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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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The manuscript "Forecasting global atmospheric CO2" by Agusti-Panareda et al. presents an evaluation of CO2 hindcasts from the IFS forecasting system against a range of in-situ and remotely-sensed observations, in preparation for real-time forecasts. Atmospheric CO2 contains information about sources and sinks of carbon. The ability to provide accurate real-time forecasts therefore has tremendous potential to provide valuable feedback with respect to changes in biogenic or anthropogenic emissions, for example related to agricultural management practices. The coupling of vegetation fluxes online within the IFS represents a critical step towards this end. By comparing to a range of observations from surface, airborne, and satellite platforms, the authors show promise in the simulation of day-to-day variability, which is traced to diurnally resolved fluxes and assimilated meteorology, but that biases is biogenic fluxes in northern latitudes leads to the accumulation of errors at longer (seasonal) timescales. The authors propose that more regular assimilation of CO2 observations

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(rather than once at the beginning of the year) will lead to improved forecasts and analyses at longer timescales.

I have only a few minor questions about the methodology and evaluation results, as discussed below. Given the novelty and advantages of near-time forecasting, and the care the authors give in the evaluation of the forecasting system, I believe this manuscript will be suitable for publication in ACP after a few minor revisions.

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.

Specific Comments

P13911, L23-28: Please comment on plans to assimilate in-situ surface data, which would complement satellite retrievals with (1) continuous monitoring (esp high latitudes), (2) information near the surface, and (3) information under clouds, at night, and in winter. Will data from ICOS network be used? Others? How will it complement GOSAT and OCO-2? If not, provide logistical/technical reasons for their exclusion (e.g., time delay too long).

P13915, L26-27: "world leading state-of-the-art NWP model" – based on what? Seems like a risky statement

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?

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P13916, L16: Given issues with seasonal amplitude and timing of NEE and it's 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.

P13918, L5: The statement "because the model is not constrained by CO2 observations" is not quite accurate. Really, the budget mismatch is due to "errors in modeled fluxes," which data assimilation can alleviate.

P13918: 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 useful to discuss whether assimilation of satellite retrievals in the tropics can help minimize future IAV 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 that 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 this argument.

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 CO2, in

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which case 3-hourly fluxes become 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 centered at Park Falls) and 3 hour fluxes everywhere else.

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) P 13929, L26-27: Will LAI and soil moisture be assimilated into the vegetation model, or prescribed?

Technical Corrections

P13916, L6: replace “three quarters of an hour” with “45 minutes”

P13917, L5: replace “sink” with “flux” (for consistency with fire and anthropogenic descriptions)

P13927, L7: “biases” of what?

Figure 2: Line 4: “amd” should be “and”

Figure 7: Increase font size of symbols

Figure 14: Need to label subpanels

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