Response to Review 2

The impact of meteorological analysis uncertainties on the spatial scales resolvable in CO2 model simulations" by Saroja M. Polavarapu et al.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-346, 2016.

Original comments are in black text. Our responses are in blue text.

Unrequested modification: In addition to changes prompted by reviewers' questions, we decided to change the predictability metric from the square root of the global mean of zonal variance to a more intuitive one: global mean of zonal standard deviation. In doing so, a coding error in the original diagnostic was found that affected only vorticity and divergence: abnormally large normalized predictability error was produced in the stratosphere and mesosphere. Since the square root and global mean operators do not commute, the change in Figures 9-10 resulting from the change in diagnostic did not have to be small, but were in fact minimal. Thus the changes seen in the revised Figs. 9-10 are mainly from correcting the coding error.

We thank the Reviewer for the comments. We believe that the revised manuscript has greatly benefitted from the comments of both Reviewers.

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 CO_2 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 CO_2 predictability, which may be suitable for ACP. At the current stage, the paper reads more like a model development paper.

Response: Before starting this manuscript we had considered writing a separate paper just documenting the model. However, we felt that although it was an arduous, painstaking task to adapt GEM-MACH to simulate CO_2, this work alone would not justify publication. This is because of the intrinsic important of fluxes to CO_2 simulations. Since we do not yet estimate CO_2 fluxes, only the transport component of simulations can be assessed, and only to a limited degree since using retrieved fluxes from another model means that model's transport errors are convolved with ours. This is an important point that is demonstrated here. It makes sense to document a system which is semi-operational, near-real time, or which estimates fluxes. On the other hand, using a coupled meteorology and transport model has some advantages that we explore here, namely, the spatial scales on which meteorological uncertainties impact transport error. These are highly original and significant results relevant to the understanding of CO_2 assimilation results. This is what is described in the main portion of the paper. However, this being the first instance of the use of this model for greenhouse gas simulation, it was necessary to document the model components and show a few results to assure the reader that predictability results, which are necessarily model dependent, and shown in section 4 are generally relevant. However, we agree with the Reviewer that this was not well indicated by the title and abstract of the original manuscript. Accordingly, we have modified the title in the revised manuscript to: *Greenhouse gas simulations with a coupled meteorological and transport model: The predictability of carbon dioxide*. Moreover, the abstract, Introduction, and discussion have been revised to obtain a more coherent picture focused on the coupling of CO_2 and weather and climate. A new section was also added to connect the ideas of predictability and transport model error. Since the aspect of transport model error that is difficult to address with an offline model is the error due to uncertain meteorology, the use of a coupled meteorological and transport model is critical for this work.

The following are my detailed comments:

1) The term of mass-conservation is loosely used. In CO_2 transport, what needs to be conserved should be the total number of molecules in the atmosphere, not mixing ratio.

   Response: The mass budget of CO_2 has been a primary objective in the development of our modelling system. We spent a significant amount of time to make changes to the NWP model to ensure the conservation of the total amount of CO_2 molecules during the full length of the simulation. In section 2 we describe changes made to the model to express CO_2 in terms of dry mixing ratio and the effort made to conserve dry air in the model. Nevertheless we have revised the text to avoid any confusion. In section 2.2 the text has been modified to explicitly mention how we compute global mass from mixing ratio. In section 2.3, the line mentioning the conservation of mixing ratio has been removed. That statement had existed only to explain why we do not use horizontal diffusion, but it was removed to avoid any possible confusion.

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 CO_2 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.

   Response: Our experiments have shown that the quality of the CO_2 simulations does not necessarily improve with more frequent cycling. Figure R-2.1 below (not shown in the manuscript) indicates indeed that the spatial distribution of CO_2 is not sensitive to the cycling frequency. By doing 24 hr cycling we reduce significantly the amount of CPU without sacrificing anything in terms of quality of results. The actual impact on the CO_2 evolution was not large in terms of XCO_2 (Fig. R2-1). The differences seem random in both space and time (from animations of figures like Fig. R2-1). However, differences do accumulate in the stratosphere and mesosphere presumably due to different wave generation characteristics which impact the slow overturning Brewer-Dobson circulation. The 24h update yielded better (slower) transport in the middle atmosphere (for reasons not yet known). Although the mass of CO_2 in the stratosphere and mesosphere is not important, this difference in circulation meant that the 24h update cycle was preferred. As for filtering techniques, this was a concern when developing our system's configuration. However, we found that it had very little impact with a 24h cycle and was more expensive to run.

