

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

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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 (X_{edd}) 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

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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 w_{co} and w_{co} 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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