Point-by-Point response to RC#2

Again, we thank the two anonymous reviewers as well as the handling Editor for their time to go through the revised manuscript and for the feedback on our response to the comments raised in their reviews.
In the following text, we address point-by-point the remaining comments raised by the second reviewer (RC#2). Note: Author Comments (ACs) are formatted in "Italic" indented text in correspondence to each RC.

I thank the authors of " To what extent does CO2 diurnal cycle impact carbon flux estimates using CarboScope?" for their detailed answers to my comments and for their revision of the manuscript.

Thank you for going through the answers and the manuscript.

These answers bring some arguments in favor of publishing this study. I do not find them totally convincing but their are significant enough to open the perspective of reaching a suitable balance.

The remaining concerns are addressed with additional analysis/modifications included in the revised MS.

The authors show that many of the GCP global inversions use the approach of Olsen and Randerson 2004, and they implicitly assume that such an approach poorly catches the actual NEE diurnal cycles.

This is addressed in the context of the next comment.

Therefore, a more convincing demonstration of the need to "revise and improve the methodology applied to generate the diurnal cycle of CO2", a better way to "draw the attention of the modelling community to consider revising the current state of knowledge regarding the validity of CO2 diurnal cycle calculated via biosphere flux models used as priors in Bayesian frameworks" could be based on the estimate of the changes in flux inversions when using this Olsen and Randerson 2004 approach vs. FLUXCOM-X to define the diurnal cycle of the prior NEE (in addition to the current test with flat vs. FLUXCOM diurnal cycles).

Thank you for the suggestion! This analysis has been added to the discussion (L 448-468). We added results from CASA-based biogenic fluxes that were downscaled to hourly NEE by Carbon Tracker (CT– NOAA) based on the method presented by Olson and Randerson (2004), where it is typically used in CT global inversion.

The authors could, at least:

1) better document, in the introduction, the dominant types of prior estimates of the NEE diurnal cycles in current global and regional inversions. I find the sentence "However, not all atmospheric inversions account for the effect of diurnal cycle in biosphere-atmosphere exchange of CO2" (l. 92) quite misleading regarding this.

We added it to the introduction in the revised MS (L 92-109).

2) demonstrate, using past publications, that the level of the uncertainty (including biases) arising from methods like that of Olsen and Randerson 2004 and even from vegetation models with sub-diurnal resolution is so large that the order of magnitude and general mapping of the corresponding uncertainty in the flux inversions can be assessed from experiments with a flat diurnal cycle.

As a first indication, the additional analysis included in the discussion (based on the suggestion above) shows that there are large discrepancies in the diurnal variations between FLUXCOM-X and one product of Olson-Randerson-based methodology. We actually took into account comparing results available from other studies in the discussions. However, given the lack of studies regarding the diurnal cycle uncertainty among biosphere models, or methodologies applied to generate the diurnal cycle, it is still an open issue that calls for a dedicated study to investigate the level of uncertainty amid the current biosphere models, at least those used as priors in inversions. This is actually the objective of the follow-up study, which requires broader collaborations among the modellers.

The general discussion in our sequence of review / response regarding the relevance of these experiments should be better reflected in the manuscript.

All the points raised during the rounds of revisions are included.

Other points:

- title: I suggest generalizing "using CarboScope" to something like "global and regional atmospheric inversions", making sure that the manuscript definitely supports such a generalization (see above). Similar comment for line 95: the general objective of the publication should be broader.

Thank you for the suggestion! The title is now slightly modified based on the suggestion: "To what extent does CO_2 diurnal cycle impact flux estimates derived from global and regional inversions". Also, the objective indicated in the introduction has changed accordingly in the revised MS (L 110).

- I am still concerned by the following sequence of sentences (lines 71-75): "That is, in the early afternoon on any sunny day of the growing season, atmospheric CO2 concentrations are lower near the surface due to active photosynthesis while CO2 is lifted up and diluted by the strong daytime vertical mixing (Stephens et al., 2007). By contrast, during nighttime CO2

concentrations accumulate near the Earth's surface owing to ecosystem respiration under a shallow boundary layer. On average, the covariations between the atmospheric transport and terrestrial biospheric fluxes... »

=> I may misunderstand the term " covariations " here. At night, both the emissions and the lack of vertical mixing tend to increase the level of the concentrations close to the surface. But, again, during daytime, the high vertical mixing tend to attenuate the decrease of the concentrations close to the surface due to the photosynthesis, i.e. the overall diurnal cycle of concentrations close to the surface. So what are these " covariations " about ? The answer to this question may push for a simplification of this section of the introduction (see my comment in the previous review regarding this) and for a better focus of this introduction on current practices in atmospheric inversions (see above).

According to the suggestion, we rephrased this section in the revised MS (L 70-75), so as to clarify the concept of covariance between atmospheric vertical mixing and terrestrial fluxes. "Covariations" was basically referring to the covariance between the vertical transport and biogenic fluxes due to the same forcing initiating both.