Response to the reviews of manuscript amt-2022-179: "Reducing errors on estimates of the carbon uptake period based on time series of atmospheric CO_2" by Theertha Kariyathan, Wouter Peters, Julia Marshall, Ana Bastos, Pieter Tans, and Markus Reichstein to Atmos. Meas. Tech.

Questions from the reviewers are written in blue, our answers in black, text copied from the manuscript is written in *italic*, and all changes in the manuscript are typed in red. When referencing page and line numbers, we are always referring to the old version of the manuscript.

Please note that this document is an updated version of the response to reviews submitted on 15/12/2022.

During the review process we came across a study by Barlow et al., 2015, where the CUP is estimated using the first derivative approach. Although we developed our approach independently, we no longer claim the novelty of this approach. We rather emphasize the ensemble approach for uncertainty estimation and rename our method to EFD (ensemble of first-derivative method) and include an extensive discussion of Barlow et al., 2015 in our manuscript. The main changes are made to the introduction (line 65) and discussion (line 270).

Answers to Reviewer 1

R1.1

This study presents a novel method to estimate the carbon uptake period (CUP) from discrete CO_2 observation time series. The process of determining CUP from discrete time series includes two critical steps: curve fitting and CUP onset and end determination. Curve-fitting methods are needed to interpolate observation at gaps and to filter out the noise and undesirable modes of variability. When analyzing CO_2 mole fraction from background observation sites, this means removing the effects of local fluxes or synoptic scale transport variations. Previous studies have shown that the conclusions from the analysis of CO_2 time series are sensitive to the choice of the curve-fitting method. CUP estimates are also sensitive to the method used. Previous studies have proposed several methods that use the zero-crossing points or crest and trough of the detrended, zero-centered seasonal cycle. The study presents a new CUP estimation method and provides a detailed uncertainty assessment of the curve-fitting methods and compares them with other methods reported in the literature. The CUP method and the detailed uncertainty analysis of the different curve-fitting methods presented in this study are very relevant. Overall, the paper is well-written and the figures are clear. I recommend the publication of the paper after the following issues have been addressed.

We thank the reviewer for this positive and constructive review. Below, we answer all comments in detail and show the changes that we think have improved the manuscript.

R1.2

It is unclear which methods described in this paper are novel. The FDT method is new and innovative, however, I have reservations about the newness of the rest of the methods. In the abstract, the authors write "…a novel curve fitting method….". The essence of both CCG and the loess method presented here is the same, Equation 1. Is the novel part of the loess method using local regression to smoothen the residuals instead of a low pass FFT filter used in CCG? Or is it that the author's method uses a 2-degree polynomial and 4 harmonic functions while the CCG method uses a 3-degree polynomial and 4 harmonic function? Moreover, the study note that there is no difference in the performance of the loess and the CCG methods (line 254). Could the authors explain what is then the advantage of the proposed loess method? In the rest of the manuscript, the authors only claim the uncertainty generation and FDT methods are new (Line 64, 264 & 323). The ensemble-based method uses bootstrapping to evaluate the uncertainties of a metric. This is again not so new in my opinion. The main novel method presented in this study is the FDT method. I suggest that the authors (1) make clear which methods are novel. (2) restructure the method section so it does not over-emphasize the newness of the loess method.

Thank you for pointing this out. We agree with the reviewer, and have removed claims regarding the novelty of any of the methods used here. We emphasise now how the ensemble-based approach can improve uncertainty estimation by considering the year-to-year changes in the seasonal cycle.

R1.2 (continuation)
(3) if the ensemble-generation method is the same for the CCG and loess methods, describe the ensemble-generation in a separate subsection.

We agree, thank you for the suggestion. The method section has been modified as follows:

1) Added a new subsection 3.3 "Ensemble generation".
2) Moved subsection (*CCGCRV fitting and ensemble generation*) before subsection 3.3 with the following modifications:
    i) Name of section changed from *CCGCRV fitting and ensemble generation* to CCGCRV fitting.
    ii) Lines 166-171 have been removed ("*Further, we generate 500.....).*
3) this is followed by subsection 3.4, "Ensemble of first-derivative (EFD) method" , where we describe the EFD method and note the difference from Barlow et al., 2015.

R1.3
The authors have made a good attempt to describe the FDT method. However, I found it difficult to understand how CUP is calculated using the X% threshold. This statement is confusing: "The value of X is chosen to minimize the threshold value (as the rate of uptake towards the beginning and end of the CUP approaches zero) while keeping the uncertainty in timing across the ensemble members small". Does the authors mean the uncertainties are calculated as a function of threshold within the range of 0 to 20 percent, and the onset and termination times are the threshold points where CUP uncertainty is smallest? This becomes clearer in the results section but it will be good to move some of the explanation from the results section to the method section. Perhaps, a figure or an additional panel in figure 4 illustrating this would make the method easier to understand. I also have some concerns about the tested threshold values. Why only 4 discrete values of the threshold were tested? One can easily do this analysis over a continuum. Where does the choice of 0 to 20 percent come from? Why the range does not include positive threshold values, for example, something like -20 to 20 percent?

