

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

Anonymous Referee #1

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This paper presents the adaptation of the GEMS-MACH model to simulate atmospheric CO₂ within an NWP framework. An evaluation of the simulations using different types of observations is performed. The question of predictability associated with different aspects of the model (growth of transport errors, imperfect initial conditions, and impact of convection) is also addressed. The paper is very well written and the predictability study shows very interesting results that are relevant to the atmospheric composition and carbon cycle community. So I would recommend the paper to be accepted subject to minor corrections. A list of general and more specific comments can be found below.

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GENERAL COMMENTS

- The potential role of the CO₂ fluxes in the predictability of atmospheric CO₂ is not clearly presented. Although this is not the main focus of this study, the generally important role of the fluxes in the predictability of CO₂ should be emphasized. The relevance of the fluxes would become more evident if the predictability diagnostic would include a measure of the evolution of mean error with forecast lead time. In the paper two different surface fluxes are used but their differences are not explained. A description of the main differences (not only in formulation but in actual flux difference) would help the interpretation of the results in terms of flux errors. Can their difference be used to test the impact of flux errors on the predictability?
- The characterisation of analysis errors using a 6-hour shift seems an unrealistically large estimate of the analysis error. If that is the case, the results might not be indicative of the real impact of using imperfect analyses in CO₂ models (e.g. in flux inversions or in forward simulations), but will only provide an upper limit for scales that can be predicted even with the use of inaccurate initial conditions for the meteorology. I think it would be useful to compare this specification of uncertainty with the uncertainties provided by your analysis system.
- The comparison of the errors associated with imperfect analysis, the predictability and omission of convective transport as well as flux errors are done for 1 to 2 month-long simulations. It is not clear whether the same results would hold for shorter simulations (e.g. 5-day or 10-day simulations) when the predictability error is not saturated.
- Finally, it would be useful to have a clearer message of the implications of the findings regarding the impact of the different error sources for the carbon cycle community.

SPECIFIC COMMENTS

- The CAMS CO₂ analysis and forecasting system is using the Bermejo and Conde (2002) mass fixer since 2015 with the introduction of the high resolution CO₂ forecast. A paper has been recently submitted to GMD to document it:
A. Agusti-Panareda, M. Diamantakis, V. Bayona, F. Klappenbach, and A. Butz (2016): Improving the inter-hemispheric gradient of total column atmospheric CO₂ and CH₄ in simulations with the ECMWF semi-Lagrangian atmospheric global model (submitted to GMD)
- Page 11, line 15: Why should the the mass mixing ratio of CO₂ be adjusted to account for changes in the atmospheric pressure associated with analysis increments? Although they will affect the conservation of CO₂ mass in the model, the associated error is small and it does not grow with time. So why can't this error be considered part of the small error in mass conservation associated with model errors in atmospheric pressure?
- Page 13, line 14: How frequently are the retrieved fluxes from Carbon Tracker updated?
- Page 13, line 11: It is not clear what is the purpose of using two versions of the Carbon Tracker fluxes. What is the difference between CT2010 and CT2013B apart from the fact that the latter extends further ahead in time? A figure showing the difference in the seasonal cycle and the annual mean distribution might help to understand how large the differences are and where/when they are more pronounced.
- Page 14, lines 23-24: This line gives the impression that improving the diurnal cycle of CO₂ in the boundary layer is only a matter of improving the parameterisation of turbulent diffusion. Whereas the atmospheric CO₂ diurnal cycle near

the surface also depends on the diurnal cycle of the CO₂ fluxes. So improving the turbulence mixing parameterisation will not necessarily improve the CO₂ diurnal cycle, in particular if there are compensating errors in the retrieved fluxes. Also, since the retrieved fluxes are not using observations of the CO₂ diurnal cycle, their diurnal cycle will be mainly reflecting the prior fluxes. This means that the error in the atmospheric CO₂ diurnal cycle will have an even larger component from the CO₂ flux error.

