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Atmospheric Chemistry and Physics Discussions

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.

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We thank the referee for carefully reading our manuscript and for providing a critical review. Below we provide point-by-point answers to each of the referee's comments and concerns.

Addressing these comments, concerns and questions helped us to prepare a significantly improved version of our manuscript. General comments

C1: Referee:

This manuscripts computes the CO2 growth rate from a combination of two near in-

frared 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 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.

Author's reply:

We agree that the manuscript would also be appropriate for a measurement- or data-focused journal and in fact we carefully thought about this option before submission to ACP. We finally concluded that ACP is appropriate because the interpretation of the satellite-derived XCO2 data set is a focus of the manuscript.

We do not consider that our manuscript is simply a technical note. It is true that the data set and the presented analysis does not show obvious contradictions with current knowledge. However, the evidence base for current knowledge is limited to the sparse but accurate ground based measurements of the in situ mixing ratios. We present an independent data set of the dry column CO2 mixing ratio or mole fraction, XCO2, and the first derived annual mean growth CO2 rates using this XCO2 data set. The values are similar to those derived from the ground based in situ mixing ratio measurements.

The novel nature of our manuscript is that we present (i) a new global XCO2 data set covering more than a decade, (ii) a method to compute annual mean growth rates from this data set, (iii) a comparison with NOAA (de facto standard) growth rates, which agrees well and thereby validates both approaches and (iv) an interpretation of the derived growth rates to compare the impact of ENSO on the mean annual CO2 growth rate with that from fossil fuel combustion and industry.

The analysis of XCO2 and the derived annual growth rates can be enhanced and extended, e.g., by using appropriate and probably very comprehensive modelling and considering additional data sets. This would enable regional surface fluxes to be assessed. However, we consider this as outside of the scope of the current manuscript. Nevertheless, we consider our analysis as an important step in terms of interpreting the satellite-derived growth rates. It provides independent and global knowledge about the annual mean CO2 growth rate.

Our preferred option would be to publish this paper in ACP (as also supported by the other referee) but of course, it is up to the Editor to decide.

C2: Referee:

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?

Author's reply:

One motivation of the approach taken was to assess the quality of the satellite XCO2 data product and of the method developed to compute annual mean growth rates. We

are aware of the fact that atmospheric transport cannot be ignored and that transport and mixing will result in similar growth rates (compared to our uncertainty) for different latitude bands. We expect the latitudinal annual CO2 growth rates and the global CO2 annual growth rates to be very similar, which is what we find. Therefore, we write: "Growth rate time series for several latitude bands are shown in Fig. 4. As can be seen from Fig. 4, the growth rates are similar in all latitude bands including the global results (for numerical values see Tab. 2). The reason for this is that atmospheric CO2 is long-lived and therefore well-mixed."

In our manuscript, we have not aimed at interpreting regional growth rates in terms of regional changes. The only figure where we aim at interpretation in terms of emissions and ENSO is Fig. 5 and here we only use the derived global growth rate. Because we use a XCO2 data set that is spatially resolved, we think that it is important to compute and discuss growth rates determined not only from globally averaged XCO2 but also from regionally averaged XCO2. This is important as this tells us something about the quality of the satellite data set and of the method used to compute growth rates. If there is a good reason, why the growth rates should be similar for all regions (see above) and if the satellite data set would not show this, then this would indicate that the satellite data or the method usedf to compute growth rates from these data would suffer from a potentially serious problem. We therefore think that it is important to compute and discuss not only growth rates computed from the global data set but also from regional sub-sets. To make this clearer we will add the following text in the paragraph, where we discuss Fig. 4:

"As a result of atmospheric transport and mixing, similar mean annual CO2 growth rates, within their measurements error, are expected for all values derived at the different latitude bands. This behaviour is shown in Fig. 4 and is interpreted as an indication of the good quality of the satellite XCO2 data product and the adequacy of the method used to compute the annual mean CO2 growth rates."

C3: Referee:

(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]).

Author's reply:

For the revised version of the manuscript, we have improved the description of our method taking the referee's comment into account. Instead of unweighted averaging, we will compute monthly XCO2 values for global or regional averages by weighting with the latitude dependent area, i.e., by weighting with the cosine of latitude. To explain this we will add these sentences: "To compute the spatially averaged XCO2 time series (shown in Fig. 2a), we first longitudinally average the XCO2 followed by the computation of the area-weighted latitudinal average of XCO2 by using the cosine of latitude as weight. We consider surface area because surface fluxes are linked to mass of CO2 (or number of CO2 molecules) rather than molecular mixing ratios or mole fractions.". Our analysis shows that this leads to minor changes of most of the numbers, figures and tables presented in our initial manuscript but it will not affect any major conclusion.

C4: Referee:

(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 emissions) 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.

Author's reply:

We agree that the relationship between atmospheric CO2 growth rate variations and underlying source/sink related processes is a very complex one. We would like to contribute to a much better understanding of these links but we acknowledge that our manuscript is very limited in this respect. We have used ONI and SOI as proxies for ENSO and ENSO-related effects because these are well-established indices. Our objective was to compare the impact of ENSO on the annual mean growth rate as compared to that of the emission from fossil fuel combustion and industry. This goal we have achieved. More detailed analysis of the impact of the individual ENSO cycles on the biosphere and the land requires comparison with complex earth system models.

C5: Referee:

(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?

Author's reply:

SCIAMACHY XCH4 is retrieved from a different spectral region than XCO2. The spectral region beyond 1.6 microns in SCIAMACHY used Ge doped InGaAs detectors. These detectors were more sensitive to high energy proton bombardment. Individual detector pixels after being impacted by a high energy proton had increased noise. This effect indeed made the XCH4 error larger but the XCO2 data product is not impacted by this effect.

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