Thank you for pointing this out. We have followed up this suggestion with an analysis of the threshold over a continuum as suggested, and it is shown and discussed below.

For clarification: the first derivative threshold is determined separately for the onset and termination of the CUP. The threshold should be such that the uncertainty in the timing of the CUP (onset and termination) should be minimized. However, we also want the threshold to capture as much of the CUP as possible. Hence, an optimum threshold should offer a balance between the two requirements. If the seasonal cycle were regulated only by biospheric fluxes, then the CUP could be defined simply by the seasonal cycle maximum and minimum. However, higher latitude sites often have flat or multiple peaks, which leads to ambiguity in determining the onset of the CUP. Therefore, we need a metric that captures the CUP without being affected by the ambiguous timing of the peak. This metric uses the percentage of the first derivative (slope) defined by X.

When X is set to zero, the CUP then corresponds to the time period between the seasonal cycle maximum and minimum. By increasing X continuously to 25 for both the onset and end of the CUP, we see only a smaller fraction of this time period. By progressively increasing X, we truncate more of the drawdown period of the CUP, but we also avoid the ambiguity of the onset timing for the sites with flat peaks. We progressively increased the value of X from zero and found that there was no significant change in the uncertainty of the CUP timing mostly beyond 12-13% (Figure 1, blue boxes and beyond). To be on the safe side, we chose 15% as the threshold. Incidentally, previous studies using flux measurements have also used 15% of the maximum GPP as a threshold to define the start of the growing season (e.g. Wang et al., 2019). For clarity, only values of X from 0 to 20 are shown in the manuscript. Negative X values corresponds to points before the maximum and after the minimum of the seasonal cycle, which are outside the time period of interest.

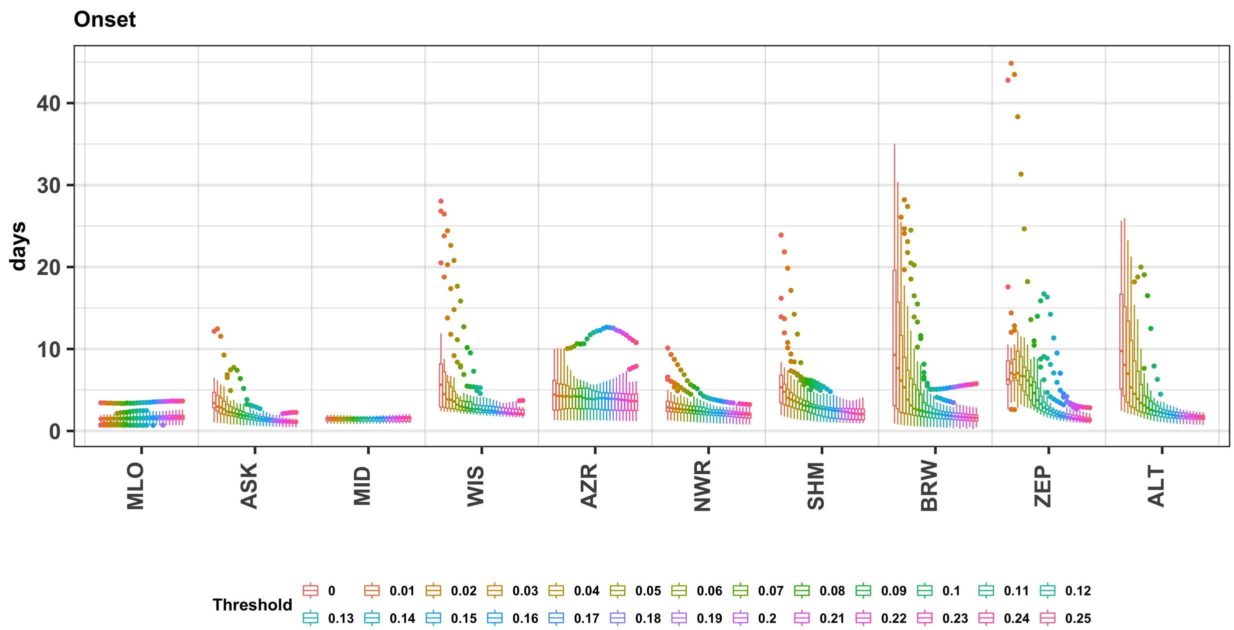

Figure 1. Similar to Fig 7 (a) in manuscript, tested for threshold over a continuum (X from 0% to 25%).