- Page 14, lines 30-31: “The GEM-MACH-GHG simulations with different posterior fluxes are closer to each other than to CarbonTracker in the tropical Atlantic”. I see large differences with CarbonTracker in the Arctic and Tropical Pacific too. Again, it would be useful to know the difference in fluxes between CT2010 and CT2013. If their difference is very small, then it is not surprising that the two simulations are so similar and we mainly see the differences between the CT transport and the GEMS-MACH transport.
- Page 15, lines 1-2: I do not understand how the finding that the fluxes are more important for explaining the tropical structure can be linked to the finding that posterior fluxes are sensitive to prior fluxes in the tropics.
- Page 15, lines 24-25: The main reason why the biases here are lower than those from Massart et al. (2016) is because of the use of retrieved fluxes. If the retrieved fluxes from flux inversion systems are well constrained by the observed annual growth rate (which is similar in all background stations) then they should produce annual biases close to zero with forward models. The GOSAT data assimilated by Massart et al. (2016) is too sparse to be able to constrain the global growth rate by just adjusting the atmospheric concentrations using a 12-hour window.
- Page 15, lines 25-26: I don't agree with this hypothesis. The main difference between the results here and those by Massart et al. (2016) is the fact that the

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retrieved fluxes are well constrained by the observed annual global atmospheric growth rate from surface observations, which should be similar to the growth rate observed in most TCCON stations. The system used by Massart et al (2016) is not adjusting the fluxes using long assimilation windows and therefore it is having a harder time constraining the annual global growth rate.

- Page 15, line 30: excluding Eureka, Karlsruhe and Izana.
- Page 16, line 5: The statement “has GEM-MACH-GHG exceptionally good agreement” is not appropriate unless it is specified that the agreement is better than CT in the free troposphere but all the models have similar error magnitude in the boundary layer.
- Page 16, line 8: “excellent annual results *in the free troposphere* because . . . “
- Page 16, lines 12-13: In autumn the gradient seems to be “too large”. In the paper it says it is “too small”. Moreover, the explanation that the vertical mixing just above the boundary layer is too weak would not make sense if the gradient was too small.
- Page 16, line 15: The departure of the vertical gradient from those observed can only be attributed solely to the model formulation if the two posterior fluxes used are significantly different. In the paper there is no evidence that this is the case for the profiles used over Canada.
- Page 16, lines 17-18: On a season by season basis, I don’t think it is possible to say that “Overall, compare to Carbon Tracker, vertical gradients in GEM-MACH-GHG agree better with independent measurements in the mid troposphere but less well in the lower troposphere.” This is only the case for the annual mean, but as mentioned in the paper the annual mean does not reflect the errors associated with the transport, but it reflects that fact that the errors have opposite sign in different seasons. So I would remove this statement.

- Page 16, line 23: what about the errors associated with the unresolved (parameterised) transport?
- Page 16, lines 25-26: The assumption of the fluxes being “perfectly known” is unrealistic.
- Page 17, line 5: The synoptic variability of the biogenic CO₂ fluxes can also have an impact on the synoptic variability of atmospheric CO₂ (Chan et al. 2004 in Tellus B, Agusti-Panareda et al. 2014 in ACP).
- Page 17, lines 8-10: It is important to also mention that the predictability error should include the random error as well as the bias. This paper addresses mainly the limits of predictability in terms of variance but often the more challenging problem for models using NWP assimilation windows is how to deal with the large-scale growing biases in the background air.
- Page 18: The number of days where the forecast is deemed to have skill is just an empirical measure that is useful in the context of comparing different parameters and different factors (as mentioned in the discussion section). I would emphasize that the numbers of 2-3 days and 5 days should not be taken as a theoretical limit (see line 27 in the paper). The reason why the predictability seems to be longer in the boundary layer could be explained by the normalization of the predictability score, as well as the stronger influence from the prescribed fluxes. The normalisation of the random error will imply that the layers with larger zonal variability (e.g. near surface or in upper troposphere/lower stratosphere with well-defined zonally propagating waves) will have lower values of the predictability diagnostic. It is also worth noting that the specific predictability diagnostic used in this paper focuses on the ability of the model to simulate the expected zonal variability. However, there could be many other measures focusing on other aspects of the predictability (eg. anomaly correlations as done by Massart et al, 2014, or mean

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error growth) that are not shown in this study. These should be made clarified in the paper.

