Reply to the interactive comments on acp-2014-264: ”Forecasting global atmospheric CO_2” by Agustí-Panareda et al.

3 September 2014

We thank the referees for their detailed and thoughtful comments, which will help to improve the presentation of our results. All general, specific and technical comments are addressed below, including the modifications performed in the revised manuscript.

General comments

- *It is not clear whether the system is ”fully coupled” with the atmospheric model receiving input from physical properties of the vegetation model (e.g., energy and momentum flux), resulting in carbon uptake feedbacks to the atmospheric circulation (e.g., evapotranspiration). My impression is that it isn’t, though hopefully it will be. If it is, this needs to be emphasized, and the effects carefully analyzed. Such analysis is likely beyond the scope of this study, but this needs to be stated. If the atmospheric model is coupled to some other ”non-vegetated” model, this should be discussed with respect to possible inconsistencies with vegetated boundary conditions.*

The CO_2 fluxes are currently not fully coupled with the water and energy fluxes, as the evapotranspiration from CTESSEL is not currently used operationally. Instead, the surface water and energy fluxes in the operational IFS are still based on the Jarvis model where the stomatal conductance is parameterised with an empirical formulation using stress functions depending on environmental conditions. In CTESSEL, the stomatal conductance is parameterised using the A-gs photosynthesis model, and the resulting evapotranspiration was previously tested by Boussetta et al. (2013a). The results showed better scores in the energy and water fluxes as well as the near surface parameters (i.e. 2m relative humidty and temperature). Unfortunately, the impact on the atmospheric circulation as measured by the standard meteorological scores was negative due to other compensating errors coming from other parts of the model. The plan is to have the full coupling in the future and work is in progress to achieve that goal.

Despite the fact that the energy and water fluxes are currently computed with different parameterization than the carbon fluxes, the vegetation and LAI datasets are the same for the Jarvis model as for the CTESSEL model. Therefore, there are no inconsistencies in terms of the representation of vegetation. Morever, a comparison between the fully coupled model and the partially coupled model by Boussetta et al. (2013a) shows that the NEE does not change much when the model is fully coupled. Because the MACC CO_2 forecasting system is based on the operational IFS, CTESSEL is also currently not coupled with the surface energy/water fluxes.

This explanation has been added in the revised version of the paper to clarify that there is only partial coupling between the atmosphere and the CO_2 fluxes (see section 2.2 in the revised manuscript):

" *CTESSEL is a photosynthesis-conductance (A-gs) model based on Calvet et al. (1998,200,2004) and developed originally by Jacobs et al. (1996). It provides CO_2 fluxes as well as evapotranspiration. However, the evapotranspiration in the IFS is currently still based on the Jarvis approach (Jarvis, 1976) instead of the plant physiological approach of CTESSEL. Despite not having a full coupling between evapotranspiration and CO_2 fluxes, there is some consistency between the two fluxes because they both rely on the same underlying representation of vegetation.*"

- *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).

The first paragraph in section 3.3.2 has been modified to justify the use of one site and one month for the evaluation of the experiment with and without fluxes varying on synoptic time scales:

"*The relative importance of the synoptic variability of NEE vs. transport can be assessed by comparing the standard hindcast with a simulation using 3 hourly monthly mean NEE from CTESSEL (i.e. without day-to-day variability) instead of real-time NEE. In order to demonstrate this, it is important to first find observing sites which are systematically affected by both NEE and synoptic advection, and properly represented in the model. The observing station at Park Falls experiences the ideal conditions in September. Both local NEE fluxes and synoptic advection are important for the simulation of the variability of the atmospheric CO_2 there. In addition, the site exhibits a good correlation between the simulated and the observed CO_2.*"

- *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 has been emphasized in section 1:

"*Thus, the CO_2 forecast model will be crucial in filling this information gap during the data assimilation process. Indeed, the main use of the forecast is to support the data assimilation of CO_2 observations. Because the data assimilation window used in the IFS is 12 hours, the main requirement for the CO_2 forecast is to have skill in the simulation of the CO_2 variability on short time-scales, e.g. diurnal and synoptic scales. The errors in the forecast will influence the quality of the resulting CO_2 analysis. For this reason, the evaluation of the CO_2 forecast errors is also very important for the analysis.*"

- *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. The first paragraph in section 3.1 has been modified to clarify the use of the optimised fluxes in the annual re-initialization of the forecast:

"*In the case of optimized fluxes Chevallier et al. (2011), there is a reasonably good fit between their budget and the observed global growth. Hence, they can be used as a reference, representing a current best estimate for the fluxes at the global scale. Note that the optimized fluxes are not available in near real time because they rely on the highly accurate atmospheric CO_2 flask observations which are currently only provided several months after the date.*"

Specific comments

- *P13911, L23-28: Comment on plans to assimilate in-situ surface data.*

There are several issues concerning the assimilation of CO_2 in situ observations in the global atmospheric CO_2 forecasting system. First of all, most in situ data are not available in near-real time. Currently, only six ICOS stations are providing data with a one-day delay. Given the sparse spatial data coverage of the stations available and the short data assimilation (DA) window used in our system - currently 12 hours - the resulting analysis increments would be very localised near the surface and around the station. Thus, the impact of the global CO_2 would be minimal. Even regionally, advection and mixing would transport and dilute the small-scale increments quite fast. Moreover, most ICOS stations are not sampling background air, but they can be strongly influenced by local surface fluxes of CO_2. Since in our DA system we are currently not able to correct the CO_2 surface fluxes, the errors in the fluxes would wipe out the impact of the DA increments around the station within 12 hours. Because of all these reasons we propose the following strategy to test the different possible configurations of the CO_2 data assimilation system:

- step 1) Assimilation of satellite data (GOSAT, OCO-2) will allow the removal of a large part of the accumulated bias in background air from the forecast.
- step 2) Assimilation of satellite data + in situ data in NRT (e.g. ICOS) will be able to better constrain CO_2 at nighttime and winter over local areas.

The plans to assimilate in situ surface data have now been emphasized in the abstract and section 1:

" *In the future, the forecast will be re-initialized regularly with atmospheric CO_2 analyses based on the assimilation of CO_2 products retrieved from satellite measurements and in situ observations.*"

" *The in situ observations at the surface are very valuable not only for evaluation purposes, but they have the potential to provide complementary information to the CO_2 satellite products for the CO_2 analysis. The continuous in situ observations have a much higher accuracy than the satellite data therefore providing a reference for correcting biases close to the surface. Although they have a sparser spatial coverage than satellite measurements, they have a much better temporal coverage at high latitudes, during cloudy conditions and at nighttime.*"

- *P13915, L26-27: "world leading state-of-the-art NWP model - based on what?*

The statement "world leading state-of-the-art NWP model" is just to emphasize that the transport from the IFS model is expected to be as accurate as possible since it provides one of the best weather forecasts in the medium-range (up to 10-days lead time) based on NWP model intercomparison of skill scores. An ECMWF technical report by Richardson et al. (2013) which shows this intercomparison of NWP scores will be added as a reference to support this statement. The report includes a regular intercomparison of the forecasts from major NWP centers. A new sentence has been added in the revised manuscript to include this reference:

" *Finally, the IFS provides one of the best weather forecasts in the medium-range (up to 10-days lead time) based on NWP model intercomparison of skill scores (Richardson et al., 2013). Because the IFS is a world leading state-of-the-art NWP model, it is also used as a reference for the development of some CTMs, e.g. TM5 (see Krol et al.,2005).*"

- *P13916, L14: Use of "LAI climatology" is misleading. Is monthly LAI fixed or year specific? Does prescription of LAI have an influence on errors in Spring NEE transition?*

The LAI used in CTESSEL is fixed and not yearly dependent. It is a monthly mean MODIS climatology based on a 9 year averaging process (20002008) (see Boussetta et al. 2013b for more details). Thus, although it is possible that it has an influence on the Spring NEE transition, one would expect this effect to vary with the year. Since the error in Spring NEE transition is always the same, i.e. the CO_2 drawdown starts too early, it is likely that there are other errors that are consistent every year which play a larger role (e.g. the persistence effect of the respiration underestimation in winter).