As noted by the reviewer, some of this explanation could be found in the result section, but we will modify subsection 3.2 and Figure 4 as suggested, so that the method is clear. The revised text reads as follows:

Line 151 is replaced with: The threshold is defined as X% of the first derivative minimum and X is determined separately for the onset and termination of the CUP. The onset/termination of CUP is defined as the closest point to the threshold value before/after the first derivative minimum (Fig. 4). The threshold for the onset and termination is chosen such that 1) the uncertainty in the timing of onset and termination is minimized across the ensemble members and 2) it represents as long a period as possible within the CUP. We varied the value of the parameter X until we found the optimum threshold. When X is 0%, it corresponds to the time period between the seasonal cycle maximum and minimum, including the full CUP but additional non-CUP periods may be erroneously included due to multiple peaks or flat maxima. By increasing the value of X we remove this error, but also truncate part of the "actual" CUP. Hence, we try to select a low value of X while reducing the uncertainty in the timing of the CUP.

Line 173 added (beginning of section 4): For the EFD method, we first optimize the threshold as described in Sect. 3.4. By continuously increasing X we found the optimum for the termination is 0% and for the onset it is 12-13%, with maximum CUP representation and no further reduction in the uncertainty beyond it. To be on the safe side, we chose 15% as the threshold (for onset) in all our analyses. Incidentally, previous studies using flux measurements have also used 15% of the maximum GPP as a threshold to define the start of the growing season (e.g. Wang et al., 2019). The result from varying X in steps between 0%-20% is shown in Fig 5.

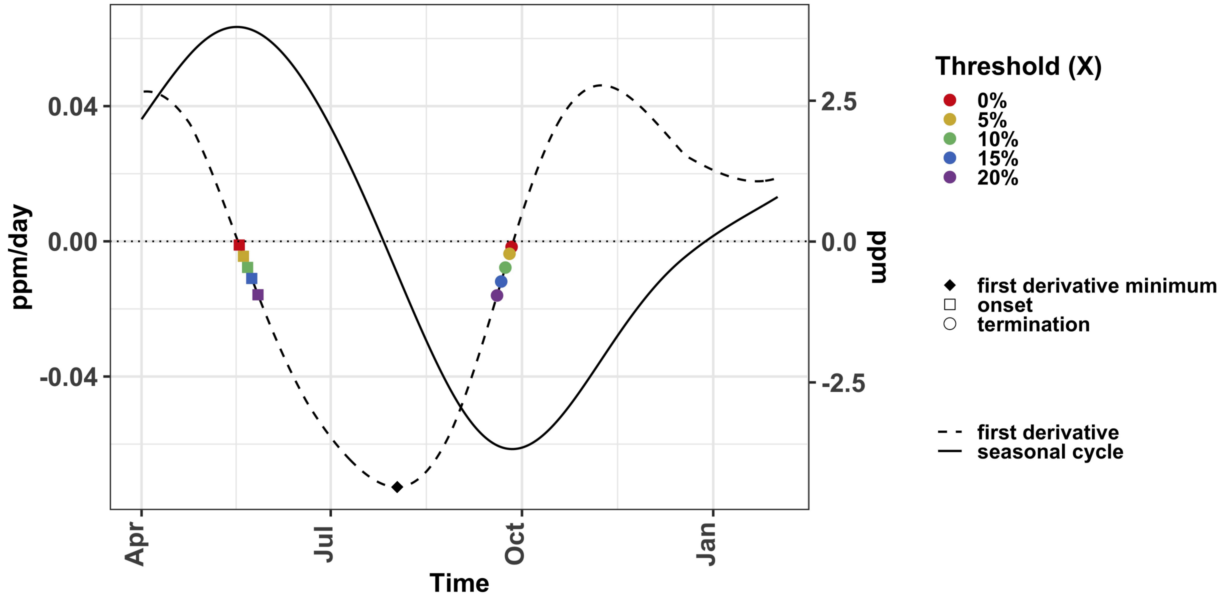

Figure 4. Schematic diagram showing the timing of the CUP as determined by the first derivative method. The timing is marked by a threshold, defined in terms of the first derivative of the CO_2 seasonal cycle. It is defined as X% of the first derivative minimum. The value of X is varied from 0% to 20% and the corresponding threshold value is marked on the seasonal cycle first derivative with different colored points. Their timing then defines the timing of the CUP for the different threshold values. The day of the onset and the termination of the CUP are defined by the points before and after the first derivative minimum respectively. The squares and circles denote the onset and threshold calculated with different thresholds.

R1.4

The study focuses on the importance of uncertainties in CUP estimates of the Northern Hemisphere CO_2 emissions when estimated using discrete measurements from select background sites. There are intra-annual variations and long-term trends in atmospheric transport which would affect the relationship between the seasonal cycle of the CO_2 observations vs the actual emissions (see Krol et al., 2018, Fu et al., 2015). The transport errors will not be an issue when the FDT is applied to a discrete fluxes time series. I suggest the authors add a discussion about the transport-variation-related errors when analyzing fluxes using remote background observation sites to the discussion section.
This is a very important point, thank you for mentioning it. We have now added the following lines in the discussion section about the transport-related errors.

