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Atmospheric Chemistry and Physics
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.

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We thank the reviewer for his/her comments. We will take them into account in the revised manuscript to improve the motivation and the message of this work. In particular, we will highlight the scientific content of our results. In the reply below we address all the reviewer’s concerns in order to clarify any misunderstanding on the importance of this study, and its relevance for the scientific community working on atmospheric composition and the carbon cycle.

General comments

* *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* CO_2 *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* CO_2 *is important is also not clearly made I think: who or what profits from this improved* CO_2 *forecast?*

The major aspects raised by the reviewer are addressed separately in detail below:

1. The scientific content of the paper.

   Any atmospheric CO_2 forecast system requires a flux adjustment of some sort in order to constrain the budget of sources/sinks at the surface and avoid the growth of biases in the atmospheric background as documented by Agusti-Panareda et al. (2014). The scientific question addressed in this paper is how to use the best information we have in near-real time to adjust the fluxes in a way that reduces the bias of the atmospheric CO_2 forecast with the minimum deterioration of the synoptic skill. The simple flux adjustment scheme proposed here is based on a climatology of optimized fluxes and it could be applied easily to other models. In the past other methods have been used by several modelling studies to remove biases attributed to the NEE fluxes. For instance, by globally re-scaling balanced NEE fluxes to match the residual land sink given by a climatology of TRANSCOM optimized fluxes (Nassar et al., 2010; Chen et al.,2013), or by re-scaling locally the NEE at boreal regions in order to get a better fit in the seasonal cycle (e.g.

Messerschmidt et al. 2013, Keppel-Aleks et al. 2012).

This paper addresses the challenge of designing an online bias correction in a forecasting system with the aim to deliver an atmospheric CO_2 forecast and analysis that can be useful to the scientific community. The other methods mentioned previously are designed to work as a one-off correction and they offer less flexibility because they are performed offline. Tunning model parameters and/or rescaling fluxes offline are not sufficient to garantee a bias reduction in the system. An online adaptive system is required because errors in the meteorology can evolve as a result of regular operational Numerical Weather Prediction (NWP) model upgrades and these affect the NEE budget in the model.

From the flux adjustment method presented in the manuscript we can learn several things about the model which can feedback later on model development as described in section 2.6 of the manuscript. The CAMS IFS model is just providing an example to show how this method can be applied efficiently in an operational forecasting system. It is also worth noting that the CAMS CO_2 forecast presented here is used by the scientific community for a variety of purposes (e.g. field experiments, boundary conditions). For this reason, we also think that the results, although specific to the CAMS CO_2 forecast model, could also be interesting to other scientists.

2. The applicability of this method to other systems is straightforward.

   The method could be useful for any model to be used in forecast mode and suffering from substantial biases in their land ecosystem flux budget. The use of the method can be two-fold: as a bias correction to the land ecosystem fluxes or as a diagnostic of bias contribution from different regions/vegetation types. The system is flexible and cost-effective to run. It only needs a few components: (i) A reference budget which can be obtained from a climatology of optimized fluxes (e.g. the MACC product can be easily obtained from www-lscedods.cea.fr/invsat/PYVAR14_MACC/V2/Fluxes/3Hourly and it is well documented); (ii) Past 10-day

   NEE simulated by the forward model; (iii) The NEE anomaly of the forward model with respect to its climate based on a 10-year simulation. The use of the NEE anomaly is optional, and the benefits/drawbacks of using it will be described in the revised version of the paper (see further explanation in the minor comments).

3. Who or what profits from this improved CO_2 forecast?

   The CO_2 forecast is a product freely available to the wide public and scientific community (http://atmosphere.copernicus.eu) with users from a variety of backgrounds. This will be emphasized in the revised version of the manuscript, including the main scientific research areas that can benefit from a CO_2 forecast which are listed below:

   - **Global data assimilation of atmospheric CO_2 observations**

     The atmospheric CO_2 forecast is used as a prior to the atmospheric CO_2 analysis. For example, the CAMS atmospheric CO_2 analysis currently assimilates the GOSAT CO_2 product using a 4D-Var atmospheric data assimilation system (Massart et al. 2016). The reduction of the bias in the forecast by BFAS is highly desirable for data assimilation because the biases violate the assumption that the error distribution of the prior is centred around the true value.

