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Atmospheric Chemistry and Physics
Discussions

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

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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 1

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 will be added in the revised version of the paper to clarify that there is only partial coupling between the atmosphere and the CO_2 fluxes, but preliminary testing of the full coupling by Boussetta et al. (2013a) shows that the impact of not having a complete coupling on the resulting NEE fluxes is modest over most FLUXNET sites.

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.

• *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.

• *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 (2000–2008) (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).

• *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).

- *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 will be modified to: "Due to errors in the fluxes which cannot be corrected because the model is currently not constrained by CO_2 observations,..."

- *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 will be mentioned in the revised version of the manuscript.

- *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 will be mentioned in the revised manuscript.

- *Section 3.3.2: It is a bit frustrating that the impact of NEE day-to-day variability is only tested in one month of one season at one site. In particular, the results at Park*

Falls, which resides in a biologically dense region, are not too surprising. It would be useful to also test for other seasons, and a more biologically remote location such as Mace Head. It is also not clear at what scales the effect of diurnal exchange at LEF become unimportant. Presumably its local diurnal exchange which dominates surface CO_2, in which case 3-hourly fluxes becomes less important in remote locations. A simple test could be to rerun the simulation for the month of September using monthly fluxes locally (e.g. 10 deg lat x 10 deg lon box centred at Park Falls) and 3 hour fluxes everywhere else.

In order to show the synergy between biogenic fluxes, meteorological forcing and transport at synoptic scales, it is important to first find a site which is systematically affected by both NEE and synoptic advection. Secondly, if the NEE is not properly represented in the model at certain sites (e.g. those affected by crops) then it is also not possible to use those sites (e.g. West Branch, Iowa, USA). Mace Head is mainly sampling well-mixed background air from the Atlantic, with synoptic anomalies located mainly at upper levels. Therefore it is not as good a site as Park Falls to study the NEE synoptic variability and its synergy with synoptic transport. Synoptic variability at Mace Head can be observed during anticyclonic conditions, when boundary layer mixing plays an important role. In winter, the variability at the sites which were used for the evaluation is dominated by synoptic advection. Whereas in summer, the local transport, e.g. convection, can dominate the variability. September is an ideal month for such a study because both local fluxes and synoptic advection are important. Therefore, the limited testing at one site and one month. The test at Park Falls demonstrates that the synoptic variability of NEE is important within the boundary layer but not in the free troposphere. In summary, we can say that the effect of the NEE synoptic variability appears to be localised. Both in the free troposphere above biological dense regions and downstream, the monthly mean large-scale CO_2 gradient is the main contributor for the representation of the atmospheric CO_2 synoptic variability, as well as the winds. This will be clarified in the revised version of the manuscript.

- *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.

- *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.

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