Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-300-AC2, 2019

Atmospheric Chemistry and Physics Discussions

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.

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Received and published: 26 August 2019

NOTE: THE LINE NUMBERS IN RESPONSES REFER TO A REVISED MAIN TEXT ATTACHED AS A SUPPLEMENT TO THESE COMMENTS.

Anonymous Referee #2 Received and published: 16 July 2019 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

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Discussion paper

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 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 CO2 cannot be explained by the net uptake of CO2 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.

RESPONSE: A 16-DGVM ensemble of extra-tropical NBP from Bastos et al. 2018 is now included (lines222-225), including representation in Figure 4. It confirms the surface flux anomalies are too small to account for IHDCO2 changes.

When considering fluxes from optimized emission products (e.g. CarbonTracker-EU) a significantly larger share of IH CO2 could be explained by the NH terrestrial biosphere, although likely not contradicting the finding that IH CO2 is strongly influenced by transport processes.

RESPONSE: We do not discuss air-surface fluxes derived from CO2 data that are less spatially representative, and/or rely on atmospheric transport modelling. The latter introduce additional model degrees of freedom and potentially overestimate terrestrial variability if the variability in atmospheric IH transport is not adequately captured" (lines 224-227).

The manuscript does not use consistent language around the main driver of NH CO2 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.

RESPONSE: This is a valid criticism. The scope of this paper is to explore and seek explanation for variations in the baseline CO2 records. While there are clear implications for the global carbon budget, lack of advanced knowledge in every individual component of the budget limits a more comprehensive approach. Reducing uncertainty in the critical atmospheric component of the global budget is the purpose of this study. This is clarified in Lines 68-73 of the rewritten Introduction an in responses to Specific comment Lines 31, 131, 172, 190.

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?

RESPONSE: The Introduction rewrite aims to clarify this. See also lines 222-225 generated in response to General comments.

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 CO2 signal seems perfectly reasonable.

RESPONSE: We make this assumption.

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).

RESPONSE: Multi-species studies, outside the current scope, have the potential to further inform this debate. They are briefly mentioned at line 63 and in the expanded

Supplementary information.

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.

RESPONSE: The advantages and disadvantages are relevant when aiming for internal consistency over three decades. Challenges to achieving consistent site differences are much greater with the conventional NDIR analysers used over most of this period. This discussion is now focussed in the new Supplement. In relation to spatial differences, NDIR in situ analyses have had calibration limitations due to short lifetime of reference and calibration gases. Flasks analyses in a central laboratory have largely avoided these limitations over decadal timeframes.

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

RESPONSE: It is a brief reflection of the difficulties in maintaining government support for monitoring. It is best articulated in the original publication.

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?

RESPONSE: Yes, poorly worded. A CO2 partial pressure difference between hemispheres is a prerequisite for net IH exchange. See lines 120-1.

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

RESPONSE: The words used were “largely cancel”, which is the case. (It is supported in Figure 4 by the ïĄĎIHCO2 response to the 1997 GFED anomaly).

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

RESPONSE: The reasons are stated in lines 141-143: “the particular 3-month seasonal selection distinguishes periods of relatively stable partial pressure differences between hemispheres and the selected seasons also distinguish eddy and mean IH transport mechanisms (FF18)”

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?

RESPONSE: This sentence generally describes selection made by each laboratory prior to publication of the monthly averages used here. In the subsequent compositing this data clear statistical outliers are revealed of unknown origin. In the composite these are suppressed by averaging. See lines 115-118.

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).

RESPONSE: See response at the end of General Comments above. The wording at line 157 now merely states the widely accepted general reason for the NH CO2 seasonality.

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).

RESPONSE: The Table is now updated to 2017 (since SIO spo data became available). The numbers are now consistent, and the main text adjusted accordingly.

Line 190: Please clarify “generally attributed to NH forest...” this manuscript argues (in later sections) that 1.) the IH CO_2 gradient is dominated by transport and that 2.) the variability in the IH CO_2 is driven by the MLO time series. Is this consistent?

RESPONSE: The seasonality in mlo CO_2 is generally attributed to NH forests, and SH seasonality is small by comparison. However, the anomalies (mean seasonality subtracted) correspond primarily to anomalies in IH transport indices.

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

RESPONSE: See reply to 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?

RESPONSE: Spline polylines, generally available in commercial plotting software, link peaks and dips and serve only to visually aid discussion.

Line220: Please clarify: the assumption that the global atmospheric CO_2 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 CO_2 budget is the same no matter if CO_2 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?

RESPONSE: Yes. Wording in Section 5 is adjusted to make this clearer.

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]

RESPONSE: These issues are addressed in the response to General comments, above.

Line253: The qualitative study on the potential impact of CO_2 emissions on global and hemispheric CO_2 concentrations seems very instructive. However, a more qualitative estimate using a GCM could be beneficial here.

RESPONSE: Maybe, if the GCM has sufficient resolution and upper equatorial tropical parameterisation? It is outside the scope of this study.

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.

RESPONSE: This point is specifically addressed in the new introduction. CO_2 data quality (measurement and spatial representation) is considered a key issue in this paper, in which the composite plays a central role. The cgo-spo comparison is the best independent evidence for both factors. Note: The Southern Ocean uptake is informed by other records (including in ultra-high precision in situ monitoring at cgo and Macquarie Island plus other Antarctic sites). This is outside the scope of this paper and is the subject of on-going investigation by others (e.g. Stavert et al.). And we speculate that other factors such as seasonality in FF emissions may be involved, see line 315-322.

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

RESPONSE: Statistical uncertainties in trends, are now included throughout the paper. We also provide an estimate of uncertainty in the annual average data of Figure 9, Line 326.

Line 368: Here, the manuscript refers to "terrestrial emissions" is this equal to

F(ffco2)+NBP?

RESPONSE: Yes, text amended, Line 367.

Line 404: How would the IHCO2 impact the growth rate of CO2 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?

RESPONSE: Possibly, but spatial representation and quality of data are limiting factors.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2019-300/acp-2019-300-AC2-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-300, 2019.