     The CO_2 analysis system could be used to assimilate/combine a wide range of observations in the future. Preliminary monitoring/intercomparison of different CO_2 satellite products can be easily performed to provide feedback to the scientific community working on satellite retrievals. The fact that the forecast can provide a realistic representation of the underlying atmospheric variability of CO_2 in a timely manner is an important part of this data assimilation and monitoring processes. One of the most prominent modes of variability in the current 5-day forecast is the day-to-day synoptic variability. Thus, the emphasis is on synoptic timescales.

- **CO_2 observing system**
  The CO_2 forecast has been used in the research of bias corrections for satellite retrievals of OCO-2 lead by Chris O'Dell and could also be used in CH_4 satellite retrievals using the proxy method (Schepers et al. 2012). The predictive skill has also been used to support the planning of flight campaigns (e.g. CHARMEX, Ricaud et al. 2016, http://charmex.lsce.ipsl.fr/, and ACT-America, http://www-air.larc.nasa.gov/missions/ACT-America/) designed to improve our understanding of processes affecting atmospheric composition. It has also been used to demonstrate the use of new instruments in field experiments (e.g. Polarstern campaign, Klappenback et al. 2015). The detection of the atmospheric signals in the 1-day forecast (or nowcasting) can also help the interpretation of the observed variability from operational in situ networks (ICOS/InGOS monitoring), as well as expanding research networks (e.g. TCCON-RD) which aim to provide observations a few days behind real time.
- **CO_2 regional modelling**
  Another core usage of the global forecast is as boundary conditions for regional models. In particularly those studies focusing on city-scale resolution (e.g. Bréon et al. 2015, Boon et al. 2015) can benefit the most from the high resolution of the NWP global model.

Because of all these growing needs for a CO_2 analysis/forecast in real time, there have been recent efforts to start similar analysis/forecasting systems by NASA GMAO (http://acdb-ext.gsfc.nasa.gov/People/Colarco/Mission_Support/, Ott et al. 2015) and Environment Canada (Polavarapu et al. 2015) with their NWP models.

* *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 Olsen 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 BFAS is the way forward for CAMS.

In the revised manuscript we will highlight the benefits of using BFAS to correct the modelled NEE as part of the CTESSEL land-surface model instead of using an offline flux product, e.g. the climatology of the MACC optimized fluxes (used as benchmark in the paper). The MACC optimized fluxes have a resolution of 3 hours, but all night-time and day-time variations for time scales less than a week only come from the underlying prior fluxes. Using a 10-year climatology means that the synoptic variability of the fluxes is not present. Agusti-Panareda et al (2014) showed that the synoptic variability of the fluxes could be important when it comes to represent the synoptic atmospheric CO_2 variability in the boundary layer. The Olsen and Randerson (2004) method could be used to remediate part of this problem. However, this solution would not be as straightforward to apply in an online forecast as it is done in an offline mode, for which all the climate forcing parameters (2 m temperature and solar radiation can be retrieved beforehand). There are also other reasons for not using an offline NEE product or optimized fluxes directly in the CAMS CO_2 forecasting system:

- Downscaling the coarse optimized fluxes (2.5x3.75 degrees) at the resolution used by NWP models (currently 9 km at ECMWF) is not straightforward. Inconsistencies in the topography (particularly around mountains and coastlines) makes the low resolution fluxes difficult to use in a high resolution model.

- Coupling of CO_2 fluxes from terrestrial vegetation and the atmospheric model represents an important step towards a better understanding of the interaction between the ecosystem and regional atmospheric processes (Lu et al. 2001, Moreira et al, 2013). Boussetta et al. (2013) showed that the coupling between the CO_2 fluxes and the water and energy fluxes in the modelling of vegetation can improve the simulation of surface parameters such as temperature and humidity as well as NEE. This coupling has been shown to benefit the simulation of the CO_2 diurnal cycle in the atmospheric boundary layer in the tropics (Lu et al., 2005, Moreira et al. 2013).
- Finally, because offline NEE products or optimized fluxes are not available in near-real time, we would need to use a climatology. The inter-annual variability associated with the land sink cannot be considered when using just a climatology of NEE. Despite being a challenging aspect of the modelling, we think it is worth having inter-annual variabilily in the model forecast. The main rationale for this is based on the understanding that the climate variables simulated in the NWP model – such as temperature and precipitation – play an important role in explaining the inter-annual variability of NEE (Schaefer et al. 2002). The motivation for including the model inter-annual variability in the flux adjustment will be clarified in the revised 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 CO_2