Line 300 replaced: In this study we use the first derivative of the concentration time series as a proxy for the large-scale spatially integrated flux (Barlow et al., 2015), however, this should not be directly translated to the underlying flux fields. The atmospheric transport plays an important role in explaining a significant portion of observed CO_2 variations at various surface stations (e.g. Krol et al., 2018; Fu et al., 2015) that might affect any interpretation of the CUP metrics. An extensive study was carried out by Lintner et al. (2006), confirming the importance of atmospheric transport to account for some of the inter-annual variations in CO_2 observed at Mauna Loa. Murayama et al. (2007) showed how year-to-year changes in the atmospheric transport create significant inter-annual variations in the downward zero-crossing date of the

CO_2 seasonal cycle that cannot be neglected. Hence, we recommend that while using the EFD method, the contribution of atmospheric transport at the studied background sites should be evaluated before interpreting and relating the CUP metrics to sources/sinks.

R1.5
Technical corrections:
“Curve-fitting” is irregularly hyphenated in the text. It needs to be hyphenated when used as an adjective, for example in Line 6, 16, 19, and so on.
Line 256: “using two different curve-fitting methods ” => “using the two different curve- fitting methods” is better.

Thank you for pointing it out, this has been corrected.

Other changes:

- We found a bug in our code. In the residual bootstrapping (Fig 3 manuscript), the resampled residuals were added to the observation, rather than the first fitted observation values. This has been corrected. However, there are no significant changes in the results. The revised manuscript has the corrected values and figures.
- We now use acronyms ZCD replacing “zero-crossing dates” to be consistent with previous studies.
- Figures 8 and 9 and Figures 11 and 12 are grouped.
- Figure 13 has been simplified by not coloring the points by year.

References:

Barlow, J. M., Palmer, P. I., Bruhwiler, L. M., and Tans, P.: Analysis of CO2 mole fraction data: first evidence of large-scale changes in CO2 uptake at high northern latitudes, Atmospheric Chemistry and Physics, 15, 13 739–13 758, https://doi.org/10.5194/acp-15-13739-2015, 2015.

Fu, Q., Lin, P., Solomon, S., and Hartmann, D. L.: Observational evidence of strengthening of the Brewer-Dobson circulation since 1980, Journal of Geophysical Research: Atmospheres, 120, 10,214–10,228, https://doi.org/https://doi.org/10.1002/2015JD023657, 2015. gml.noaa.gov: Trends in CO2, [online] Available from:https://gml.noaa.gov/ccgg/trends/, accessed: 2022-06-8.

Krol, M., de Bruine, M., Killaars, L., Ouwersloot, H., Pozzer, A., Yin, Y., Chevallier, F., Bousquet, P., Patra, P., Belikov, D., Maksyutov, S.,Dhomse, S., Feng, W., and Chipperfield, M. P.: Age of air as a diagnostic for transport timescales in global models, Geoscientific Model Development, 11, 3109–3130, https://doi.org/10.5194/gmd-11-3109-2018, 2018.

Lintner, B. R., Buermann, W., Koven, C. D., and Fung, I. Y. (2006). Seasonal circulation and mauna loa CO_2 variability. Journal of Geophysical Research: Atmospheres, 111(D13).

Murayama, S., Higuchi, K., and Taguchi, S. (2007). Influence of atmospheric transport on the inter-annual variation of the CO_2 seasonal cycle downward zero-crossing. Geophysical Research Letters, 34(4).

Wang, X., Xiao, J., Li, X., Cheng, G., Ma, M., Zhu, G., Altaf Arain, M., Andrew Black, T., and Jassal, R. S.: No trends in spring and autumn phenology during the global warming hiatus, Nature Communications, 10, 2389, https://doi.org/10.1038/s41467-019-10235-8, 2019.

Answers to Reviewer 2

This study presents a curve-fitting method and an ensemble-based approach for quantifying the carbon uptake period (CUP; onset, termination and duration) from atmospheric CO_2 measurements. The authors have applied the technique to a handful of sites in the Northern hemisphere and shown that the uncertainty associated with the onset and termination of CUP is less with their proposed approach relative to more traditional techniques prevalent within the community. While the illustrations are high-quality, the scientific relevance and the overall flow of the manuscript needs to be improved. Right now, the manuscript reads like a collection of results based on investigations that were conducted and a figure and text to support the investigation. It does not dig deep into the implication of some of the findings (for e.g., Figure 13 is fascinating from a carbon cycle perspective but not explained in any great detail). In addition, the authors have applied their approach to only one seasonal cycle metric and it is not clear if the proposed alternative can be applied to other metrics. There are also inherent assumptions related to the first derivative method that require additional investigations. Along with my comments below, I have suggested a few basic analyses and additional sensitivity test that will improve this study and make it scientifically robust and appealing to the larger carbon cycle science community. I sincerely hope that the authors consider these suggestions. for improving the manuscript.

