Comment on amt-2022-179
Anonymous Referee #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 CO₂ 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.

Comments:

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

- 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.

- 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.

- 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?

- Lines 146-152, Page 7 – A big assumption in implementing the FDT approach is that “the first derivative of the CO2 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.

- 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 other.

- 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.