Following the reviewer's advice, we have computed the root-mean-square (RMS) error of the different CO_2 experiments with respect to observations at the tower sites shown in Fig 11 of the manuscript (see Table 1 below). With the RMS error it is not as easy to see the improvement in the modelled variability as with the correlation coefficient r, because the RMS error increases very rapidly when there is large variability. This effect can be clearly seen at Park Falls at 30 m above the surface. Despite the substantial improvement in the model variability with BFAS ($r = 0.8$) compared to the CONTROL forecast ($r = 0.3$), the RMS error is larger in BFAS than in the CONTROL experiment by more than 1 ppm. This happens because the BFAS experiment overestimates the amplitude of the synoptic variability which is nearly non existent or even anticorrelated in the CONTROL experiment. At West Branch, the BFAS experiment has a much lower RMS error than both the experiments without BFAS and with optimized fluxes. Table 1 can be included in the supplement of the revised manuscript.

The impact of BFAS on the diurnal cycle amplitude has been evaluated in the northern hemisphere land (north of 20^{o}N) based hourly data from all the in situ stations compiled in the NOAA Obspack (2015) dataset for 2010 (Fig. 1 of this reply). The mean error of the diurnal cycle amplitude (daily max value minus daily min value) is reduced for all seasons, with larger improvements in winter, autumn and spring. The RMS error on the other hand is slightly worsened. This is not surprising since the reference optimized flux dataset is not designed to represent the synoptic variability of the diurnal cycle amplitude (see green and dark blue bars in Fig. 1 of this reply). Summer months

Table 1. Root mean square error [ppm] of different forecast (FC) experiments with observations at three NOAA/ESRL tall towers for daily mean dry molar fraction of atmospheric CO_2 in March 2010. The dash symbol means the correlation is not significant.

NOAA/ESRL Tower site (ID)	Latitude, Longitude, Altitude	Sampling level [m]	BFAS FC	CTRL FC	OPT FC	OPT-CLIM FC
Park Falls, Wisconsin (LEF)	45.95°N, 90.27°W, 472 m	30	6.12	4.97	3.04	3.31
		122	4.05	5.44	2.09	3.06
		396	2.93	5.10	1.37	1.99
West Branch, Iowa (WBI)	41.72°N, 91.35°W, 242 m	31	3.79	10.39	5.06	6.96
		99	2.91	9.94	2.95	3.92
		379	2.46	8.91	3.20	2.43
Argyle, Maine (AMT)	45.03°N, 68.68°W, 50 m	12	3.72	3.76	2.35	1.30
		30	3.55	3.36	1.66	0.82
		107	2.86	3.37	1.06	0.76

have larger diurnal cycle amplitudes and as expected the model also has larger errors in JJA. However, the impact of BFAS on the RMS error is the same for all months. This assessment of the diurnal cycle can be included in the supplement of the revised manuscript.

* *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 new experiment has been performed in which the γ factor is set to zero in order to demonstrate the value of having the inter-annual variability in BFAS. Indeed the inter-annual variability can be important factor in the simulation of CO_2 (Schaefer et al. 2002, Chamard et al. 2003). However, because is not the same in every region/season/year it can also be difficult to demostrate its impact with observations (Figs 2, 3, 4 and 5 of this reply). In BFAS, the use of the γ factor to represent the inter-annual variability from the model generally has a small impact. However, there are seasons and regions where we see the impact of using the γ factor. As expected, this impact tends to be larger in the tropics, where the model inter-annual variability is also largest (Agusti-Panareda et al. 2014). However, we can also see some impact in the northern and sourthern hemisphere for the MAM, JJA, SON seasons. In summary, including the inter-annual variability factor in BFAS is beneficial as in most cases it leads to a bias reduction, with just a few exceptions for the SON season (see LN20N in Fig. 1 and LTrop in Fig. 4 of this reply).

Minor comments

* *Page 3, line 5: I do not agree that the current monitoring of CO_2 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 CO_2. 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.*

The reference to in situ observations was meant to include both continuous and flask measurements (lines 8 to 10 in Page 3). In the revised version of the manuscript this will be clarified by specifying both explicitly.

* *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.*

This will be mentioned as future improvements planned for BFAS in section 6.1 of the revised manuscript. The impact on the diurnal cycle will be included in the supplement as mentioned above.

* *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*

The runs are the same and the text will be corrected in the revised version.

