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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 the critical review. In the following, we provide 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:

The paper describes the analysis of column-average dry-air mole fractions of CO2 observed by SCIAMACHY and GOSAT. The data being analysed represent over a decade of substantial international efforts and is an amazing accomplishment that is documented in many previous papers. The headline figures from this paper look impressive but the subsequent analysis is weak and does not add much to the main paper. Below I substantiate these comments. I recommend the paper be published but only after the major issues are addressed.

Author's reply:

The primary objectives of the paper are (i) to present a new global data set, which has not yet been published in a peer-reviewed journal, (ii) to describe and apply a method to compute annual mean growth rates from this data set and (iii) to interpret the variations of the derived annual mean growth rates.

The analysis of the annual CO2 growth rates can be extended and enhanced e.g., by using appropriate and comprehensive modelling and considering additional data sets. However, we consider that our analysis is a relevant and important approach, which is independent of atmospheric model assumptions and uses available time series of data. Below we explain how we plan to address the major and minor comments as given in the review in order to improve the paper.

Major points

C2: Referee:

The authors will be acutely aware that it is difficult to compare NOAA ground-based data with XCO2 data from ground-based or space-based remote sensing instruments. Columns are an integrated sum of many geographically distributed sources and sinks from a range of times that have been distributed throughout the atmosphere. Consequently, it is difficult to compare NOAA and XCO2 CO2 growth rates. Here, I am suggesting only that the authors acknowledge this as a difficulty.

Author's reply:

Printer-friendly version

Discussion paper

We agree that annual growth rates of CO2 determined from the NOAA in situ ground based measurements, which are very accurate but sparsely distributed, will not necessarily be identical with the annual growth rates computed from measurements of the dry column atmospheric mixing ratios or mole fractions of CO2 measured over cloud free scenes from space.

Atmospheric CO2 has different sources and sinks. Changes in the biological sources of CO2, such as respiration and bacterially initiated decomposition and oxidation of organic matter, contribute significantly to changes of the CO2 growth rate. Furthermore, CO2 has a variety of geologic sources such as volcanic eruptions. Changes of the amount of CO2 from volcanic eruption may contribute to the mean annual CO2 growth rate. A small amount of CO2 is produced by the oxidation of CO initiated by OH. CO2 is removed from the atmosphere by the biosphere through photosynthesis on the land and in the ocean. This accounts for the removal of around half of the CO2 emitted each year. As is well known, CO2 is only significantly removed chemically or photochemically at high altitude from the atmosphere by the reactions of O(1D), its short wave UV photolysis and ion-molecule reactions. In the mesosphere and thermosphere, the column of CO2 is only a small component of the total column. For chemically long lived gases such as CO2 differences in atmospheric ratio at any point in the atmosphere depends on the time taken for CO2 to mix. After its release or removal, which takes place primarily at the surface or in the boundary layer, the air mass with elevated or depleted CO2 is transported by advection and convection and mixes into the atmosphere. This impacts on the horizontal and vertical distributions of CO2. The reduction of CO2 as a function of altitude enables the age of air mass to be estimated in the stratosphere, where vertical mixing is slow and varies in the range 2-8 years. In the troposphere mixing times are faster than exchange between the troposphere and the stratosphere. It therefore cannot be expected that annual mean growth rates computed from CO2 measurements at the surface are exactly identical with growth rates computed from XCO2.

Nevertheless, growth-rates from NOAA are the de facto standard and therefore we think that it is very important to show comparisons with this reference data set.

In order to better acknowledge this difficulty we will add the following sentence at the end of the paragraph, where the comparison with the NOAA growth rates is presented: “Perfect agreement is not to be expected as these two growth rate time series have been obtained from CO2 observations, which represent very different vertical sampling of the atmosphere (surface (NOAA) versus entire vertical column (satellite))”.

