

Interactive comment on “A segmentation algorithm for characterizing Rise and Fall segments in seasonal cycles: an application to XCO₂ to estimate benchmarks and assess model bias” by Leonardo Calle et al.

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We thank both reviewers for their comments and suggestions. We know this paper is technical work and requires valuable time for such reviews. We think text added to the manuscript as a response to reviewers has made improvements and helped clarify assumptions and interpretation.

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1 Response to Reviewer 2

Review2 comment1: Determining the amplitude and phase of a time series is a notoriously difficult problem, especially a time series with a superimposed time-dependent trend, normally requiring a lengthy time series to minimise the effect of edge effects. The GOSAT record runs from 2009 to present so I am curious while they curtailed their analysis at 2012.

Author's Response R2C1: We clarified in the methods section '2.1 Satellite XCO₂ data' that 'Satellite data was freely obtained and analyzed only for 2009-2012 because it corresponded to the overlapping timeframe of available simulation data.'

Review2 comment2: Armed with only a few seasonal cycles the authors will find it difficult to properly remove the lower frequency variations, which will arguably pervade the column measurements more so than surface measurements. The authors have used a spectral method to remove short-term variations less than 80 days. It would be useful (for at this reader) to understand why they chose that value as a cut-off.

Author's Response R2C3: We used an 80-day cutoff value because it was specified as the standard value to remove short-term variations in seasonal cycle analyses when using the ccgrv algorithm (Pickers and Manning 2015; also, described in <<https://www.esrl.noaa.gov/gmd/ccgg/mb/krvfit/krvfit.html>>). To our understanding, and according to Thoning et al. (1989; pp 8558, 2nd para.; <https://doi.org/10.1029/JD094iD06p08549>), a low pass filter of 50 days was originally applied to remove shorter-frequency variations in the data that were unrelated to large-scale atmospheric mixing. That is, the intention of the low pass filter of 50-days was to retain month-scale variations in

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the atmospheric data. Apparently, the standard was since extended to 80-days for the short-term cutoff so that only variations that were evident, or maintained, for the time scale of 3-4 months were retained (3-4 month in the frequency domain is 4.56 cycles/yr). In the end, we thought such a cutoff was suitable for this analysis because seasonal-scale variations are of general interest to terrestrial carbon cycle scientists. We added the following clarifying sentence to the text.

Author's changes to text R2C3: 'The cutoff for the short-term filter was set at the recommended value of 80 days (Thoning et al., 1989). The short-term cutoff of 80-days retains data variations that are evident, or maintained, for the time scale of 3-4 months (4.56 cycles/yr).'

Review2 comment3: I thought that the math was presented in an unnecessarily complicated way. Surely, the second derivative and first derivative taken together are sufficient to determine the peak, trough and any saddle point found in the time series. Saddle points can be found in Arctic seasonal cycles, for instance.

Author's Response R2C3: Yes, we tend to agree. We had simplified the text description as such, but chose to also provide a mathematical description for those inclined towards symbols or for reproduction of the procedural steps of the algorithm without having to review the computer code. We would like to keep the mathematical level at this length, if there is no strong objection.

Review2 comment4: Nevertheless, the method appears to be sound. The authors appear to focus on model evaluation instead of using the method to improving understanding of the carbon cycle. Consequently, there is little in the way of physical interpretation of the metrics in sections 3.2 and 3.3.

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Author's Response R2C4: Yes, good point; we struggled with this ourselves given space limitations in describing the algorithm, the evaluation, and subsequent interpretation of models. We tried to outline future approaches in the Discussion for such interpretations. The issue is that we deal with a convolution of near- and far-field surface fluxes. We think the methods and algorithm presented in this study are a step forward towards the attribution of variation in the seasonal cycle metrics.

Review2 comment5: How do the authors take into account the uncertainties associated with the column data?