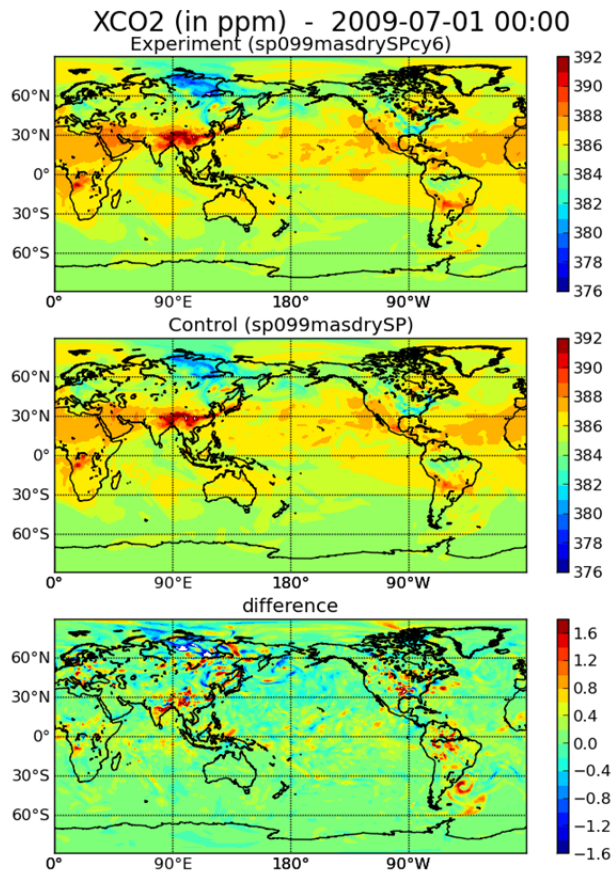

Fig. R2-1: The CO_2 state on July 1, 2009 00Z from 2 model simulations which are identical except for a 6h update cycle (top) and a 24h update cycle (middle) and differences between the top and middle panels (bottom). Difference patterns change daily but remain on fine spatial scales.

For this work, it is important to demonstrate that our 24h forecasts are within the uncertainty of reanalyses. As the base model is an operational model, its forecasts are routinely verified against measurements and compared with other operational centres (as noted in the original manuscript). But what is more relevant to the carbon cycle community, and to this paper, is to demonstrate that stitching together a sequence of 24h forecasts has relevance to a time series of reanalyses since the latter can be used to constrain flux inversions. Therefore we could compare our analysis errors to that of reanalyses. However, we went much further than that and demonstrated that the entire forecast (up to 24h) is as close to a reanalysis as are any two reanalyses datasets. The point is that reanalyses are not perfect and the difference in reanalysis datasets is a measure of their uncertainty (and is commonly used as such in the climate science community). Thus our full time series of disjoint 24h forecasts has no more uncertainty than the time series of reanalyses. This point is now explained in the last paragraph of section 4.1 of the revised manuscript.

3) Figure 5 and 6 qualitatively compare the GEM CO_2 fields to CarbonTracker. A figure showing the difference between GEM CO_2 and CarbonTracker will be more quantitative. Also, it would be helpful to include a column CO_2 north-south gradient comparison. Figure 5 indicates that CarbonTrakcer and GEM may have quite different N-S gradient.

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Response: Figures 5 and 6 have been replaced so that now one panel depicts the difference between GEM and CarbonTracker. The text has been correspondingly adjusted.

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 CO_2 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 CO_2 forward simulations, and then compare GEM CO_2 to MACC III CO_2 fields.

