Atmos. Chem. Phys. Discuss., 14, C5619–C5625, 2014
www.atmos-chem-phys-discuss.net/14/C5619/2014/

Atmospheric Chemistry and Physics
Discussions

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

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Received and published: 5 August 2014

We thank the referees for their detailed and thoughtful comments, which will help to improve the presentation of our results. Both general and specific comments are addressed below. The technical comments will be included in the final response.

REPLY TO REFEREE 2

GENERAL COMMENTS

• *Unfortunately the evaluation of the experiment with and 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 are not used for this particular experiment, which would have strengthened the scientific significance of the paper

In order to evaluate the impact of the fluxes varying on synoptic time scales it is important to select sites which are strongly influenced by vegetation types that are well represented in the model. A part from Park Falls, the variability at other sites is either mainly influenced by advection or by vegetation types that CTESSEL is not able to represent properly, e.g. crops as shown by the WBI site in West Branch (Iowa, US). The benefit of coupling the meteorology and NEE fluxes at synoptic timescales can only be demonstrated at the sites where the NEE fluxes are modelled reasonably well and for the period when the synoptic variability of the fluxes associated with the passage of synoptic weather systems is important. This limits substantially the sites and period that can be used. Park Falls also offers several levels. The top level (at 396 m above surface) is mostly located in the free troposphere during nighttime. Therefore, it allows an assessment of the vertical extent of the impact of synoptic variability of fluxes which could not be tested at the other continous sites (see also reply to referee 1).

• *Discussion on what is the goal of a CO_2 forecast and its accuracy requirements. Specification of a target would help to interpret the results of the quality assessment.*

The main goal for the CO_2 forecasting system is to allow the assimilation of observations in near-real time. This is already mentioned in the introduction, but it will be emphasized further in the revised version of the paper. As the main use of the forecast is to support data assimilation of observations with short-length window, the main requirement is that it simulates the CO_2 variability on short time-scales, ie. diurnal cycle, synoptic variability. Provided there is enough coverage, the data assimilation is able to remove the biases in the well-mixed background CO_2. Because the only observations that have a good coverage in near-real time are based on satellite near-infrared data, there is a lack of sampling for times where there is not sunlight. Namely, where there is winter darkness at high-latitudes and night-time. Therefore, the seasonal cycle at high latitudes, the diurnal cycle as well as the vertical profile are not going to be properly constrained by total column satellite observations and the CO_2 analysis will have to rely on the model. Therefore, it is also important to evaluate the model biases, as they will largely influence the biases in the resulting analysis.

In the revised version of the paper this main goal and the aim of the forecast evaluation will be emphasized. The other potential uses and their associated accuracy requirements will also be mentioned in the introduction. These users have to be aware of the current model biases, particularly as the analysis is not yet available in near-real time. If their applications of the CO_2 forecast critically depend on the presence of biases, then they should either device a bias correction as it is done for limited area simulations which correct the biases in the boundary conditions (e.g. Göckede et al. 2010,), or they should use the analysis which will be available by the end of this year. Other users, might be more interested in the synoptic variability than in the absolute value. This is the case for fligh campaign planning.

• *It is not clear why the use of optimised fluxes is limited to the initialisations 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).*

The forecasts are initialized with a simulation using optimised fluxes in order to have bias-free initial conditions of the atmospheric CO_2 at global scale and then see how the annual bias accumulates throughout the year, allowing a link between the atmospheric error evaluation and the global annual budget for the surface fluxes. The optimized fluxes are also used to assess the seasonal time scale of the global budget for CTESSEL as shown in Fig. 2(b). The uncertainty of the optimized fluxes at local scales is too large to be able to use them as a reference for a site-specific model flux evaluation.

SPECIFIC COMMENTS

• *Page 13910, line 23: I am wondering why only satellite data are mentioned here for the assimilation step. 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.*

Initially, the plan is to use only satellite data because they are the only observations with enough global coverage available in near-real time (e.g. 3 to 5 days delay for GOSAT retrievals). If and when other independent observations are available in near-real time (e.g. TCCON total column and IAGOS aircraft profiles), then a bias correction scheme could be implemented (e.g. the variational bias correction within the ECMWF 4D-VAR assimilation system). The second step will be to use both satellite and in situ data (e.g. ICOS) in order to better constrain the CO_2 surface concentrations in the analysis. The plans and the underlying strategy for data assimilation will be mentioned in the revised manuscript (see also reply to referee 1).

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

Currently there is no reference to the proportional tracer mass fixer, but work is in progress to document the use of several mass fixers available in the IFS (see Diamantakis and Flemming, 2014) for CO_2 and CH_4. The following short explanation will be provided in the revised manuscript: "*The global proportional mass fixer consists on rescaling the 3-D field of the tracer mixing ratio by using a global scaling factor obtained by dividing the globally integrated tracer mass before and after the advection in the model.*"

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

An extrapolation will be applied using a mean annual growth (i.e. an average of the last ten years) available from inventories. An update of the latest estimates of the anthropogenic flux annual growth is provided by the Global Carbon Project. This will be clarified in the revised manuscript.

• *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.*

The optimized fluxes are not used elsewhere in the evaluation of the paper. A reference to Chevallier et al. (2011) will be added in the revised manuscript.

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

The persistence effect is the main hypothesis to explain the difference in the atmospheric CO_2 errors between spring and autumn. The seasonal cycle amplitude of the NEE budget in CTESSEL is too weak (see Fig 2b), i.e. respiration/photosynthesis are too weak in the winter/summer. Because of the persistence effect, this will lead to an early drawdown in spring (associated with the winter negative bias), but in the autumn the negative and positive biases from the winter and summer will compensate. An explanation to clarify the differences between the two transitioning season (spring and autumn) will be added in the revised manuscript (see also reply to referee 1).

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

For the aircraft evaluation, the model was not interpolated but the nearest model gridpoint, model level and 3-houlry archived time to the observation was used. This will be clarified in the revised manuscript.

• *Appendix A2: The model is corrected for the wet fraction of the air mass, to derive dry air total column mixing ratios for the 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.*

The humidity correction is required because we are using the total pressure, which needs to be corrected into dry pressure. This will be clarified in the revised manuscript.

• *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).*

The extra information which is not relevant will be removed and the information on the bias and standard deviation will be included in the figure caption.

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