Author's Response R2C5: We use the Level-2 product that contains only high-quality and bias-adjusted data points. With regards to additional uncertainties in the satellite column data, we assume that uncertainties are random and normally distributed around zero, such that they average-out when taking the mean of all data points within a region. Spatially-averaged column uncertainties can be minor for seasonal cycle analyses if only considering the effect of the averaging kernel (0.15 ppm on average; Lindqvist et al. 2015 <https://doi.org/10.5194/acp-15-13023-2015>), but could amount to larger errors (1.5 ppm) if instrument noise, the main source of uncertainty, is also considered (Yoshida et al. 2011 <https://doi.org/10.5194/amt-4-717-2011>). We added the following caveat to the text in the methods section:

Author's changes to text R2C5: 'Satellite data have uncertainties of their own based on instrument noise, version of retrieval algorithm used to filter atmospheric effects, and averaging kernels (Yoshida et al. 2011, Lindqvist et al. 2015). A full quantification of uncertainty in satellite-derived seasonal cycles is beyond the scope of this study, but such an analysis could be useful for benchmarking purposes as models continue to reduce large biases

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(» 1.5 ppm). Nevertheless, we make the assumption that lower biases are generally indicative of better model performance.'

Review2 comment6: For the model analysis, do the authors sample the model when/where there are observations?

Author's Response R2C6: Yes, we use a co-location method to sample the simulated data. Clarifying text was updated as below, ref. Guerlet et al. 2013 <https://doi.org/10.1002/jgrd.50332>

Author's changes to text R2C6: 'We then used 'co-location' sampling of the ACTM XCO₂ data to match the location and timeframe (13:00 hr local time) of observations, \pm 5 days to account for (i.e., by averaging) sub-weekly transport errors (Guerlet et al., 2013).'

Review2 comment7: Line 350: "We suggest that the latitude of the inversion of period asymmetry is a characteristic indicator of global atmospheric dynamics and biosphere productivity." It would be useful for the reader to understand the origin of this suggestion.

Author's Response R2C7: We appreciate the suggestion. We replaced text and clarified as below:

Author's changes to text R2C7: We hypothesize that the latitude at the point of inversion of period asymmetry is a characteristic indicator global atmospheric dynamics and biosphere productivity. Our rationale is that if (i) the primary driver of the period of drawdown (Fall) or release (Rise) in XCO₂ seasonal cycles is the terrestrial biosphere, and (ii) DGVMs themselves simulate the terrestrial biosphere, then variation in the simulated point of inversion of asymmetry by different DGVMs suggests a strong

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influence of biosphere activity on this emergent pattern. The most obvious driver affecting the period being plant phenology. However, we already know that seasonal cycle in XCO₂ is dominated by flux seasonality in land biosphere, with the ocean and fossil fuel emission seasonality plays only a secondary role.

Review2 comment8: Line 360: "It may be possible to add this emergent pattern as a benchmark to evaluate models that attempt to reproduce more direct indicators of biosphere activity..." How important is atmospheric transport in determining zonal variations in this emergent pattern?

Author's Response R2C8: The effect of transport on zonal variation of this emergent pattern is likely to be large (Fig. 13 in Basu et al.; doi:10.1029/2011JD016124). Please note that the transport model (JAMSTEC's ACTM) used in this study generally performs well while evaluated against SF₆ measurement, a tracer of atmospheric transport (<https://www.atmos-chem-phys.net/11/12813/2011/>; doi:10.1038/nature13721).

Review2 comment9: For the reasons outlined in the (balanced and frank) discussion I am left wondering how the metric will be used to "correct" models given the uncertainties associated with emissions from fossil fuel combustion and cement production. Could similar patterns emerge from nature and models for different reasons?

Author's Response R2C9: We know contribution of fossil fuel and cement (FFC) emissions will be less influential in seasonal cycle metrics. This is not to say that seasonality in FFC emissions is absent, but more so that the biosphere imprints a much larger signal on these patterns (Fig.

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4; doi:10.1186/s40562-017-0074-7). Yes, similar patterns could emerge from nature and models for different reasons, and we think that the time-stepping of simulated processes in most models does not lend itself to realistic timeframes of surface fluxes that, ultimately, influence seasonal patterns in XCO₂. For instance, the timing of fire and deforestation has a strong seasonality in the tropics (burning and clearing during dry seasons) and is implicit in the satellite data, but such seasonal dependence is lacking in model schemes. In this sense, the idea that these benchmarks will help correct models might be overstated. Perhaps it is better to suggest that models move toward these benchmark by first understanding the limitations in direct comparisons of modeled surface fluxes to atmospheric XCO₂. While potentially of great value to modelers, global ecosystem models were never designed with goal of using large scale emergent patterns in XCO₂ as benchmarks so there are some basic hurdles to overcome.