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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Computation and analysis of atmospheric carbon dioxide annual mean growth rates from satellite observations during 2003–2016" *by* Michael Buchwitz et al.

Anonymous Referee #2

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This manuscripts computes the CO2 growth rate from a combination of two near infrared satellite sensors over almost a decade and a half. The authors show that their estimated growth rates are in line with NOAA growth rates computed from marine boundary layer sites, and variations in the growth rate are correlated with expected mechanisms such as the ENSO cycle and anthropogenic emissions. This is all reasonable. However, I do not think that Atmospheric Chemistry and Physics is the correct journal for publishing this manuscript, because the manuscript does not present anything new about either the atmosphere or surface processes that influence the atmosphere (my comments on variation partitioning follow later). What I learned from this manuscript is that the merged XCO2 data product Obs4MIPs gives global CO2

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growth rates that are reasonable, in line with other estimates, and can be correlated with known factors influencing the carbon budget. This is a perfectly fine message, but it's primarily a message about the Obs4MIPs data product, and therefore a better venue for it would be an alternative measurement- or data-focused journal such as Atmospheric Measurement Techniques or Earth System Science Data. If the authors insist on publishing this in ACP and the editor agrees, I would strongly suggest making this a technical note instead of a research article.

Regardless of where this manuscript is published, there are a few issues that I would recommend the authors address, which are as follows:

(1) I fail to see the significance of splitting the growth rate into latitude bands. The authors must be well aware that such a split, while numerically possible, is impossible to tie to any set of surface processes because of atmospheric mixing, since the interhemispheric mixing time is a year or less. What was the authors' purpose behind deriving growth rates in zonal bands?

(2) While computing the global average XCO2, did the authors account for differing surface areas at different latitudes? There is less atmospheric mass at high latitudes, and unless this is taken into account, a straight-up averaging of gridded XCO2 globally is not going to give the correct mean CO2 mole fraction, which would invalidate its link with the global flux. It's not clear from the manuscript if the authors already took care of this (the NOAA estimate includes proper weighting by surface area [Ballantyne et al, 2012]).

(3) Every El Niño is different. Some cause large changes in ocean fluxes, while others cause large changes in land fluxes, which in turn can either be ecosystem-driven or fire-driven [Sarmiento et al, 2010]. The growth rate in global CO2 is a combination of all possible factors. To try and correlate this growth rate with an ocean-only indicator like ONI or SOI is a drastic oversimplification. To then use that correlation to infer the percentage variation in the growth rate due to ENSO (as opposed to fossil fuel emis-

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sions) is even less robust. If the authors really want to dig into the factors behind CO2 variability, I would suggest some index more strongly tied to the terrestrial biosphere, such as biomass-weighted precipitation or temperature anomalies, which in turn are influenced by ENSO.

(4) SCIAMACHY sensors were degraded a few years into flight, influencing the precision of retrieved XCH4 [Frankenberg et al, 2011]. Was a similar effect seen for retrieved XCO2? If so, why doesn't that should up as larger errors bars in figure 3(c) after 2006?

References:

[1] A. P. Ballantyne, C. B. Alden, J. B. Miller, P. P. Tans, and J. W. C. White, "Increase in observed net carbon dioxide uptake by land and oceans during the past 50 years," Nature, vol. 488, no. 7409, pp. 70–72, Aug. 2012. [2] C. Frankenberg, I. Aben, P. Bergamaschi, E. J. Dlugokencky, R. van Hees, S. Houweling, P. van der Meer, R. Snel, and P. Tol, "Global column-averaged methane mixing ratios from 2003 to 2009 as derived from SCIAMACHY: Trends and variability," J. Geophys. Res., vol. 116, no. D4, p. D04302, Feb. 2011. [3] J. L. Sarmiento, M. Gloor, N. Gruber, C. Beaulieu, A. R. Jacobson, S. E. M. Fletcher, S. Pacala, and K. Rodgers, "Trends and regional distributions of land and ocean carbon sinks," Biogeosciences, vol. 7, no. 8, pp. 2351–2367, 2010.

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