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Interactive comment on "The impact of meteorological analysis uncertainties on the spatial scales resolvable in CO_2 model simulations" *by* Saroja M. Polavarapu et al.

Anonymous Referee #2

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The paper includes two parts. It first describes a new coupled meteorological and tracer transport model based on Environment and Climate Change Canada's operational weather and prediction model, and then discusses the predictability of CO2 due to initial state sensitivity and land and ocean surface states. While the paper devotes most of the space to describe the steps adapting the GEM-MACC to do tracer transport, both the title and abstract do not reflect such effort. I would recommend dividing the current paper into two papers: one is on model development that is more suitable for Geophysical Model Development (GMD), and the other is on CO2 predictability, which may be suitable for ACP. At the current stage, the paper reads more like a model development paper. The following are my detailed comments:

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Discussion paper

1) The term of mass-conservation is loosely used. In CO2 transport, what needs to be conserved should be the total number of molecules in the atmosphere, not mixing ratio.

2) The reason of using 24-hour tracer-transport forecast cycle is not well illustrated in the text. Though spurious gravity wave could be generated during forecast, NWP has been using filtering technique to reduce spurious gravity waves (Nezlin et al., 2009). In the off-line tracer transport model, the meteorology analysis fields are read in every 6 hours, while this paper uses 24-hour. The paper only compares the 24-hour forecast to other reanalysis products. The accuracy of 24-hour meteorology forecast compared to the analysis fields read in by GEM-MACC needs to be assessed. Also, the sensitivity of CO2 transport to the length of tracer-transport forecast-cycle needs to be quantified, which can be addressed by changing the forecast cycle to 6 hours.

Nezlin, Y., S. Polavarapu, and Y. J. Rochon (2009), A new method of assessing filtering schemes in data assimilation systems, Q. J. R. Meteorol. Soc., 135, 1059–1070.

3) Figure 5 and 6 qualitatively compare the GEM CO2 fields to CarbonTracker. A figure showing the difference between GEM CO2 and CarbonTracker will be more quantitative. Also, it would be helpful to include a column CO2 north-south gradient comparison. Figure 5 indicates that CarbonTrakcer and GEM may have quite different N-S gradient.

4) The explanations for the disagreement between obs and aircraft and some surface insitu observations are not convincing. Figure 3 a and figure 8 b and c show that the summer draw down at lower levels simulated by the GEM model is much stronger than observations. The authors attribute this to the accuracy of underlying fluxes (P15, L33), since the model simulated CO2 agrees better with the observations when using the posterior fluxes constrained by GOSAT. While the accuracy of underlying fluxes could be the reason, I think it is more likely due to the accuracy of convective transport because the aircraft observations are over NA where surface flask observations

are dense. On the contrary, both figures seem to indicate that the vertical mixing at lower levels is too weak during both summer and winter. I suggest the authors using MACC III that is constrained by surface flask observations to do one more CO2 forward simulations, and then compare GEM CO2 to MACC III CO2 fields.

5) In adapting the model to do CO2 transport, the authors do not include the horizontal diffusion (section 2.3). What is the impact of horizontal diffusion on CO2 fields? The authors at least can run an experiment including horizontal diffusion for few day and then compare to the control run. Within a few days, the mass conservation is not that critical.

6) In the second part of the paper, the authors try to quantify the CO2 predictability in weather time scales and seasonal time scales. The discussions are lack of physical interpretation of the results and the implication of the results for flux inversion. Unlike meteorology fields and other air quality variables, such as O3, CO2 transport and CO2 observations are mainly used to quantify surface fluxes, not for prediction; the CO2 prediction itself does not have any applications. Under this context, it is important to illustrate the connection between CO2 predictability discussed here and the CO2 flux inversions.

7) The authors simulate the analysis errors by shifting the met analysis by 6 hours. This would create large errors due to inaccurate description of diurnal cycle, which is especially significant over extratropics. Since ECCC has a hybrid approach to simulate background error covariance (page 10, line 24), the analysis error can be approximated by the spread of ensemble forecasts used in the hybrid scheme, which should be more realistic.

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