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Interactive comment

Interactive comment on "Variability in a four-network composite of atmospheric CO_2 differences between three primary baseline sites" *by* Roger J. Francey et al.

Anonymous Referee #2

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Summary:

Francey et al. present an analysis of monthly data for three measurement stations (MLO, CGO and SPO) to investigate the interhemispheric CO2 difference (IH CO2, defined as concentration difference of MLO-CGO or MLO-SPO) variations over the last 25 years. After comparing the different data sets, the potential biogeochemical drivers for the short-term, seasonal and inter-annual variability of IH CO2 is discussed. The manuscript highlights the ability of different atmospheric transport indices to explain these IH CO2 variations. The manuscript is well-structured and contains important and refined ideas that will be useful in guiding the improvement of GCMs commonly used

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Discussion paper

in global carbon cycle modelling. Its topic is well suited for ACP and surely of interest to the wider scientific community. However, two general comments and a few minor comments should be addressed before publication.

General comments:

One key argument of this paper is that the variations of IH CO_2 cannot be explained by the net uptake of CO_2 in the NH terrestrial biosphere. Unfortunately, this assumption is based only on a single DGVM, which are known to be highly uncertain. A comparison of CABLE and other models can be found here: https://journals.ametsoc.org/doi/pdf/10.1175/2008JCLI2378.1. Multiple DGVMs or an ensemble mean of CMIP LSMs would be more robust here. When considering fluxes from optimized emission products (e.g. CarbonTracker-EU) a significantly larger share of IH CO_2 could be explained by the NH terrestrial biosphere, although likely not contradicting the finding that IH CO_2 is strongly influenced by transport processes. The manuscript does not use consistent language around the main driver of NH CO_2 enhancements. The claim that respiration is maximal in early parts (FEB-APR MAY-JUL) seems unlikely and needs to be supported. However, later the authors refer to NBP and "terrestrial emissions" (which could likely peak in different seasons than autotrophic & heterotrophic respiration. As this is a key issue a clarification would be most helpful.

Specific comments:

Line 31 and Line 44: Why is the proposed method only compared to growth rate studies here and not to 4D-VAR and ensemble kalman filter/smoother data assimilation systems that have been shown to reproduce global scale fluxes with fairly reasonable performance?

Line 55: Figure 1 seems to suggest that with-in network errors do not cancel our. No consistent offset between the different measurement programs was found for all sites. However, to assume that the difference is small compared to the IH CO_2 signal seems perfectly reasonable.

Line 60: Multi-species observations are mentioned here, but not discussed in this manuscript. Examples of relevant species and isotopes should be given (or paragraph removed).

Line 63+: The authors do not mention the added value of quasi-continuous data from in-situ observations. If they were available (or used here), issues relating to the impact of different sampling dates and sampling frequencies of the different flask programs could assessed more quantitatively .

Line 122: Unclear what kind of information is available or referenced in Keeling 1998. It would be great to have more details .

Line 131. Please clarify: the maximum of 7-10ppm of IH CO2 occurs when most exchange occurs between NH and SH? Seems counter-intuitive or do you talk about the specific gradients of two sites here?

Line 133: Do the offsets really “cancel” here? (see Line 55).

Line 155: Why are those definitions of seasons used here and not the more common meteorological or astronomical definitions? Please consider clarifying.

Line 167: Why was the decision made not corrected or flag the flask data but “average out” potential outliers by making a composite time series?

Line 172: Please provide a reference on the assumption that maximum respiration from NH forests occurs in the seasons in FEB-APR and MAY-JUL? This seems fairly unlikely, as especially FEB-APR is still cold in most of the boreal forest regions in the NH and both autotrophic and heterotrophic respiration typically increase later in the year e.g. in (late) summer. Or please clarify if this refers to net biome productivity or net ecosystem exchange or "terrestrial emissions" (mentioned later in the manuscript).

Line 183: The data in table 1 seems inconsistent with the example here. In general, 1992-2016 is mentioned, but later 1992-2017 is shown. (Figure in supplement has 288 months of maximum data, main text figure refers to 300 months maximum).

Line 190: Please clarify "generally attributed to NH forest..." this manuscript argues (in later sections) that 1.) the IH CO2 gradient is dominated by transport and that 2.) the variability in the IH CO2 is driven by the MLO time series. Is this consistent?

Line 199: Why is 2017 now included (see comment L183)

Line 214: Some technical detail on how the fit was done would be most useful to the reader here, did you perform a sensitivity analysis for different types of splines?

Line 220: Please clarify: the assumption that the global atmospheric CO2 budget is not driven by IH transport but by exchanges to other compartments (biosphere, oceans, etc.) does not have to rely on assuming that transport is correct. The total global atmospheric CO2 budget is the same no matter if CO2 is in the NH or SH. I assume the authors want to argue that the NH versus SH budget might be significantly wrong when not accounting for IH exchange or maybe that the sites used (e.g. MLO) reflect more signals than just emissions and sinks in NH?

Line 235+: Why was the CABLE model used. DGVMs are inherently uncertain, so using multiple DGVMs or an ensemble estimate would give a better representation of the range of estimates of NH terrestrial fluxes. Optimized emission products, e.g. CarbonTracker-Europe report significantly higher NH terrestrial uptake (same order of magnitude as IH processes discussed later in the manuscript and in Figure 4). Available for download at: http://www.carbontracker.eu/fluxtimeseries.php [See also general comment]

Line 253: The qualitative study on the potential impact of CO2 emissions on global and hemispheric CO2 concentrations seems very instructive. However, a more qualitative estimate using a GCM could be beneficial here.

Line 286: The authors raise an interesting point here: the MLO-SPO and MLO-CGO data is "effectively identical". Why was SPO data included in this study? A separate study of CGO-SPO could have been more enlightening if it could address the question of CO_2 uptake in the Southern Oceans.

Line 345: What are the uncertainties of the estimated annual concentration trends and are they statistically significant (and different)?

Line 368: Here, the manuscript refers to "terrestrial emissions" is this equal to F(ffco2)+NBP?

Line 404: How would the IHCO2 impact the growth rate of CO_2 derived at other long-term reference sites in the NH. Like Barrow, US and Alert, CA? Could analyzing data from those sites help to further separate the impact of IH mixing versus NH fluxes?

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