We thank the reviewer for the critical comments and questions raised. We believe that by answering these questions, the interpretation and portrayal of our results has been improved. Overall, the results and discussion section were restructured and partly re-written to explain our results more clearly. We focus now on the utility of the ensemble-based approach to quantify the uncertainty in the estimation of the CUP using the first-derivative method.

R2.1

Line 1 in the Abstract should read – 'High-quality, long time series measurements of ...'

Thank you, 1 has been modified as suggested.

Abstract. High-quality, long time series measurements *of atmospheric greenhouse gases show interannual variability in the measured seasonal cycles.*

R2.2 (This comment is broken up into three parts, which are addressed separately.)

R2.2.1

Lines 9 – 10: It is a bit misleading to claim that that the approach has been applied to analyze different seasonal cycle metrics as well as claims about the novelty of the approach. The authors have implemented this approach for quantifying one seasonal cycle metric, i.e., the carbon uptake period and associated parameters. What other metrics can be robustly calculated using this approach? It would be extremely relevant to include this in the discussion section.

We agree with the reviewer that the formulation was inaccurate. Here, the ensemble-based approach has been applied to only the CUP and associated parameters, however the approach can also be used to quantify uncertainty in other seasonal cycle metrics for example the seasonal cycle amplitude. Since this is not demonstrated in the study, line 9-10 has been modified:

Line 9-10: We use this ensemble-based approach to analyze the carbon uptake period (CUP: the time of the year when the CO_2 uptake is greater than the CO_2 release): its onset, termination and duration.

Moreover, we added a sentence on how the method can be applied to other metrics in the discussion (replaced Line 320 and moved it to start of discussion in the revised manuscript (Line 355)):

Line 320: Here we show that CO_2 seasonal cycle metric estimates can be strongly sensitive to the method used, hence any method must be thoroughly evaluated before it can be used to derive trends from the atmospheric data. In Barlow et al. (2015) the robustness of the first derivative is tested by evaluating its ability to capture a known trend from a synthetic time series. They found a larger threshold value for the onset (25%, suggesting a shorter CUP in their approach) from a synthetic data trend analysis in which they applied a linear trend with Gaussian variations of the peak uptake date to a CO_2 time series. However, we argue that the data-derived year-to-year uncertainty from our ensemble provides a more robust threshold estimate and we derived a tighter threshold than Barlow et al. (2015) (15% for onset). Further, Barlow et al. (2015) showed that their method can retrieve the true linear trend to within 10-25%. Our EFD-approach provides uncertainty on the year to year variability in the seasonal cycle metrics based on the fitted data residuals, which could be used in a trend analysis to give differential weights to each year. Also, trend analysis on the individual ensemble members would allow uncertainty on the trend to be calculated. Our demonstration of the EFD-method on the CUP could be extended to other metrics that are derived directly from the seasonal cycle in a similar way, for example the peak to trough amplitude, especially when curve fitting discrete data, at sites with broader or multiple peaks. In a similar fashion, the ensemble-based approach could be used to evaluate a newly proposed method or select an optimal method for evaluating any other metric based on reduced uncertainty.

R2.2.2

Right now, the Discussion section reads more like a collection of results than a true Discussion that provides scientific implications (see also comment #7) and relevance of this method for the carbon cycle community.
This will be addressed along with comment 7 (R2.7)

R2.2.3

In addition, the technique proposed by the authors are not new per se, but its application for quantifying the seasonal cycle metric is novel – the authors need to clearly distinguish this throughout the manuscript.
Thank you for pointing it out. The parts of texts which claim the novelty of the methods used here have been modified (e.g., Lines 7, 64, and 264). Further changes were made to address a similar comment from Reviewer #1 (See R1.2). We also include an extensive discussion of Barlow et al., 2015, a study that had previously introduced the first-derivative approach.

R2.3

Lines 21 – 22 – the statement is applicable to not just measurements made at Mauna Loa, but almost all atmospheric CO_2 measurements, be it in situ or remotely-sensed. Kindly rephrase to either make it more generic or more specific to Mauna Loa.
Thank you, we agree that this should be corrected. Lines 21-22 have been modified based on this comment as follows:

Line 21: Ongoing in-situ measurements of the atmospheric CO_2 mixing ratio have revealed an increase in CO_2 mole fraction in the atmosphere. *The increase in atmospheric* CO_2 *due to release of carbon from fossil fuel burning and land-use change is buffered by net* CO_2 *uptake by the ocean and land biosphere (Keeling, 1960).*

R2.4

Line 50 – The authors should be more specific about which metrics they are talking about and specify the ones that are highly sensitive to data gaps or noise in the time- series.