Response: In Fig. 3a the disagreement between the model and Alert observations was attributed to fluxes because different retrieved fluxes from GEOS-Chem (using GOSAT) observations did not have this mismatch. Thus, we can conclude a mismatch in long range transport between CarbonTracker and GEM-MACH-GHG. However, as noted by the Reviewer, the problem with the GEOS-Chem flux integration is that the observing system also changed along with the source of fluxes. While using MACC III posterior fluxes constrained by surface observations would help settle the issue, we also now have access to a similar dataset, namely, GEOS-Chem posterior fluxes constrained by only surface observations, as well as other new information. This additional information supports our original point of view. Firstly, a GEM-MACH-GHG simulation with GEOS-Chem fluxes constrained only by surface observations also agrees better with observations than when CarbonTracker fluxes are used (Fig. R2-2). Secondly, GEM-MACH-GHG constrained with CarbonTracker-CH4 fluxes also agrees well with measurements at Alert. Since the transport is identical for both CO_2 and CH_4 (both were carried in the same model) yet only CO_2 has a disagreement with surface observations at high latitudes (Fig. R2-3), the CarbonTracker CO_2 fluxes are clearly implicated. That is, the CarbonTracker transport errors do not match GEM-MACH-GHG transport errors in high latitudes in the autumn. With this additional information, we can now safely attribute the mismatch at high latitudes to the fluxes and hence to the mismatch of GEM and CarbonTracker transport at high latitudes. Figure S6 of the supplemental material was modified to include Fig. R2-2 and text was correspondingly modified. However, we did not add Fig. R2-3 to the supplemental material because our CH_4 simulations were not described in this article. This will be the topic of a forthcoming article (not yet in preparation). As for convective transport, our updraft velocities compare well to observed values, but we do not yet have tracer transport through the shallow convection scheme, and this could explain the overestimated vertical gradient seen in Figure 8. The shallow convection scheme is now described in section 3.5 and this point is now mentioned in connection with Fig. 8.

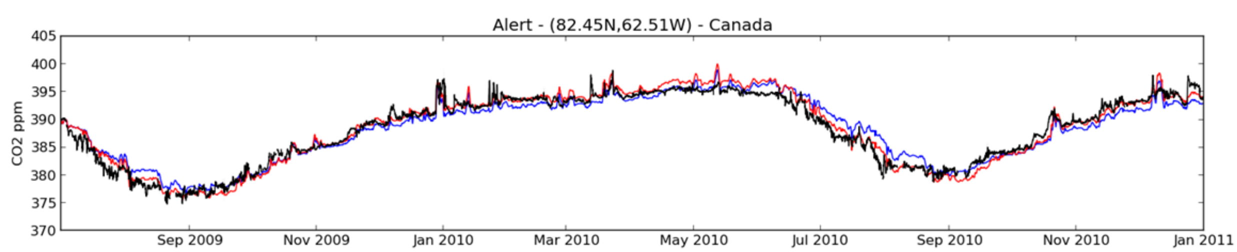

Figure R2-2: Time series of CO_2 at Alert. GEM-MACH-GHG simulations with GEOS-Chem posterior fluxes obtained with GOSAT (red) and with surface observations only (blue) are compared to measurements (black) for July 2009-Dec 2010. Note the agreement with observations in autumn for both model simulations.