* *Page 14, line 20: A table listing the annual mean fluxes for transcom regions for all simulations would be valuable I think*

The proposed table for the budget in the Transcom regions will be included in the revised version.

* *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?*

We think the negative bias in the southern hemisphere comes from biases in tropical Africa. Preliminary experiments to assimilate IASI CO_2 using the CO_2 forecast have shown a large systematic difference throughout the free tropospheric column over tropical Africa which is consistent with the negative bias in the southern hemisphere. This will be mentioned in the revised manuscript.

* *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.*

The authors have contacted Ed Dlugokencky regarding the acknowledgements and received his confirmation that these are sufficient. An acknowledgement for the Obspack data used for the plots in the supplement of the revised manuscript will be added.

* *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.*

CLIM-OPT uses the climatological fluxes from MACC (i.e. the total CO_2 flux) and BFAS justs uses a climatology of the MACC residual biosphere fluxes. This will be clarified in Table 2 and in the text of the revised manuscript.

* *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.*

The units will be changed in the revised version of the manuscript.

* *I believe Figure 12 and 13 are not needed and could be removed.*

The authors disagree on this point. The fact that BFAS can change the gradient of the fluxes and as a result improve the atmospheric CO_2 synoptic variability is an achievement that needs to be properly documented.

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FULL FIGURE CAPTIONS

Figure 1: Evaluation of diurnal cycle amplitude of CO_2 dry molar mixing ratio [ppm] for the different forecast experiments (see legend) in the northern hemisphere land (north of 20°N) based on hourly data from all the in situ stations compiled in the NOAA Obspack (2015) dataset for 2010. Top panel: mean error; middle panel: root mean square error; and lower panel: number of observations.

Figure 2: Mean error of atmospheric CO_2 dry molar mixing ratio [ppm] for different forecast experiments (see legend) with respect to insitu and flask observations for different seasons and regions (N20N: north of 20°N; Trop: between 20°S and 20°N; S20S : south of 20°S) with a separation between land and sea points denoted by a preceeding "L" and "S" in the region name respectively. The observations were extracted from the NOAA Obspack (2015) dataset in 2010. The number of observations used for the statistics are shown as grey bars in the panel below each plot.

Figure 3: Root mean square error of atmospheric CO_2 dry molar mixing ratio [ppm] for different experiments (see legend) with respect to insitu and flask observations for different seasons and regions as described in Fig. 2. The observations were extracted from the NOAA Obspack (2015) dataset in 2010. The number of observations used for the statistics are shown as grey bars in the panel below each plot.

Figure 4: Mean error of atmospheric CO_2 dry molar mixing ratio [ppm] for different experiments (see legend) with respect to NOAA aircraft vertical profiles (Sweeney et al. 2015) in the free troposphere (1000 m above surface) for different seasons and regions as described in Fig. 2. The observations were extracted from the NOAA Obspack (2015) dataset in 2010. The number of observations used for the statistics are shown as grey bars in the panel below each plot.

Figure 5: Root mean square error of atmospheric CO_2 dry molar mixing ratio [ppm] for different experiments (see legend) with respect to NOAA aircraft vertical profiles (Sweeney et al. 2015) in the free troposphere (1000 m above surface) for different seasons and regions as described in Fig. 2. The observations were extracted from the NOAA Obspack (2015) dataset in 2010. The number of observations used for the statistics are shown as grey bars.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-987, 2016.

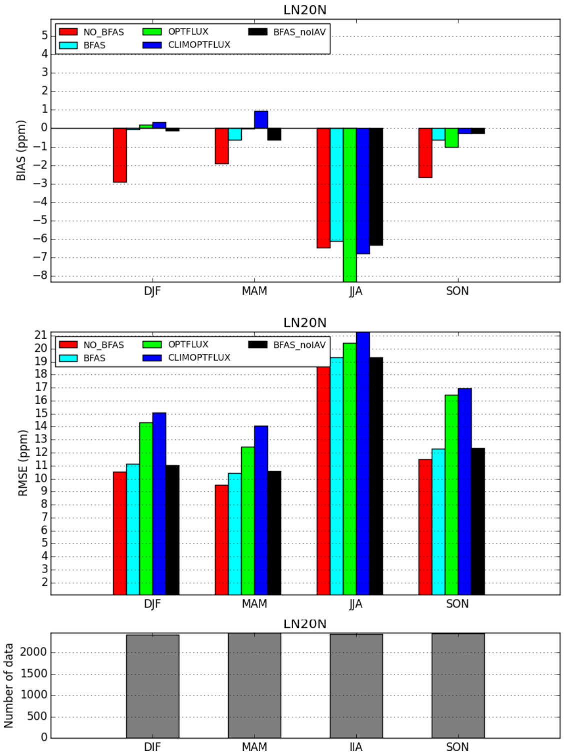

Fig. 1. Evaluation of diurnal cycle amplitude of CO2 [ppm] for the different forecast experiments (legend) in the NH land based on data from in situ stations (NOAA Obspack 2015). See full caption in text.