Thank you for the comment. An example is given in Line 51 (*One example is the timing of the carbon uptake period (CUP)*), but the lines were rephrased to improve clarity. Lines 50 -55 have been modified as follows:

Metrics derived from CO_2 time series such as the seasonal cycle peaks can be highly sensitive to data gaps and noise. This is especially true for metrics associated with the growing season onset at higher latitude sites, where CO_2 time series show flat or multiple peaks in winter (Barlow et al., 2015). Hence, deriving other metrics like the timing of the carbon uptake period (CUP) from the seasonal cycle maximum results in less robust estimates.

R2.5

Line 69 – 70 - What about checking an alternate approach? In the Introduction, the authors made the argument that multiple approaches should be tested. Why haven't they implemented that rationale here?

We agree that the lines are confusing. Here, we want to test the robustness of the EFD method. We want to understand if the low uncertainty while using the EFD method is dependent on the specific curve-fitting method used here. This is why we use another curve-fitting method and test the EFD method using both. We observe that, for both curve-fitting methods used, the EFD method leads to lower spread in the estimated CUP.

We corrected the sentence as follows:

Line 69 : We also tested if the EFD method is sensitive to the specific curve-fitting method applied by fitting the data with the commonly-used CCGCRV method, which is a frequency-domain-based filter, similar to the wavelet transform approach of Barlow et al. (2015).

R2.6

Lines 146-152, Page 7 – A big assumption in implementing the FDT approach is that "the first derivative of the CO_2 dry air mole fraction is a proxy for the flux", thereby completely ignoring the role of atmospheric transport. This is especially relevant as the majority of sites the authors have selected are the marine boundary layer sites, which are designed to sample the background flux and not necessarily changes in local flux. Can the authors demonstrate the robustness of their assumption by doing pseudo-data / simulated data experiments? For example, the authors can use known fluxes from CarbonTracker or CarbonTracker-Europe, generate pseudo-data at the sites used in the study, and demonstrate that the first derivative is indeed an approximation of the flux signal.

Thank you, this is a very important point. We consider the first derivative to be a proxy for the seasonal cycle of hemispheric-scale NEE, not a one-to-one measure of local fluxes. The seasonal variability in atmospheric CO_2 at background sites should reflect the spatial integral of the fluxes over large latitudinal or hemispheric scales, but the area of integration is affected by atmospheric transport, especially at marine boundary sites as mentioned by the reviewer. To address the reviewer's concern, we performed a synthetic data experiment using perturbed NEE from the Jena CarboScope inversion to test the accuracy of the EFD method in deriving a prescribed change in CUP.

We did simulations in which idealized NEE fluxes were transported forward and the atmospheric concentrations were sampled at the location of the measurement sites. In the baseline simulation, a fixed year from the Jena CarboScope Inversion (Rödenbeck et al., 2003) (doi:10.17871/CarboScope-sEXTocNEET_v2021) was used to generate an idealized NEE flux time series that has no interannual variability (IAV) in the CUP_NEE (CUP of the NEE flux) at any given pixel. Then, we prescribe changes to the CUP_NEE at Northern Hemisphere land pixels with clear seasonal cycles by steps of -10,-8...+8,+10 days, creating different ΔCUP_NEE scenarios (change from baseline CUP_NEE). The fluxes were transported forward using the atmospheric transport model TM3 (Heimann and Körner, 2003) with wind fields from a fixed year (to remove transport IAV) and the resulting change in the CUP derived from the simulated CO_2 mixing ratio(ΔCUP_MR) was compared to the ΔCUP_NEE. We calculate ΔCUP_MR using both the EFD method and zero-crossing method and compare their sensitivity to ΔCUP_NEE.

