

Interactive comment on “Variability in a four-network composite of atmospheric CO₂ differences between three primary baseline sites” by Roger J. Francey et al.

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Received and published: 26 August 2019

Anonymous Referee #1 Received and published: 3 July 2019 THE RESPONSES REFER TO LINE NUMBERS IN A REVISED DOCUMENT ATTACHED HERE AS A SUPPLEMENT.

The article by Francey et al describes and analyses 25-year composites of inter-hemispheric (IH) baseline CO₂ differences from NOAA, CSIRO and two independent SIO analysis. They show a good agreement between the 4 monitoring networks and explore the influence of atmospheric dynamics on the CO₂ IH gradient with a focus on El Niño periods. The results highlight the importance of IH CO₂ transfer parametriza-

C1

tion in global carbon cycle models. In general, the paper is scientifically sound and worthy of publication; however, the writing needs some modification and improvement. There is also several typos regarding the results hence the paper needs a careful editing/checking. We have modified, hopefully improved, and more carefully checked, the writing. After addressing the comments, the manuscript will be suitable for publication. General Comments:

General Comment 1. Currently the introduction sounds like this paper is submitted to AMT and not ACP, and it does not entirely sound like an introduction. For moments it is too technical and it is hard to understand the aim of the work. I recommend re-framing the introduction, to better attract the readers that are interested about this work. I would suggest to include an overview of the key findings of FF16 and FF18 (relevant to the work in this paper), and highlight better what this work aims to add to the previous findings. Parts of referring to results in FF16 and FF18 are indeed included later in the paper; however, the way/order they are represented is making things hard to link together and to see the big picture (and it makes the paper harder to read). Potentially the advantages/disadvantages in the Introduction could be added into a separate section (or combine with Section 2).

RESPONSE. A substantial rewrite of the Introduction is in response to these recommendations. Technical discussion is moved to Section 2 or addressed in an expanded Supplement. The Scientific ‘big picture’ should be clearer, and the different roles of FF16, FF18 and this paper are more clearly specified. The advantages and disadvantages are important when aiming for internal consistency over three decades. However, because of REF1 comments this discussion is now in the Supplement.

General Comment 2. Usually Introductions end with a paragraph summarizing the aim of the work. I had the feeling that this comes quite late at the end of Section 2. In addition to re-framing the Introduction I would move the last paragraph from Section 2 to the Introduction. This would also partially solve the missing key findings of FF16 and FF18.

C2

RESPONSE: The final paragraph of the introduction (Line 68) now defines the scope of the paper.

General Comment 3. At the moment the clarification/discussion of few things that can potentially lead to confusion for the readers are missing from the paper. It would be good to just summarize somewhere in the paper: 1) the important ENSO periods discussed in the analysis. The reason for this is that I kept thinking that the highlighted 2010 period in Figure 3 had potentially something to do with the strong 2011 La Niña event (although without reading FF16 first). 2) Why La Niña is not discussed at all and how it would impact IH (e.g., La Niña periods facilitates interhemispheric mixing of trace gases while El Niño inhibits interhemispheric exchange...). In Section 4 the authors wrote "Different responses of IH CO₂ to wind indices at different ENSO events, and from non-ENSO periods, are discussed in Section 6." and since ENSO includes both El Niño and La Niña, the impact of La Niña should be at least mentioned somewhere. These things are discussed in FF16 and FF18; however, it would be beneficial to add a sentence or two here also to be easier to track/understand the results.

RESPONSE: The concerns about La Niña are mentioned in the Abstract " (line 25)", in lines 63-66 of the Introduction and elsewhere the term ENSO is used to include both El Niño and La Niña events. The ONI in strong La Niña years are now included in Figure 9.

General Comment 4. Page 1 line 24 and elsewhere in the paper: "5-year relatively ENSO-quiet period" I assume the authors meant "5-year relatively El Niño-quiet period" since 2011 was a strong La Niña period, so ENSO-quiet period is misleading.

RESPONSE: Agreed, see point 3 for the revised approach

General Comment 5. Figure 4 - are there emission anomaly uncertainties that could be added to the Figure and included in the discussion in Section 4.1?

RESPONSE: To address this point, we have added a 16 DVM model composite, and

C3

seasonal Fossil data to Figure 4 to demonstrate their relative magnitudes. Because all surface-to-air fluxes appear very much smaller than required to significantly alter the baseline CO₂ differences, we have not dwelt on obtaining uncertainties from source data.

Specific Comments:

* Section 2 is quite lengthy and could be simplified. A careful reading to condense some of the text would be useful. RESPONSE: A new section 3 is introduced, and the sampling advantages/disadvantages moved to Supplement S3 in order to break up the lengthy section.

* Page 5 line 183: "e.g. 256 of 300 months have 4 networks" - It seems like the Table shows 268 instead of 256 for mlo-spo, or I misunderstood the Table in which case the authors need to give a better explanation of the Table. Moreover, for mlospo with both 268 or 256 the total number of months does not add up to 300. Also regarding the Table, what is the difference between empty boxes and the ones with '-'. If nothing then use consistent marking.

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.

* Page 7 line 228: "It is assumed here that flux variations from ocean sources are much smaller than terrestrial" - it would be good to have some references here. Also just mention why anthropogenic (fossil fuel) emissions are not compared here.

RESPONSE: This is now addressed in lines 216, lines 315-320, and Figure 8. The seasonal variation in Southern Ocean fluxes (an important part of the global ocean flux) are similar to terrestrial fluxes from the region but much smaller than NH terrestrial flux variation. High precision continuous CO₂ monitoring across the Southern Ocean (Stavert et al., 2019, and personal communication) confirm relatively small variation. Fossil emission seasonal variations are included line 322 and in the new Figure 4(d).

C4

* Page 9 line 334: It would be good to state why did the authors choose the ONI index as the ENSO index instead of the other indices. It would be also interesting to see if other ENSO indices show the same results (but not strictly necessary to include in this paper if it is too time consuming).

RESPONSE: We have re-examined the ONI data and they give essentially the same result as Nino3 and Nin3.4 indices, as stated in line 331.

*Page9 line337: "there are no significant ONI"-well relative to ElNiño but not relative to La Niña, so the statement is a little bit misleading.

RESPONSE: See reworded lines 3333-336.

Technical Comments:

*Page2 1st and 2nd paragraph: It feels like a weird jump between the two paragraphs, a 'linking' sentence between the two would be good.

RESPONSE: A linking sentence is provided, lines 33-36.

* Page 5 line 166, Page 8 line 308, Page 9 line 334: however -> ;however, * Page 6 line 200: & -> and * Page 8 line 271: FF18. These -> missing space before These * Figure 5: ITCZ -> Inter Tropical Convergence Zone, the abbreviation was not defined * Page 9 line 314: "see Discussion, Section 5" -> see Discussion, Section 6 maybe? * Page 9 line 339 ppm.(PgC) -> missing space before (PgC) * Page 10 line 258-359: why is this a separate paragraph? * northern hemisphere is somewhere capitalized and somewhere not in the text * NH abbreviation is not defined * Figure 3 inconsistency between figure and caption: (a) mlo-cgo, (b) mlo-spo -> (a) mlo-spo, (b) mlo-cgo * Figure 7 and 8 please add x axis label

RESPONSE: Thank you, these 10 items have been corrected

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2019-300/acp-2019-300-AC1->

C5

[supplement.pdf](#)

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

C6