C3: Referee:

The global growth rates determined by XCO2 are I believe valid and physically meaningful. However, regional growth rates (no matter how you divide the Earth) make little or no sense because of atmospheric transport that moves air from one region (e.g. zonal band indicative of midlatitudes) to another. It is tempting to interpret regional growth rates, but they are (strictly speaking) scientifically meaningless without understanding changes in atmospheric transport. By (implicitly) ignoring atmospheric transport the authors are essentially assuming that observed regional CO2 variations results exclusively from that region.

Author’s reply:

We are aware of the fact that atmospheric transport cannot be ignored in this context. In our manuscript, we have not aimed at interpreting regional growth rates in terms of regional changes. In fact, we expect the growth rates to be not exactly identical but similar (taking into account the uncertainty of our growth rate) due to atmospheric transport and mixing. 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.” 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 rates. 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 derived growth rates especially if one assumes that growth rates are expected to be similar for the selected regions.

To make the above argument clearer in the manuscript, we will add the following sentences 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.”

C4: Referee:

The authors’ attempt at quantifying the respective role of human emissions and ENSO on CO2 growth rates is unfortunately (at least in this reviewer’s opinion) a fool’s errand. Our knowledge of human emissions is relatively good but still poor. Liu et al 2017 (Science) showed contrasting tropical carbon cycle responses in response to ENSO. These different responses will only complicate the correlative analysis of CO2 growth rate and ENSO indices.

Author’s reply:

Our approach to quantify the different roles is based on our new growth rate time series and well-established other time series. Our estimation method to quantify contributions from human emissions and ENSO is one attempt to address this aspect but we do not claim that our approach is the best possible. We think however that our approach is at least a reasonable and an important first step and we aimed at presenting our method as clearly as possible so that readers can judge to what extent they find the corresponding result useful or not.

We do not consider the task of trying to separate the impact of human emissions and that of ENSO on the mean annual CO2 growth rate is a fool's errand. Rather we consider our approach is an example of an Occam's razor i.e. in explaining a thing (here: the variation of the satellite-derived growth rate), no more assumptions should be made than are necessary. Nevertheless, we agree that our growth rates may contain more information than extracted using the method applied in our paper.

The interesting work of Liu et al 2017 (Science) uses a complex earth model, constrained by a limited number of satellite observations in the tropics and other a priori knowledge, to identify different responses in the different tropical continents to the surface flux of CO2 and thus carbon. Our approach to quantify the different roles of ENSO and anthropogenic fossil fuel emissions uses the reported time series of mean annual CO2 growth rates and well-established time series of ENSO indices and the known estimates of anthropogenic emissions from fossil fuel combustion and industry. This approach is our attempt to address what we and others consider an important issue viz: the attribution of growth rate variations to known anthropogenic emissions from fossil fuel combustion and industry and to that from the impact of ENSO. The latter has many potential impacts on the earth system amongst which are in the tropics the creation of regions of flooding and drought, increasing fire and biomass burning and changing sea surface temperature. These effects all impact on the growth rate of CO2 in different ways. However, in this study we have not tried to separate the different impacts of ENSO. Rather in this study, we attribute the importance of ENSO and the known anthropogenic fossil fuel combustion and industry sources to the observed annual growth rates. Our results are not in conflict with this scientific finding of Liu et al 2017 (Science). The use of our longer term time series of XCO2 provides an opportunity when coupled with models to investigate the regional impacts of ENSO both in the tropics and the extra tropics in a separate study.

Overall, we consider that our approach is relevant, reasonable and plausible. We describe our assumptions and the derivation of the attribution clearly so that readers can reproduce the results, criticise our assumptions and make improved analyses.

Minor points

C5: Referee:

Line 6. Geological processes are only a minor sink of CO2 over decadal scales. I applaud the authors being comprehensive but this reviewer suggests a focus on the timescales that correspond to the analysis being presented.

Author's reply:

For the revised version of the manuscript we will remove the link to geological processes in the introduction.

C6: Referee:

Line 10/11. Relating GtC/yr to ppm is an undergraduate exercise that barely needs a reference let alone two.

Author's reply:

We will remove one of the two references keeping only the reference to Ballantyne et al., 2012.

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