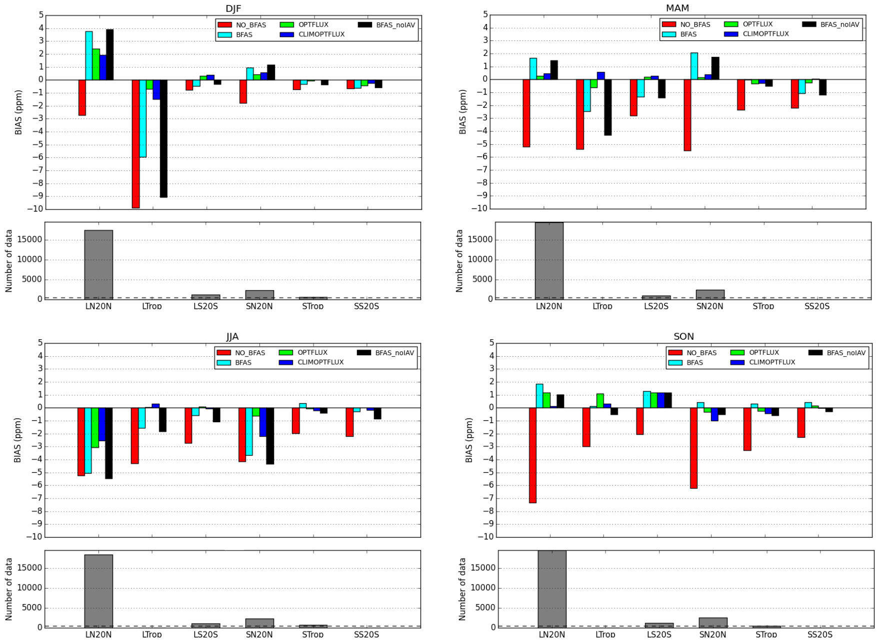

Fig. 2. Mean error of atmospheric CO2 [ppm] for different forecast experiments (see legend) with respect to insitu and flask observations from NOAA Obspack (2015). See full caption in text.

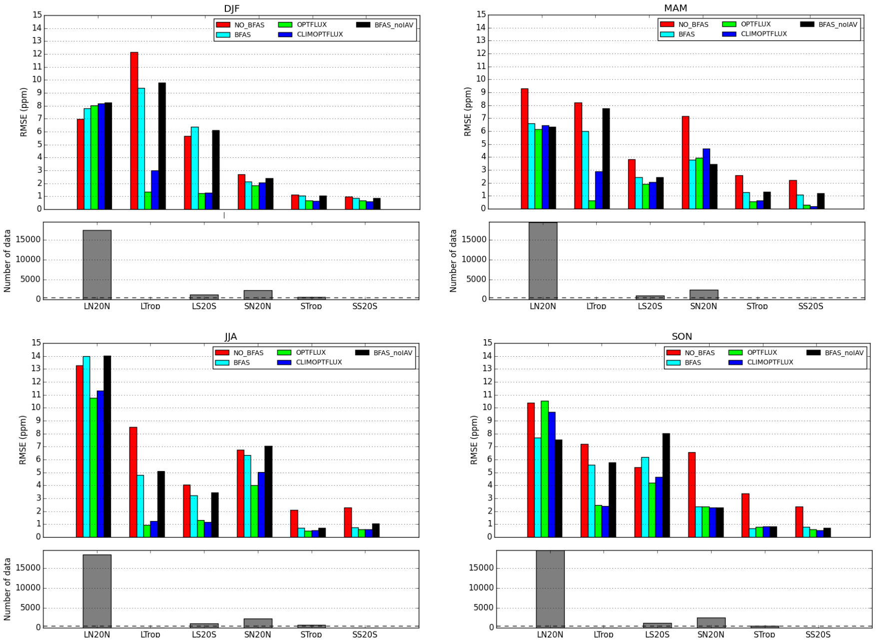

Fig. 3. RMS error of atmospheric CO2 [ppm] for different experiments (see legend) with respect to insitu and flask observations from NOAA Obspack (2015). See full caption in text.