- Page 20, line 3: Which surface fluxes?
- Page 20, lines 20-21: It is not clear how the spectra shown in Fig 11 is computed, specially the specification of the total and the zonal wave numbers shown in Fig 12 and 13.
- Page 20, lines 27-28: Does this mean that the spatial scales of the predictability errors are only assessed for a 1 to 2 month simulation?
- Page 21: Although the emphasis is on the predictability of the meteorological parameters affecting the predictability of CO₂, the influence of the CO₂ fluxes in a real forecast setting (i.e. where the fluxes cannot be retrieved) should be emphasized. It seems as if they just play a secondary role as they are just mentioned as a remaining factor after all the others have been considered. A lot of emphasis is given to the role of the ocean and land surface conditions, but from Fig S7 I would say that there is still predictability at large scales even when those surface conditions are shifted by 3 months. I think this proves that at large-scales the fluxes are really the dominant factor for the predictability.
- Page 22, lines 1-2: I do not understand why the results are consistent with the transport biases acting at large-scales. Isn't the limit of predictability associated with imperfect analysis most relevant for small scales?
- Page 22, lines 13-16: I would say that the larger influence of the flux differences near the surface is not because the retrieved fluxes assimilated observations near the surface, but because any surface flux will have a much larger influence on the CO₂ near the surface than at upper levels. The diminishing impact of flux differences with height has nothing to do with whether the observations assimilated were near the surface, in the mid troposphere or for the total column, but

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- on the fact that the influence of any surface flux will always diminish with height because of the transport and mixing away from the source/sink region.
- Page 22, lines 28-29: This sentence is not clear.
 - Page 23, line 9: Can you associate the zonal spectra to the tropics exclusively? What about the Rossby wave in mid-latitudes?
 - Page 23, lines 24-25: I think it would be useful to add the range of errors found on the seasonal time-scale when you say that the model compares well with observations(e.g. within ± 2 ppm based on Fig 8).
 - Page 23, line 27: The statement that the gradient in the model is excellent can be misleading as it only applies to the annual mean. Since the gradient for specific seasons is what really reflects the transport uncertainties, I would say that the gradient is slightly overestimated in the free troposphere probably due to a lack of mixing in the model.
 - Page 23, line 31: This is not the first time when the predictability of CO₂ has been investigated (see Massart et al. 2016, ACP). The results from Massart et al. (2016) show the forecast of column-averaged CO₂ has skill up to day 5, so the 2-3 day is not a theoretical limit, but it is dependent on the diagnostic used. This should be clarified somehow.
 - Page 24, line 5: The climate predictability is important, but so is the predictability of the CO₂ fluxes.
 - Page 24, line 6: If I understood well, the impact of imperfect analysis is tested by shifting the analysis field by 6 hours. This is not equivalent to "a 6 h forecast error" as mentioned in the paper.

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MINOR COMMENTS

- Page 14, lines 1-2: It is difficult to see the blue line associated with the model in the middle panel.
- Figure 1: I think the sentences "...there should be 4 times as many blue boxes as shown. Some of these were omitted for clarify of presentation" could be misleading if the meteo analysis in the CO₂ model is only used at 00 UTC. If not, this should be clarified in the paper.
- Figures 3: It is difficult to see the blue line in the middle panel.
- Page 15, line 1 : replace “dynamics” by “transport”.

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