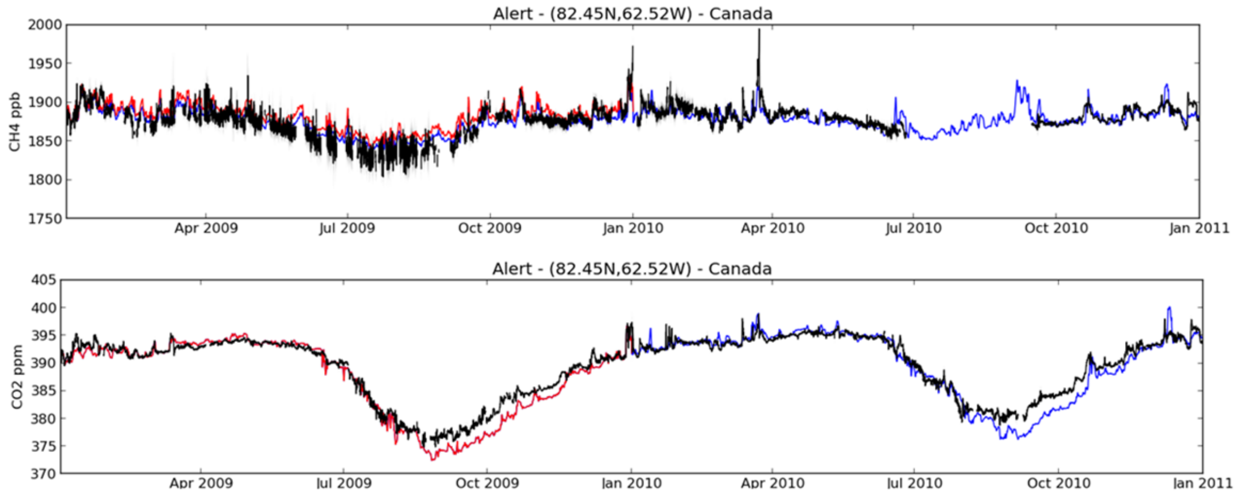

Figure R2-3: Time series of CH_4 (top) and CO_2 (bottom) at Alert. A recent GEM-MACH-GHG run with both CO_2 and CH_4 using posterior fluxes from CT2013B (for CO_2) and CarbonTracker-CH4 (for CH_4) (blue) is compared to measurements (black). Note that there is no disagreement in autumn for CH_4 as there is for CO_2 with the same model transport.

5) In adapting the model to do CO_2 transport, the authors do not include the horizontal diffusion (section 2.3). What is the impact of horizontal diffusion on CO_2 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.

Response: During the model development (a couple of years ago), we did in fact look at the impact of horizontal diffusion. Not surprisingly, the CO_2 field is slightly smoother (the diffusion uses a del-6 operator) but there is an impact on global mass conservation on the time scale of one year. As noted in the original manuscript (p.8, lines 13-14), given our desire for exact mass conservation, horizontal diffusion cannot be included. Nevertheless, for interest sake, we show here a 2-year-old plot of spectra from a model run with the standard high order (del-6 operator) (red), a comparable run with no horizontal diffusion (black) and another with greatly enhanced diffusion (del-4 operator) (blue). The impact of horizontal diffusion is on wavenumbers 100 and higher. Similar results are seen for other vertical levels and other dates.

