

Interactive comment on “The Orbiting Carbon Observatory-2: First 18 months of Science Data Products” by Annmarie Eldering et al.

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We appreciate the time that referee #1 invested in our paper. This comment contains detailed responses to their review. A manuscript with tracked changes will be attached in a later comment.

Response: We appreciate the detailed review from anonymous reviews #1, and the reviewer clearly understands our intentions with this paper – to document the mission and current status. We have made a number of edits to the paper based on the comments, which are detailed below.

Comments from anonymous reviewer #1 and author responses

Abstract: I recommend to add information on which version of the data products is

C1

presented (v7/v7r, Lite files) and which time period (September 2014 – January 2016?). I find this more important than launch date and the data when OCO-2 joined the A train (it is sufficient to mention the latter in the Introduction).

Response: we have made this revision to the abstract

Page 3, line 21 following: I recommend adding information on where (approx.) the shape is nearly rectangular (high latitudes?) and where it is very narrow (tropics?).

Response: we have modified the description in section 2 to make this more clear.

Page 7, line 17-18: Sentence “Enhanced XCO₂ coincident with biomass burning in the Amazon, central Africa, and Indonesian is also obvious in these figures”. Sorry, but this statement is not supported by the figures shown, which only display XCO₂ but no (independent) information on biomass burning. Please provide more evidence to support that statement or revise the statement.

Response: We have added a citation of Van Der Werf (2010) which clearly shows the location and seasonality of the biomass burning that we refer to.

Page 11, line 13: Statement “and this variability drives the standard deviation”. The standard deviation could also be driven by biases. How can you be sure that this is not the case? Please provide more evidence to support that statement.

Response: This was a useful comment – we looked in more detail at the variability of the retrievals, and added a statement clarifying that this could be due to the variability of sources, but land retrievals are more variable than the ocean glint, and that is another possible explanation of the standard deviation.

Page 11, Section 5.2: The difference of the two curves shown in Fig. 13 is 2-3 ppm which is a large difference for a CO₂ seasonal cycle. Please mention this and comment on this.

Response: Both reviewers had comments about this figure, focused on the ‘apples to

C2

orange' comparison of total columns and surface measurements. It would be significant additional scope to discuss the vertical distributions, averaging kernels, and how to properly compare the surface data to the total columns. Therefore, we have decided to limit this discussion to just OCO-2 data, showing a weekly timeseries and pointing out the standard deviation of the OCO-2 data relative to the changes in time. We have also improved the time axis on the graph.

Page 11, line 32: Sentence "The OCO-2 record adds additional detail to our understanding of the latitudinal gradients of XCO₂". Really? Please list explicitly what the new knowledge is or revise this statement. Figure 14 shows a large difference between southern hemisphere XCO₂ between the two time periods Sept-April (blue, yellow) and May-Aug (orange, red). Is this assumed to be a real feature?

Response: Based on your comments and the comments of another reviewer, we have decided to eliminate Figure 14 from the paper. You are correct in that more detailed are needed to explain what we have learned and what questions remain about the observed gradients, and we will leave that to later publications.

Page 12, line 33: Please add more info on the 1.5 ppm. Does this number correspond to bias-corrected and quality filtered single observations as contained in the v7/v7r Lite files?

Response: We have added a sentence stating explicitly what data is used in the Wunch et al analysis, where that figure is reported.

Page 28, Figure 12: Why only Mauna Loa for comparison? Would be interesting to also see comparison with NOAA data for other latitude bands (e.g., to see if the latitudinal dependence in October is consistent with NOAA or not). Please check figure title (I guess Jan means "Jan 2016 – Jan 2015" although the title suggests 2015 - 2014).

Response: We have updated the figure title and figure caption - you are correct, Jan is a 2016-2015 value, and the others are 2015-2014. NOAA ESRL only reports growth

C3

rates at Mauna Loa, and thus we have included only that data. There are other papers that have examined the growth rate from similar remote sensing data, and we now cite those in this paper.

Typos etc.:

Page 1, lines 10-13: The curly braces "{}" need to be removed, I think. Page 3, line 5: remove empty space after "4.3.1".

Response: This has been done.

Page 5, line 36: Check sentence (" . . . here, data . . . ") Page 29, Figure 13: Please improve x-axis numbering so that it is easier to see the beginning of each year.

Response: This has been done.

Pages 14 – 18: References: Please check all references carefully: Needs some harmonization, e.g., with respect to authors list: Currently it is (i) a mix of listing only one author, or several or all (sometimes even with ". . ."), and (ii) author's given name is typically abbreviated but not always, etc. Furthermore mix of "in preparation" and "(in prep)".

Response: This has been done.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-247, 2016.

C4