

Response to Reviewer 2

We would like to express our gratitude to reviewer 2 for their insightful and constructive suggestions to reformat the paper and extend our analysis to additional model estimates in CarbonTracker2019. We have done our best to use these comments as a guide to composing a revised version of the paper that improves and expands on our original submission.

1 Comments and responses

1. **Comment:** My largest concern with the paper is that the authors are underutilized their contact tracing simulations. The contact tracing approach was not clearly explained, and results from these simulations were introduced only in the discussion section. This needs to be fleshed out more in the results section, with a mind to new or refined supporting figures. Fig. 10 could be modified to better guide the reader as to the similarities noted with Fig. 4. For example, could boxes be added in Fig. 10 around the zones whose amplitude is in the top Nth percentile in Fig. 4? Related to the contact tracer approach, it was unclear why the CarbonTracker posterior fluxes were run through GEOS-Chem rather than simply using the CarbonTracker posterior CO₂ fields. Was this so the transport model was consistent for the CO₂ simulation and the contact tracer simulation? If so, this should be explained in the text. However, the use of a single transport model is a major limitation to the authors' conclusion that accumulation of transported CO₂ controls the seasonal cycle amplitude spatial patterns. It may be onerous to repeat the surface contact tracer analysis in another transport model, but could the authors combine information from the CarbonTracker (TM5) posterior and their own CarbonTracker/GEOS-Chem simulations to substantiate their conclusion?

Reply: We now show results with CarbonTracker2019 X_{CO_2} , as well as GEOS-Chem. The X_{CO_2} amplitudes are generally similar in GEOS-Chem and CarbonTracker/TM5, with some expected differences attributable to the different transport schemes used in GEOS-Chem and TM5. The similarity of X_{CO_2} SCA in GEOS-Chem and CarbonTracker/TM5 while using the same terrestrial and ocean fluxes suggests that large-scale atmospheric transport that influences SCA is similar in the two models. We are then able to diagnose the role of transport in GEOS-Chem using the novel surface contact tracers. Furthermore, we have made the results from our contact tracer simulations a more integral part of the paper by including dedicated methods, results, and discussion sections (see Sect. 2.7, 3.5, and 4.1 of the revised manuscript), which now provide more details on this component of the analysis.

2. **Comment:** A second major issue is that the key insights from the research could be better laid out. Is it that the column seems to show earlier drawdown than inversions constrained by surface observations? The different behavior in Siberia compared to other zones? What are the implications for a correlation between amplitude and HDD in terms of processes that affect the seasonality of net exchange? Perhaps reorganization of the discussion section, with the surface tracer analysis presented in the results, will make it easier for these points to come through in the discussion.

Reply: We have reformatted the paper, with the tracer analysis presented in the results and more streamlined discussion. We state in that the correlation between SCA and HDD would be interesting to consider in future studies, but do not know what is driving this relationship at this time.

3. **Comment:** I struggled with Section 3.2, since the section begins with a listing of material in the supplement. The authors should organize this information to provide a clear summary in the first few sentences of the paragraph/section, and then later refer to the supplementary figures for more details. In particular, the "unrealistic drop in wintertime values" might warrant a figure in the main text since the realism of the seasonal cycle fits is crucial for interpretation of the rest of the paper.

Reply: This section has been revised to make it more clear. We think that it is reasonable to refer the reader to the supplement if they are interested in the exact nature of how the winter data gaps affect seasonal cycle shape, rather than including an additional figure. Some details have also been added to the methods section on seasonal cycle fitting at the

behest of reviewer 1 that may be pertinent to this comment.

4. **Comment:** The term HDD was used before the concept was introduced or defined (L29 p7).

Reply: Text has been revised to correct this.

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5. **Comment:** The authors cite a meridional gradient in Fig. 12, which I didn't see convincingly in any panel. This is brought up in the discussion (p14), and I don't think removing this sentence would affect the authors' underlying argument. If it is left in the document, more support is required.

Reply: This sentence was removed.