A new sentence has been included in the revised manuscript to clarify the term "LAI climatology" (see section 2.2):

"*The GPP represents the photosynthetic fluxes which are driven by radiation, soil moisture, soil temperature and a prescribed satellite MODIS Leaf Area Index (LAI) fixed monthly climatology (`http://landval.gsfc.nasa.gov/`) based on a 9 year averaging process (2000-2008) as described in Boussetta et al.,2013a).*"

- *P13916, L16: Given the issues with the seasonal amplitude and timing of NEE and its relation to gross fluxes of GPP and TER, it is worthwhile to describe the "reference respiration parameter" in more detail, including its sensitivity (or relation) to GPP, temperature and moisture.*

GPP is computed independently from the ecosystem respiration (R_{eco}) in the model. In terms of model parameters, GPP is highly sensitive to the mesophyll conductance parameter (g_m^*) and R_{eco} is very sensitive to the reference respiration parameter (R_0). Both parameters are defined as constants for each vegetation type (see Table 1 in Boussetta et al. 2013a) which have been obtained via an optimization procedure also described in Boussetta et al. (2013a). Because this optimization procedure relies on FLUXNET data and not all vegetation types are properly sampled, we expect higher uncertainties in the model parameters for certain vegetation types (e.g. tundra, crops). Other error sources could come from the vegetation classification itself, which was design for defining roughness lengths rather than carbon cycle studies.

The relation of the CO_2 fluxes with temperature is parameterized by a Q_{10} function and the relation with moisture is given by a soil moisure stress response function. The model parameters affecting the relationships with moisture and temperature are listed in Table 2 of Boussetta et al. (2013a).

More detail on the computation of GPP and R_{eco} in the model has been included in the revised manuscript (see section 1.1):

" *The NEE results from the Gross Primary Production (GPP) and the ecosytem respiration (R_{eco}) fluxes which are computed independently in the model. The GPP represents the photosynthetic fluxes which are driven by radiation, soil moisture, soil temperature and a prescribed satellite MODIS Leaf Area Index (LAI) fixed monthly climatology (`http://landval.gsfc.nasa.gov/`) based on a 9 year averaging process (2000-2008) as described in Boussetta et al. (2013a). The ecosystem respiration is given by empirical formulas driven by soil moisture, soil temperature and snow cover. The model parameters affecting the sensitivity of GPP and R_{eco} to temperature, soil moisture and radiation are listed in Table 2 of Boussetta et al. (2013a).* "

- *P13918, L5: The statement "because the model is not constrained by CO_2 observations" is not quite accurate. Really, the budget mismatch is due to "errors in modelled fluxes" which data assimilation can alleviate.*

The statement has been modified to:

"*The CO_2 fluxes in the model are currently not constrained by atmospheric CO_2 observations. Thus, the budget of the total CO_2 emissions – affected by all the errors in the CO_2 fluxes – does* *not match the observed atmospheric growth.*"

- *P13918: Interannual variability (IAV) is only briefly discussed. Although not a major focus of the study, the large error in IAV originating in the tropics should be mentioned. If a mechanistic source of error is known (e.g. fires, high sensitivity of biology to climate), please discuss. At the very least, it would be helpful to discuss whether assimilation of satellite retrievals in the tropics can help minimize future IAV errors.*

The IAV in the CO_2 budget in the model comes mainly from the NEE and not the fires as shown by Figure 2a in the manuscript. Both GPP and R_{eco} have very large (opposing) values in the tropics. In the tropics there is also a large sensitivity of the GPP and R_{eco} to climate forcing linked to both vegetation-linked model parameters and high values of radiation, soil temperature and soil moisture. Therefore, any IAV in climate fields (e.g. temperature, radiation, soil moisture) will lead to large variability of the fluxes. However, the CTESSEL model is not designed to study IAV as it is a very simplified model without a proper representation of Carbon stocks and ecosystem disturbances, e.g. affecting tree mortality. Thus, large errors are expected. The IAV is only evaluated in the context of the global budget of fluxes, in order to try to understand where the CO_2 errors in the CO_2 atmospheric model are coming from.