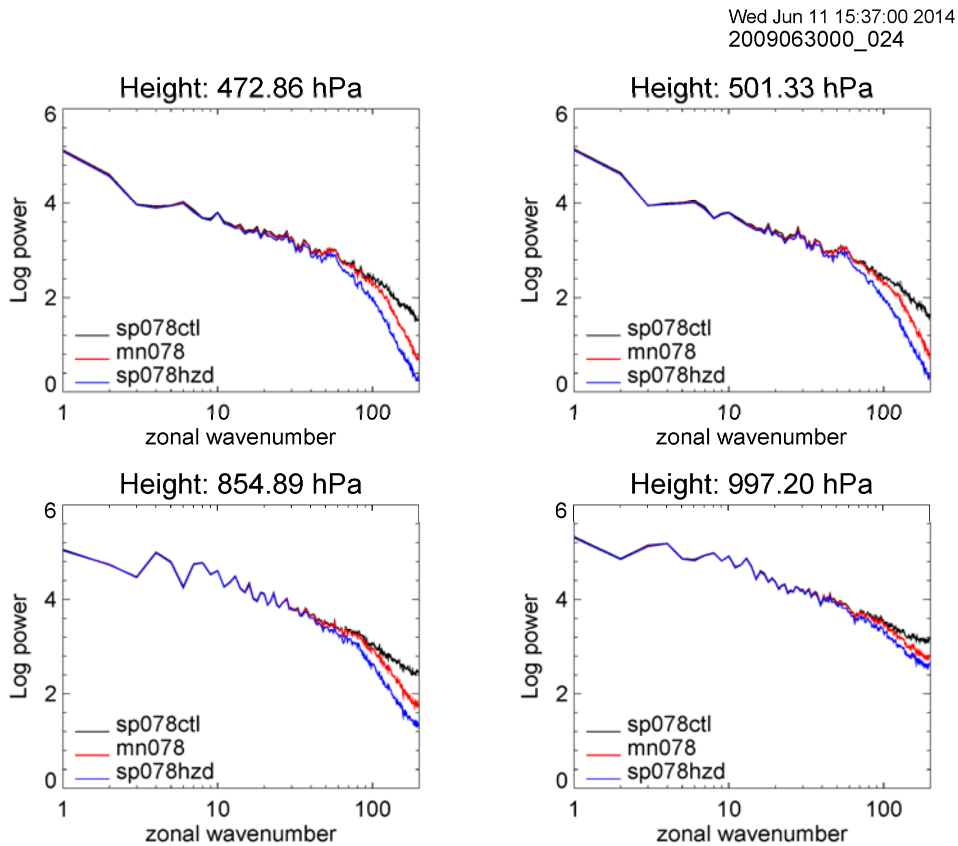

Figure R2-4: Spectra of CO_2 as a function of zonal wavenumber for various model levels whose approximate pressure is given above each panel. The black curves correspond to the control cycle while the red curves add horizontal diffusion with high order (del-6) filtering. The blue curves use an even stronger, less scale-selective filtering (del-4).

6) In the second part of the paper, the authors try to quantify the CO_2 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, CO_2 transport and CO_2 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 CO_2 predictability discussed here and the CO_2 flux inversions.

Response: We agree with the reviewer that it is important/necessary to identify the physical mechanisms which determine the CO_2 predictability. Indeed, a focus of this work is on the processes connecting meteorological and CO_2 predictability. For this reason there is an attempt to address it in section 4 where we discuss the impact of convective mixing on the error spectra. Aside from the consideration of spatial scales, atmospheric processes from stratospheric sudden warmings, to tropical modes of predictability, to atmospheric waves have been invoked in the discussion of results. It is exactly this type of analysis which is new to carbon cycle science and which is one of the novel features of this work.

A relevant matter for the flux inversion community is the use of transport model predictions (i.e. forecasts) in the computation of model-data mismatches. Transport model predictions are also performed with the fluxes retrieved from such an inversion to obtain CO_2 state estimates. The issue of transport model error (hence predictability) is of considerable concern to the flux inversion community. However, there may be a mismatch in terminology used by the weather prediction data assimilation community versus that used by the flux inversion community. To address this, a new section (section 2) was added to the revised manuscript to connect the concepts of predictability and transport error and to identify the components of transport error that are discussed in the article.

We recognize that the predictability of the CO_2 state has important implications for flux inversions. The new section connecting the concepts of predictability with transport error combined with the completely rewritten discussion section (the new section 6) address this issue. The abstract was also rewritten with this point in mind.

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.

Response: The reviewer is correct that errors in the diurnal cycle would be created by shifting analyses by 6 h. We had considered this issue but decided it was not critical because the shifting is done everywhere at a given universal time, rather than local time. Thus the shift will be at different parts of the diurnal cycle for different longitudes, so the bias introduced by the shift averages out for the meteorological fields. It was assumed that even with the covariation of the wind fields with the CO_2 flux over 24 h, the impact when averaged over longitudes would again average out given that we only considered global diagnostics. However, the only way to know for sure if this is true, is to remove the diurnal cycle from the 6h analysis differences and rerun the perturbed analysis cycle. Therefore, we have done this. It is straightforward (though time consuming) to do this by simply removing the monthly mean of each hour from the perturbation. Figure R2-5 shows the impact of removing the diurnal cycle from 6h analysis differences on spatial spectra. The Figure corresponds to Figures 11-12 of the original manuscript with the red curves depicting the CO_2 spectra for differences in CO_2 due to 6h analysis differences, whereas the cyan curve depicts the impact from 6h analysis difference with the diurnal cycle removed. The month chosen is July 2009 because this is the month for which the impact of the diurnal cycle is the largest. In Fig. R2-5 (top row) there is an impact on the largest scales, with much reduced analysis uncertainty when the diurnal cycle is removed. The impact is largest near the surface where the boundary layer diurnal variation is important and in the stratosphere and above where tidal signals are important. However, the conclusion regarding the lack of predictability for smaller spatial scales (where the red or cyan curves cross the reference spectra (black or blue)) remains the same. Additionally, the conclusion regarding the lack of information at small spatial scales (the asymptoting of the red or cyan curves to the black one) remains the same. Because there is an impact from removing the diurnal cycle on predictable spatial scales, Figures 11-13 have been updated in the revised manuscript by replacing the red curve with the one corresponding to the new cycle with analysis perturbations having the diurnal cycle removed.