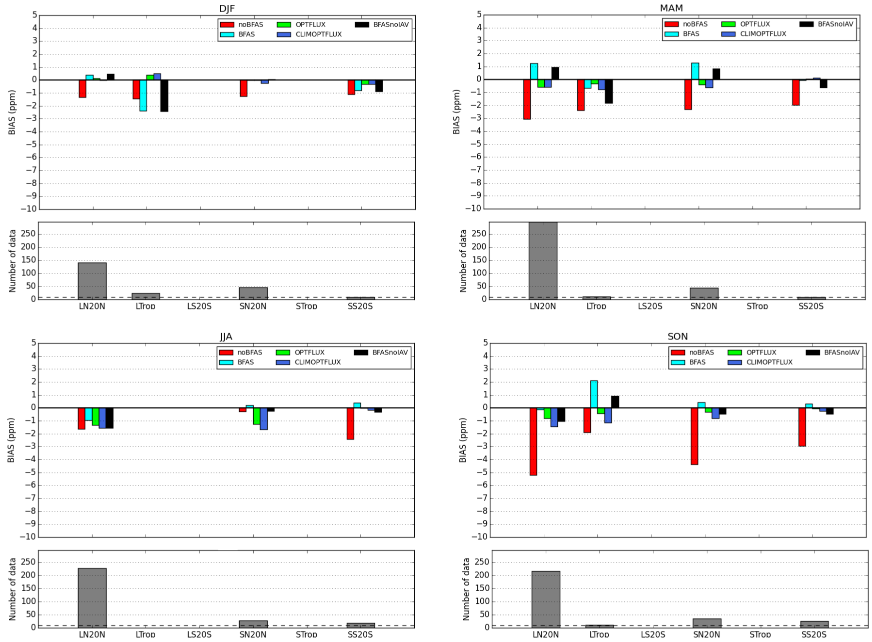

Fig. 4. Mean error of atmospheric CO2 [ppm] for different experiments (see legend) with respect to NOAA aircraft vertical profiles (Sweeney et al. 2015) in the free troposphere. See full caption in text.

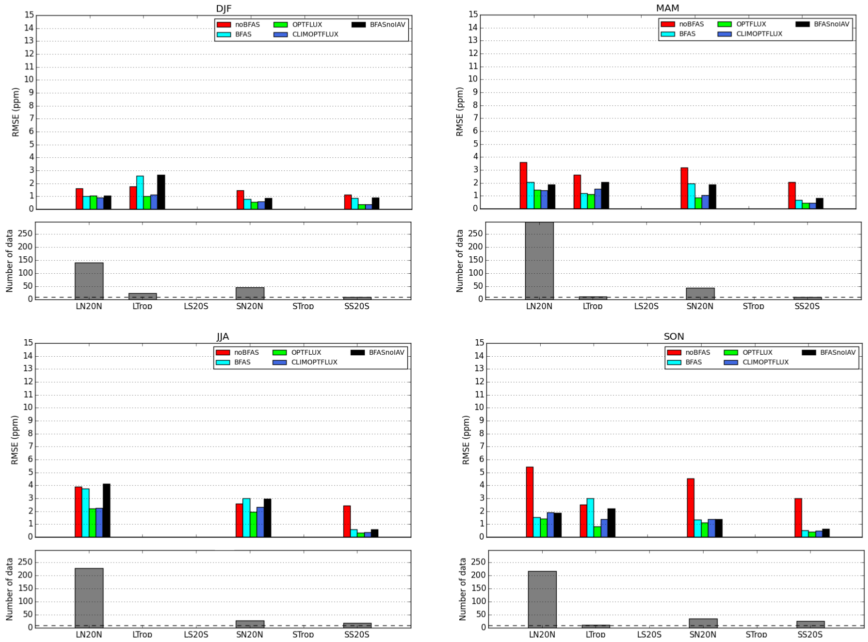

Fig. 5. RMS error of atmospheric CO2 [ppm] for different experiments (see legend) with respect to NOAA aircraft vertical profiles (Sweeney et al. 2015) in the free troposphere. See full caption in text.