The assimilation of satellite retrievals in the analysis system at ECMWF will correct for the atmospheric concentrations but not the fluxes. Some of the errors in atmospheric CO_2 concentrations associated with the CO_2 surface fluxes will be corrected in the analysis, although not all the regions of the tropics will be sampled due to the high frequency of cloud cover. The high uncertainties in the tropics (both from NEE and transport modelling) and the possibility of reducing these errors using data assimilation has been mentioned in the revised version of the manuscript (see section 3.1):

" *The large error associated with this interannual variability stems from several factors. Namely, the high sensitivity of the biogenic fluxes to climate forcing in the model, combined with large uncertainty in the model parameters, as well as missing and simplified processes in CTESSEL. Moreover, the large gaps in the meteorological observing network in the tropics result in higher errors associated with the climate forcing of the NEE fluxes. Assimilation of satellite products (e.g. soil moisture, LAI and* CO_2*) might help in the evaluation and reduction of these uncertainties and associated errors.*"

- *Section 3.3.1: It is interesting that synoptic correlations are much weaker (and sometimes negative!) in Spring compared to Fall. If the "transition period" of changing NEE sign is responsible, wouldn't the Fall transition also cause low correlation? What's the difference? An alternative hypothesis is a "persistence" effect, where very low background values from summer uptake leads to enhanced variability in the following months, such as synoptic transport, which is well simulated, plays a greater role in day-to-day variability and local exchange (low Fall NEE) less of a role. It might be worth testing for this effect by examining the standard deviation of daytime averages in Fall compared to Spring, where larger Fall values would support his argument.*

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 persistence effect, this will lead to an early drawdown in spring (due to the winter negative bias), but in autumn the positive and negative biases will compensate. This explanation has been included in the revised manuscript (see last paragraph in section 3.3.1).

- *Section 3.5: Please state the purpose of evaluating the interhemispheric gradient (i.e., another metric to examine errors in seasonal exchange in northern vs tropical latitudes)*

The inter-hemispheric gradient is always evaluated when testing transport models for flux inversions. In this paper, we see that the errors in the inter-hemispheric gradient of CO_2 are consistent with the flux errors. The errors associated with transport are likely small compared to the flux errors. This is also consistent with results from the CH_4 TRANSCOM experiment, where the IFS model was deemed to produce a reasonable inter-hemispheric gradient compared to observations. A new sentence has been added in the revised manuscript:

" *The interhemispheric gradient is an important feature for CTM simulations, because it can be used to detect errors in both transport and* CO_2 *fluxes. As the TRANSCOM evaluation showed a good interhemispheric gradient for* CH_4 *in the IFS (P. Patra, personal communication, 2012), we expect most of the error to come from the* CO_2 *fluxes.*"

- *P13929, L26-27: Will LAI and soil moisture be assimilated into the vegetation model, or prescribed?*

The LAI and soil moisture will be assimilated into the vegetation model. This has been clarified in the revised manuscript (see section 5):

"*For example, the assimilation of the near-real time albedo and LAI from the Copernicus Global Land Service (Boussetta et al.,2014), and the SMOS/ASCAT soil moisture products (Munoz-Sabater et al., 2012; Munoz-Sabater et al., 2013; de Rosnay et al., 2012) could improve the phenology and the meteorological forcing on the modelled NEE fluxes respectively.* "

- *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 abstract has been updated in the revised manuscript to mention the assimilation of the in situ data as well as the satellite data for the production of the CO_2 analysis.

