Atmos. Chem. Phys. Discuss., 14, C4033–C4035, 2014
www.atmos-chem-phys-discuss.net/14/C4033/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Forecasting global atmospheric CO_2" *by* A. Agustí-Panareda et al.

Anonymous Referee #2

Received and published: 23 June 2014

The publication describes a forecasting system for atmospheric CO2. As far as I am aware it is the first of its kind. At the current stage there is no assimilation of CO2 data, as a result of which deviations between the presented CO2 re-analysis and the evaluation data set are still sizeable. Scientifically the most interesting aspect, in my opinion, is the dynamical coupling with CTESSEL. The results provide some evidence that the carbon flux response to changing weather conditions is essential for reproducing the observed short-term variability in CO2. Unfortunately the evaluation of the experiment with an without fluxes varying on synoptic time scales is limited to only one of the sites (Park Falls). It is not clear to me why the other continuous measurement sites that are used in this study were not used for this particular experiment, which would have strengthened the scientific significance of the paper.

One obvious question that comes to mind when reading this study concerns the scien-

tific goals of a CO2 forecast system. It is discussed, but only in the summary section. I had expected this to be part of the introduction, which would clarify up front why the system is setup the way it is. A related question, which is not answered at all, concerns the accuracy requirements of the forecast system for achieving each of its goals. If that was quantified up front as well, the validation with independent data could be used to assess what the current system can be used for already, and what is needed to bring other objectives within reach. Without a target it is not really clear what the results of the quality assessment really mean.

It is not clear why the use of optimized fluxes is limited to the initializations at the start of each year. Besides their use to avoid that the global background diverges from the measurements, they could also have been used to verify the explanations that are given for concentrations mismatches in terms of shortcomings of the CTESSEL predicted fluxes (for example on the seasonal time scale).

Besides these general comments some more specific issues are raised below that need to be addressed to make this manuscript acceptable for publication in ACP.

SPECIFIC COMMENTS

Page 13910, line 23: I am wondering why only satellite data are mentioned here for the assimilation step. In order to reduce biases I had expected the assimilation to be driven by data that are tied to the WMO calibration standard, and therefore have low bias themselves. Using only satellite data, measurement biases end up influencing the forecast. This is problematic especially when forecast data are used as input to satellite retrieval schemes. In that case the origin of biases will become very difficult to trace back.

Page 13915: A reference is needed for the mass fixer. If it is not described in a publication than a short explanation of the method should be given here.

Page 13916: It is not clear how the anthropogenic fluxes will be updated to near-real time, when the system is run in forecast (i.o. hindcast) mode. The emission inventories lag behind real time by at least a year.

Page 13918: ‘optimized fluxes’. I guess that the results from Chevallier’s inversions are meant here. Up to this point these fluxes are only mentioned in connection with initial conditions. If they have a more general role in the paper then this should be explained somewhere. Otherwise a reference at this location suffices.

Page 13922: Like the onset of the growing season is introducing uncertainty, because of NEE switching sign, I had expected similar problems in fall when the reverse happens. This does not appear to be the case, however, which seems worth mentioning here.

Page 13925, line 26: I suppose what is meant here is that the model was sampled by interpolation to the coordinates of the measurements.

Appendix A2: The model is corrected for the wet fraction of the air mass, to derive dry air total column mixing ratios for comparison to TCCON. However, I suppose that what is simulated by the model is actually the dry air mole fraction already, and therefore there is no need for a humidity correction anymore.

Figure 6: Some info in the headers of the figure panels remains to be explained in the caption (if it is not important then please leave it out).

TECHNICAL CORRECTIONS

Page 13916, line 22: skip one ‘grid’

Page 13926: ‘performance’ i.o. ‘preformace’

Figure 7a: The symbols in the figure are too small to be seen.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 13909, 2014.