the winter maximum starts earlier and lasts longer in WACCM than in MLS, at least above 66 km.

Page 6, line 2-3: "The Prandtl number is 2 for the model runs in this work" I am not quite clear what this means. My understanding is that the Prandtl number describes a physical property of a gas or liquid, namely the relation between momentum diffusivity and thermal diffusivity; as such it should be an exact quantity. The Prandtl number of gases is usually given as lower than 1; for air, values around 0.7-0.8 are given. Does this change around the mesopause (where molecular diffusion becomes more important) compared to the lower atmosphere, or is this really used as a scalable fudge factor in WACCM? – I am aware that this is a feature of WACCM which has been implemented for a good reason; I'm not suggesting that this is changed. I am just curious what it means.

Page 6, line 5: The terms of Eq. 1 are "renamed" here. "Rewritten" suggests that you adapted the terms mathematically.

Page 6, lines 18 and following: I found it quite intriguing that air parcels ending above 66 km actually have their origin in the summer hemisphere. Maybe you can add a mention of this here.

Page 8, line 17 and following, discussion of Figure 4: again, I was intrigued to see that differences between wco and wco corrected sometimes are larger than 1 km / day: two to ten times larger than (most) estimates of descent rates based on tracers as given in Table 1. That really is a big discrepancy.

Page 10, lines 7-12: here you compare w* from the model (Figure 8) with values derived from tracer observations (Figure 4) – it would certainly be easier to follow your argument here if a) the panels in Figure 4 were larger, and b) more importantly, the scale of the colour bars was the same in Figure 4 and 8. It is difficult to appreciate that the values of "w* corrected" provided by tracer observations in 60-90° (Figure 4) is really smaller than the values provided from model wind fields in Figure 8, as the scale in Figure 8 actually covers a smaller range (-1 to 1 km/day compared to -2 to 2 km/day in Figure 4).

Page 10, line 18: see my comment above – what does a Prandtl number of 2-4 mean?

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