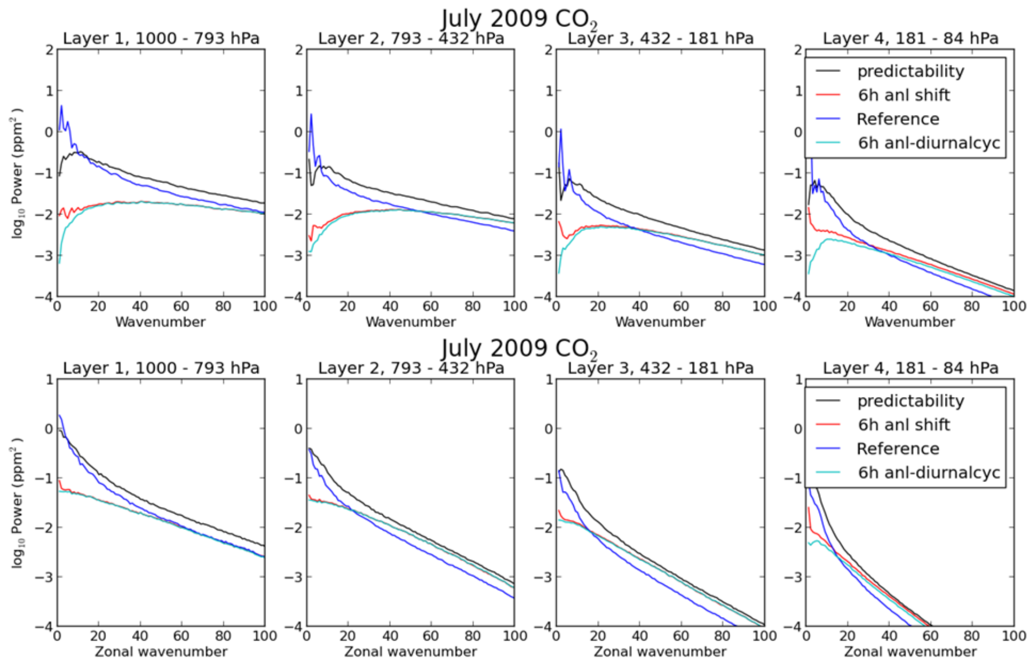

Figure R2-5: Spectra of CO_2 as a function of total (top row) or zonal (bottom row) wavenumber for July 2009 various model levels whose approximate pressure is given above each panel. The black curves represent predictability error while the red curves represent error due to a 6h shift in analysis. The red curves should be compared to the cyan curves which result when the diurnal cycle is removed from the 6h shift.

As stated in the original manuscript (p20, lines 9-11 and p25, lines 8-10), the ideal perturbation is a realization of analysis error, but dealing with operational systems poses significant logistic challenges. It is true that ECCC has a hybrid approach to obtaining background error covariances as of Nov. 18, 2014 but the hybrid system relies on a separate ensemble Kalman Filter system to provide realizations of forecast errors. It does not use nor does it compute analysis errors. However, ECCC also has a separate EnKF which is used for ensemble prediction along with the operational deterministic 4D-Var (i.e hybrid) analysis system. We had naturally considered the possibility of getting realizations of analysis error from the EnKF (though it uses lower resolution, a different model version and a lower lid from the deterministic system which produced the meterological analyses used here) but these were not archived by CMC in 2009-2010. The archives only start around 2011, and are not reliable so a full year of members could not be extracted. As noted in the original manuscript (p25, lines 11-16), we are devoting significant time right now to developing an ensemble Kalman Filter for greenhouse gas which, when ready, will give us exactly what we need. In the meantime, our proxy for analysis error (the 6h analysis difference minus the diurnal cycle) is useful (even though this proxy will be larger than analysis errors) since none of our main conclusions are affected by this choice, namely, that (1) some scales in CO_2 cannot be resolved due to the presence of analysis uncertainties, and (2) that for small enough spatial scales, the error spectrum due to analysis uncertainties asymptotes to the predictability spectrum hold regardless of the size of the error. Additionally, in the revised manuscript we stress that the proxy should over estimate analysis error.