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Discussions

Interactive comment on "A biogenic CO_2 flux adjustment scheme for the mitigation of large-scale biases in global atmospheric CO_2 analyses and forecasts" *by* A. Agustí-Panareda et al.

Anonymous Referee #1

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This paper presents an improvement of the simulated atmospheric CO2 budget within the IFS/CAMS modeling system. This improvement consists of an adjustment of the underlying biospheric CO2 fluxes (GPP, TER, and NEE). The authors use a scheme that relies on pre-calculated values of these fluxes from a separate inverse model. These fluxes were created as part of a joint project. The authors show the improvement of the new CO2 budget over the old one and try to show that this new system has advantages when forecasting atmospheric CO2 especially at the synoptic time scale. This works through a better representation of large scale zonal gradients of CO2, that come to observing sites strongly when the synoptic weather patterns change. Finally,

the authors discuss future steps for this system. This paper is written well and structured and represents a large piece of development of the CAMS model.

Nevertheless, in my opinion this paper has a number of problems and I believe that it is not currently suitable for publication in ACP. The first is that the paper contains relatively little scientific content, and there is nearly nothing that can be learned from the paper for a big audience. And even for researchers in the field of atmospheric CO2 modeling, these methods are very system specific and not easily used by others even if they needed such flux adjustments. So this paper should probably remain a technical report for the Copernicus project, or perhaps it can be published in Geophysical Model Development journal. The case of why having better synoptic variations in forecast CO2 is important is also not clearly made I think: who or what profits from this improved CO2 forecast?

Another issue with the paper is the choice of the control run. Taking the fluxes from the neutral-biosphere in CTESSEL is clearly wrong, and there could have been many easy ways to improve on those. I think that a better benchmark is the available MACC fluxes, as the authors show that these already do quite a good job in matching observations if simply prescribed to the CAMS model. The authors state that these fluxes do not have synoptic variability, and I am not clear why this is because their resolution is never mentioned in the paper. But if diurnal and synoptic variations are needed, the simple method of Olson and Randerson (2004) can be used to include the effect of temperature and light on monthly mean fluxes to get hourly ones. If the BAFS system was shown to be better than such an offline flux product, it would be much more clear to me that this way of BAFS is the way forward for CAMS.

In this manuscript, it is not clear to me why certain metrics were chosen for evaluation. The authors present mean biases and standard deviations in Figures 9 and 10, correlation coefficients in Table 4, no metric for Figure 11, but there are never root-mean-square differences reported which I think are most useful. I think in figure 11 the MACC fluxes have the lowest RMSD than the BFAS fluxes. And from the captions it seems that both observations and simulations are done as daily (24-hour?) averages. I think that this daily averaging is needed because the independent adjustment of the GPP and TER scaling factors leads to strong variations in NEE that do not necessarily preserve a good diurnal cycle. But I might be wrong on that, as I could not assess this from the figures shown. 24-hour average observations could have a lot of hour-to-hour variability which should be shows by an error bar. The statistics and figures moreover seem to cover only the month of March and a few selected days in March. It remains unexplained why this choice was made, and what the metrics look like for other months. I would expect for instance in summer to see even larger day-to-day variations in NEE, and then also in atmospheric COâĆĆ.

I would like to know what the added value is of having the gamma-parameter included in BFAS. The description of its calculation and adjustment is quite extensive but I do not really understand what role it plays. Perhaps there could be an experiment where BFAS is used without the adjustment in equation 3. After all, not needing the ensemble of forecasts would make the scheme a bit simpler, and perhaps just as good? I know I am likely to be wrong as the authors have decided to include this procedure in BFAS, but I would like to see the evidence to support that decision.

A further list of minor comments can hopefully help the authors prepare a new version for their manuscript if they want to submit it somewhere else.

- Page 3, line 5: I do not agree that the current monitoring of CO2 relies on satellites and it is even a bit insulting to the real monitoring groups to say it. I suggest to change it because satellites do not yet see reliable COâĆĆ. In fact, the second part of this statement is also not right because the observations you show and that MACC fluxes rely on mostly come from flasks and not from in-situ instruments. - Page 12, line 20: the current adjustment scheme for GPP and TER does not include any covariances between the adjustments, but we know that they often respond in the same direction and that errors are correlated. It would be good to think about an adjustment scheme that uses such information. Showing the posterior diurnal cycle is also needed. - Page

13, line 20: You use now the names OPT-CLIM and later on in the text and tables CLIM-OPT. Is this the same run? It was to me confusing. Also see later remark about Table 2. - Page 14, line 20: A table listing the annual mean fluxes for transcom regions for all simulations would be valuable I think - Page 15, line 25: The SH problems could come from a different north to south transport characteristic of the two atmospheric models used (IFS and LMDZ?). Can this be illustrated with a simple SF6 simulation and compare it to observations? - Acknowledgements: please check the data usage policy of NOAA as I do not believe you can simply take data from their FTP and then publish it with this statement. - Page 30, Table 2: I was confused because it says that CLIM-OPT uses MACC fluxes as reference in BFAS but from the methods I understood that CLIM-OPT or OPT-CLIM used the climatological fluxes from MACC directly as underlying biosphere fluxes? I discovered this only towards the end of reading and it made me think I misunderstood the simulations completely. Even now I doubt it. - Figures 4 and 7: it would be better to use PgC/yr as units and not GtC/day because now they just look very small on the y-axis with many insignificant digits to start. - I believe Figure 12 and 13 are not needed and could be removed

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