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Interactive comment on "Mass-conserving coupling of total column CO_2 (XCO_2) from global to mesoscale models: Case study with CMS-Flux inversion system and WRF-Chem (v3.6.1)" *by* Martha P. Butler et al.

Anonymous Referee #1

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General Comments

The authors have created a method for interpolating global model mixing ratios at coarse spatial scales to the grid for higher resolution models such as WRF-Chem, and then evaluate the differences between the resultant simulations with identical surface fluxes against TCCON and co-located GOSAT soundings near the TCCON site for the concentrations and rawinsonde data for the winds. This activity is useful and interesting for the community of regional tracer modelers, but the conclusions don't seem to demonstrate the utility of the boundary condition interpolation technique, and they

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certainly don't imply the added value of the regional modeling approach. While this negative result is in itself important, some further evaluation is required to understand why this is the case before I can recommend publication of these results. Specifically, evaluation against aircraft data to better understand the model-model differences in the tracer distribution in the vertical dimension, and evaluation against GOSAT data spatially. The tremendous amount of NOAA surface and tower data would also be extremely useful for differentiating between the PBL dynamics, as would meteorological analysis of the PBL differences between the two models. This represents an expanded scope for the manuscript, but the introduction of the boundary interpolation alone does not represent a significant scientific advance of sufficient scale to warrant publication.

2.3.1 I wonder how sensitive your results are for different diurnal cycles. Particularly at higher spatiotemporal resolution, this could be important for matching observations. It does simplify the interpretation vs. the parent model, though.

2.3.2 To call the method "mass conserving" would suggest that the XCO2 values at the boundaries should be much closer than 0.1ppm, right? I understand that the propagation into the domain might lead to these differences, but I would expect the overall differences at the time of interpolation to be tiny. Perhaps you could demonstrate that the actual mass is conserved, even if the column average mixing ratio is not (due to different surface pressures). Maybe some of the mass is lost in the upper 50hPa? Later on it says that you are using the CMS-Flux mixing ratios above 50mb, which makes this difference even more confusing.

Section 3 It seems that a lot of insight could be gained from comparisons to GOSAT in a spatial context, rather than just the model-model differences at simulated GOSAT sounding locations and times. Why is this not shown? Certainly the comparison in 3.2 at the Lamont TCCON is part of this, but the spatial information could shed light on the boundary condition effects, etc in other parts of the domain. This is reinforced by the very small bias of GOSAT at the Lamont TCCON site relative to the models. Remember that 0.5ppm difference in the column (particularly for a large scale average

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as shown in Figure 4) can amount to a significant difference that would be interpreted as a flux difference in an atmospheric inversion.

Further insight into the vertical mixing differences could (and should) be gained by comparison to the NOAA light aircraft time series, for example at Lamont. Since the fluxes are the same in each model, the only difference would be the transport.

Section 4 This would benefit from a comparison with the results recently made available in Schuh et al (2019), in which the authors examine the differences in GEOS-Chem (which drives CMS-Flux) and TM5 (which uses a different reanalysis). In particular, the authors look at the differences in vertical mixing and try attribute these differences to the way convection is handled in a rough way. They also draw conclusions about broad scale flux inference from these differences. It is a complementary study that deserves some mention here.

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