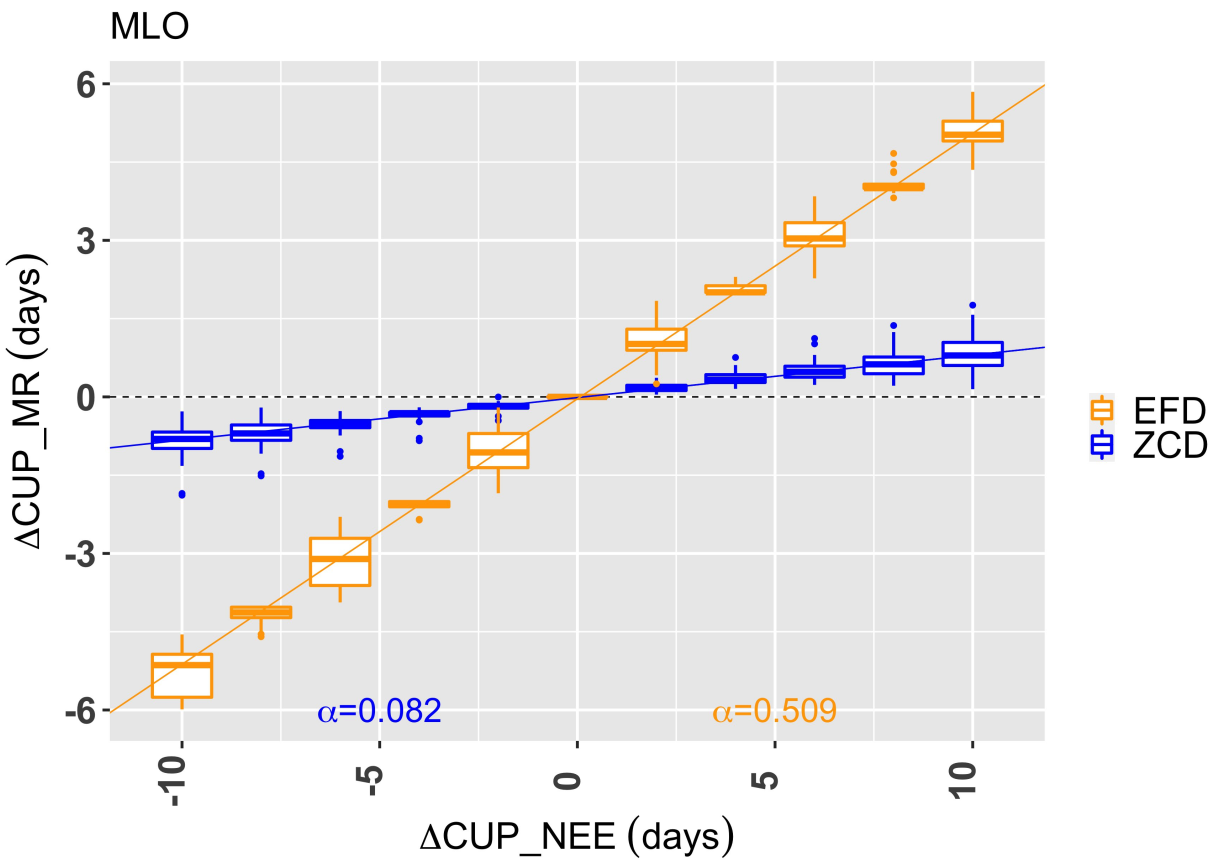

Figure 1: The boxplot shows the ΔCUP_MR over the studied years estimated using the zero-crossing method (blue) and the EFD method (orange) at MLO. 'α' denotes the slope of the regression line fitted to the median of the boxplot.

We find that the EFD-estimated ΔCUP_MR has a strong linear relationship to the applied ΔCUP_NEE, but it returns a ΔCUP_MR value smaller than what was applied in flux space. How much smaller depends on the station, for MLO (shown in the figure 1), it is quite close to a factor of two. The zero-crossing-estimated ΔCUP_MR has a shallower slope relative to EFD estimation. This shows that the zero-crossing method is relatively less sensitive to the changes in the "actual" CUP. These differences are shown for the station MLO in Figure 1. This implies that the EFD-derived CUP is likely a more robust metric than the zero-crossing-approximated CUP, but indeed should not be interpreted as a direct one-to-one signal of the underlying flux field. Transport to the sites, and mixing of spatially varying NEE signals with differing CUP timing integrate to a reduced atmospheric expression of CUP changes in biospheric fluxes. That integration depends on the site location, which needs to be taken into account. We believe this requires a more detailed study about the influence of transport on signals received at the background sites, which is not within the scope of this study. Therefore, we have modified the discussion to indicate that the role of transport should be considered when studying the observed CO_2 seasonal cycle.

Line 300 replaced: In this study we use the first derivative of the concentration time series as a proxy for the large-scale spatially integrated flux (Barlow et al., 2015), however, this should not be directly translated to the underlying flux fields. The atmospheric transport plays an important role in explaining a significant portion of observed CO_2 variations at various surface stations (e.g. Krol et al., 2018; Fu et al., 2015) that might affect any interpretation of the CUP metrics. An extensive study was carried out by Lintner et al. (2006), confirming the importance of atmospheric transport to account for some of the inter-annual variations in CO_2 observed at Mauna Loa. Murayama et al. (2007) showed how year-to-year changes in the atmospheric transport create significant inter-annual variations in the downward zero-crossing date of the CO_2 seasonal cycle that cannot be neglected. Hence, we recommend that while using the EFD method, the contribution of atmospheric transport at the studied background sites should be evaluated before interpreting and relating the CUP metrics to sources/sinks.