- *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 has been provided in the revised manuscript:

"*The global proportional mass fixer consists on re-scaling the 3-D field of the atmospheric* CO_2 *mixing ratio by using a global scaling factor. This factor is obtained by dividing the globally integrated atmospheric* CO_2 *mass before the Semi-Lagrangian advection in the model by the one after the advection.*"

- *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 has been clarified in the revised manuscript:

"*Note that the same climatological trend will be used to extrapolate the anthropogenic fluxes to the present in the operational* CO_2 *forecast.*"

- *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) has been added in the revised manuscript.

- *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-hourly archived time to the observation was used. This has been 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 has been 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 has been removed and the information on the bias and standard deviation has been described in the figure caption of the revised manuscript.

Technical corrections

- *P13916, L6: replace "three quarters of an hour" with "45 minutes".*

  Done.

- *Page 13916, line 22: skip on 'grid'*

  Done.

- *P13917, L5: replace "sink" with "flux" (for consistency with fire and anthropogenic descriptions).*

  Done.

- *Page 13926, 'performance' i.o. 'preformace'*

  Done.

- *P13927, L7: "biases" of what?*

  The biases refer to biases of atmospheric CO_2 (both close to the surface and in the total column). This has been clarified in the revised version of the manuscript.

- *Figure2: Line4: "amd" should be "and"*

  Done.

- *Figure 7: Increase font size of symbols.*

  Done.

- *Figure 14: need to label subpanels.*

  Done.

REFERENCES

Boussetta, S., Balsamo, G., Beljaars, A., Agusti-Panareda, A., Calvet, J.-C., Jacobs, C., van den Hurk, B., Viterbo, P., Lafont, S., Dutra, E., Jarlan, L., Balzarolo, M., Papale, D., and van der Werf, G. : Natural carbon dioxide exchanges in the ECMWF Integrated Forecasting System: implementation and offline validation, *J. Geophys. Res.-Atmos.*, 118, 1–24, doi: 10.1002/jgrd.50488, 2013a.

Boussetta, S., Balsamo, G., Beljaars, A., and Jarlan, J.: Impact of a satellite-derived Leaf Area Index monthly climatology in a global Numerical Weather Prediction model, *Int. J. Remote Sens.*, 34, 3520–3542, 2013b.

Chevallier F., N.M. Deutscher, T.J. Conway, P. Ciais, L. Ciattaglia, S. Dohe, M. Fröhlich, A.J. Gomez-Pelaez , D. Griffith, F. Hase, L. Haszpra, P. Krummel, E. Kyrö, C. Labuschne, R. Langenfelds, T. Machida, F. Maignan, H. Matsueda , I. Morino, J. Notholt, M. Ramonet, Y. Sawa , M. Schmidt, V. Sherlock, P. Steele, K. Strong , R. Sussmann, P. Wennberg, S. Wofsy,

D. Worthy , D. Wunch, M. Zimnoch, Global CO2 fluxes inferred from surface air-sample measurements and from TCCON retrievals of the CO2 total column, *Geophys. Res. Let.*, 38, doi:10.1029/2011GL049899, 2011.

Diamantakis, M. and Flemming, J., Global mass fixer algorithms for conservative tracer transport in the ECMWF model, *Geosci. Model Dev.*, 7, 965979, 2014.

Göckede M., D.P. Turner, A. Michalak, D. Vickers and B.E. Law, Sensitivity of a subregional scale atmospheric inverse CO_2 modeling framework to boundary conditions, *J. Geophys. Res.*,115, D24112, 2010.

Richardson, D.S., Bidlot, J., Ferranti, L., Haiden, T., Hewson, T., Janousek, M., Prates, F. and Vitart, F., Evaluation of ECMWF forecasts, including 20122013 upgrades, *ECMWF Technical Memo*, No. 710, (`http://old.ecmwf.int/publications/library/ecpublications/_pdf/tm/701-800/tm710.pdf`).