

Interactive comment on “Unprecedented strength of Hadley circulation in 2015–2016 impacts on CO₂ interhemispheric difference” by Jorgen S. Frederiksen and Roger J. Francey

AC Chatterjee (Referee)

abhishek.chatterjee@nasa.gov

Received and published: 27 May 2018

This study examines the relative roles of the mean IH CO₂ transport (Hadley circulation) and the eddy transport (Pacific duct) in the gradient of CO₂ concentrations between Mauna Loa and Cape Grim. The authors look at the evolving relationship between the two transport indices and conclude that specifically for the 2015-2016 El Niño event, the record strength of the Hadley circulation contributed to the observed annual IH CO₂ difference. It is noteworthy to see a study connecting the large-scale dynamics to observed variations in atmospheric CO₂ concentrations. Its publication in ACP would be timely. However, I first recommend that the authors consider the

Printer-friendly version

Discussion paper



following issues and take steps to address them:

(1) The observed interannual variability in Cmlo-cgo has to be a function of both the transport variability and the underlying surface flux variability. This latter part, especially the role of terrestrial ecosystems during the boreal summer-autumn is largely ignored. For example, in Section 4 (Lines 22-24) the authors talk about the impact of fossil fuel emissions over NH but do not counteract that with the strong biospheric uptake that happen at the same time. I would recommend that the authors have a discussion at the outset on how they are considering surface flux variability in their analyses. In the current version of the manuscript, this is not clear at all.

(2) Since the authors examine the IH CO₂ annual difference from 1992 through to 2016, it is curious that the authors don't attempt to put their findings for the 2015-2016 El Niño in context of the 1997-1998 El Niño. Figure 6 indicates that the eddy transport may have placed a larger role in the IH CO₂ annual difference relative to the mean transport. This raises a bigger question - each El Niño has its own unique flavor, thus giving rise to its own individual teleconnection patterns (see Capotondi et al. [2015] BAMS, also available here - <http://ocean.eas.gatech.edu/manu/papers/PDFs/Capotondi-2015-Understanding-ENSO-Diversity.pdf>), it will be great to see a brief summary/discussion of how different El Niño flavors, and potentially a shift in El Niño type (EP – to – CP El Niños) may impact the two transport indices.

(3) The authors heavily rely on the information from the Francey and Frederiksen [2016] paper, especially in the discussion about the Atlantic duct (Section 3.1). The authors may want to include the relevant figure in this paper or introduce the necessary concepts here as well. Currently, it is challenging to put this paper in context without going back and reading the 2016 paper (which is what I had to do). For example, in Section 2, the authors talk about modeling that was done in FF16 – it is impossible to know what kind of modeling was done. It is possible to reduce the dependence on that paper by introducing the concepts about the Pacific, Atlantic duct early on and providing a short summary of the findings. In a lot of places, reference to FF16 is not necessary.

[Printer-friendly version](#)[Discussion paper](#)

(4) Overall quality of the text and figures: A couple of figures need to be improved, especially Figures 1 and 5. Either the figure resolution is low or it is too hard to read the figures. For the NASA movie, the authors may want to check the appropriate procedure to reference a video animation. The authors also need to provide the necessary credits to NASA Goddard Space Flight Center and the production team, including the URL for the movie (see <https://svs.gsfc.nasa.gov/12445>).

(5) Minor/technical comments: a) Abstract – Line 15-16 – incomplete line b) Section 3.2, Lines 3 - 6 – it is not clear how Figure 4 captures the convective transport of CO₂ emissions. Later the authors claim – “It demonstrates that when the Pacific duct is open there is also large-scale uplift slightly downstream of Asia, so . . .”. It is not clear how all this information is derived from Figure 4. c) Throughout the manuscript, the authors introduce the different transport indices in line (i.e., in the text). Given that this paper will be of significant interest to the carbon cycle community (and several of whom may not be familiar with these notations), it may be useful to have a Table that introduces the notation, what it means/represents and how it is calculated. d) The overall text requires some careful and thorough editing. Several sentences are hard to read either due to a lack of punctuation or overuse of conjunctive adverbs.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-203>, 2018.

[Printer-friendly version](#)[Discussion paper](#)