R2.7 and R2.2.2

Section 5 – Discussion – other than a few segments, this section seems to be a continuation of the previous section. The authors need to rethink the way they present this section, move the results to the previous section and/or focus more on the scientific implications of their findings. It would also be useful to dig deep into a couple of the results and talk about the scientific findings rather than present one result after the another.

Thank you for the suggestion. The separation of the results and discussion section has been improved. To do this we have reordered some of the sections, separating the results from the discussion accordingly. A summary of the resulting structure is given below.

Results

- Comparison of the three CUP calculation metrics is presented in Figures 6, 7, 8 and 9.
- Fig 13 (now Fig. 8) and its explanation (Line 297) and additional observation that: The X-axis range, showing the CUP from ZCD in Fig.8, is unlikely to represent the "actual" year-to-year variation in the CUP, with the largest variation seen at MLO, NWR and MID. (This will be further explained in the discussion)
- Further testing using CCGCRV fitting, presented in Figure 10 and 11.

Discussion

In the Discussion we draw upon Figures 5 and 10 to interpret the figures presented in the Results section above. We have further included the other modifications following the reviewer comments. Figure 10 shows that years with a similar duration of the "actual" CUP can have different CUP duration when determined with the zero-crossing method, explaining the large year-to-year variation in the X-axis of Fig. 13.

R2.8

Figure 13 - What are the conclusions from this figure? Do we show any important trends? Any relevance to carbon cycle science? Similar to the previous comment, this seems another missed opportunity to delve deeper into the results and provide scientific implications and context for the results. I would strongly recommend the authors to select a few key results and figures, and then delve deeper into them rather than presenting all results and figures generated during their investigation.

Thank you for these questions. Analyzing Fig 13 in light of these questions improves our understanding of the obtained results. Our observations are summarized as follows and will be included in the discussion.

We find that in addition to having a larger annual uncertainty, the range of CUP values over the study period for the ZCD approach is much larger than that of the EFD approach for some sites (Fig. 8). For example, at MLO the zero-crossing-approximated CUP ranges from 100 to 250 days, corresponding to a period of 3-8 months. Changes in the growing season in the Northern Hemisphere are not expected to be this large. As an example, Jeong et al. (2011) estimated the length of the growing season using satellite measurements of normalized difference vegetation index (NDVI). When integrating over the temperate northern hemisphere, the length of the phenology-derived growing season was found to vary by less than 25 days from 1982-2008. The ZCD approach includes changes in both the latter part of the net uptake period and the early release period, making it difficult to separate the contribution of the net uptake and net release periods to the changes in the CUP estimate. To understand this large spread in CUP, we compare two years with very different CUP values estimated by the ZCD at MLO, 1992 with 192 days and 1998 with 147 days (Fig 11). We find that the difference in the CUP estimate is due to the change in the early release period, whereas the uptake periods are essentially the same. When using the EFD method, by contrast, the two years show similar CUP, 134 and 126 days, respectively. By definition, the EFD is not affected by differences in the net release period, and therefore provides more robust CUP duration estimates.

Atmospheric transport can contribute to the inter-annual variability in CUP estimates while using both the EFD and ZCD. However, the ZCD is influenced by transport variability in both the late uptake and early release periods. Hence, changes in the early release period could be erroneously interpreted as changes in the CUP when using the ZCD. Years with extreme CUP approximated by the ZCD suggest that there is reduced net respiration in the early release period, thereby prolonging the time to reach the UZCD. This is determined by the interplay of the CO_2 uptake and release processes, which are influenced by physical factors like temperature, soil moisture and solar radiation. For example, in dry conditions there is less respiration by plants and slower decomposition of organic matter in the soil, resulting in reduced CO_2 release to the atmosphere (Yan et al., 2018). The rate of decomposition further depends on the snow cover and available detritus content in the soil following leaf senescence. Furthermore, in the early release period, when the solar radiation is not limited, plants may continue to photosynthesize depending on water availability and temperature, leading to reduced net CO_2 release. Thus, in years with extreme CUP as approximated by the ZCD, the physical processes that affects the release period should be investigated. In comparison to the CUP definition, the approximation by the ZCD is also sensitive to variations after the summer minimum, i.e. during the early release period. A more thorough investigation of the sensitivity of the EFD and ZCD to CUP interannual variability would require dedicated modelling experiments, which is beyond the scope of the current study.

Other changes:

- We found a bug in our code. In the residual bootstrapping (Fig 3 manuscript) the resampled residuals were added to the observations, we corrected this by adding the residuals to the first fitted observation values. However, there are no significant changes in the results. The revised manuscript has the corrected values and figures.
- We now use acronyms ZCD replacing “zero-crossing dates” to be consistent with previous studies.
- Figure 8, 9 and Figures 11,12 are grouped.
- Figure 13 is simplified by not coloring the points.

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