

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 #3

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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

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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).

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.

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 (δIŠŇδIŠŠδIŠŠδIŠŠ) 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.

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.

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

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seem to be the dominant term. A more nuanced conclusion would therefore seem to be in order.

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