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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Signs of reduced biospheric activity with progressing global warming: evidence from long-term records of atmospheric CO_2 mixing ratios in Central-Eastern Europe" *by* Łukasz Chmura et al.

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Subject 1:

"I find the introduction unfortunately too poor in terms of framing the current study in the previous works evaluating trends in the seasonal cycle amplitude of CO2 in the Northern Hemisphere (Graven et al. 2013, Forkel et al, 2015, Piao et al. 2017, Yin et al. 2018, to name only a few)."

Response:

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Thank you for this comment. This deficiency of the original MS will be corrected in the revised text. In this regard, we refer also to our response to the comments of the reviewer #1 (subjects 1 and 3) and #2 (subject 3).

Subject 2:

"Why the authors analyse the amplitude in the seasonal cycle of CO2 mixing ratios (SCA) and what is the current debate on trends and drivers of SCA - why is central Europe a region of interest and why do they claim that central Europe is poorly represented (compared to other regions in the globe, it has much higher density of stations...)."

Response:

The European continent belongs to one of the most densely populated areas of the world and is heavily industrialized. Hence, large sources of anthropogenic carbon dioxide emissions are located over a relatively small area. European climatology is complex and certainly vulnerable to the influences of climate change. The carbon emissions in the region are well cataloged, so the European continent is a very good testing ground for carbon budget models.

Station density in Europe is indeed high, but only a few of them are located in the eastern part of the continent. It was our intention to underline the importance of these eastern regions, which generally receive less attention in European carbon budgets despite the fact that they cover a substantial portion of the entire continent. Kasprowy Wierch station fills this gap, as it is exposed to westerly circulation, but also on a number of occasions, is sampling easterly air masses (c.f. Fig 10).

The text will be revised accordingly.

Subject 3:

"What is the value of long term monitoring sites used in this study – what novel aspects are brought by this study."

Response:

We appreciate this comment.

Data record from measurement sites operating coherently for a long time is relevant for the analysis of long-term changes in the carbon budget in the same way as long records of surface air temperature might be useful for the detection of climate change. In our study, we intend to emphasize the evidence for long-term changes in the regional carbon budget of Central-Eastern Europe leading to changes in SCA contrasting those reported for higher northern latitudes (e.g. Piao et al. 2017, Yin et al., 2018).

The text will be revised accordingly

We also refer to our discussion of subject 8 in this document and to our response to reviewer #1, subject 3.

Quoted references are listed at the end of this document.

Subject 4:

“Likewise, the conclusions fail to set the current findings in contrast with the previous studies, especially since they are to some extent contradictory with some studies (e.g. Graven, Forkel).”

Response:

We thank the reviewer for pointing out the literature references.

The apparent contrast between conclusions of our study and previous findings quoted by the reviewer may stem from the fact that the aforementioned studies were focused on high-latitude areas, and their results might not be directly applicable to the European continent. We discuss this more broadly in the subject 8 of this document.

The text will be revised accordingly.

Subject 5:

“Lines 231-234 Why do the authors immediately conclude that the reduction in the winter peak is due to reduction in fossil fuel emissions only? Is it not possible that biospheric processes play a role? The authors could answer this question by using FF emission data and transporting the fluxes forward to evaluate the contribution of FF to the seasonal cycle amplitude of CO2 on this site. I would argue that the analysis does not settle the attribution to either anthropogenic or biospheric fluxes.”

Response:

We thank the reviewer for this comment.

The issues that are raised here are commented upon in the respective responses to reviewer 1 and 2. Not to repeat the argumentation, here we will give a short answer and provide references to more detailed discussions in the other responses.

We agree that the argumentation that we have used in the manuscript should be more comprehensive, specifically concerning the relation between anthropogenic and biogenic contributions during winter (see the answer to RC01, subjects 1-5 and RC02, subjects 1-3). This expansion will be a part of the revised manuscript.

We thank the reviewer for the suggestion about estimating the seasonal cycle amplitude. This is certainly a direction we would like to take in the near future. However, in the current manuscript we wanted to limit the reliance on the modelled results, as these often have uncertainties which are not easily quantifiable (see also reply to subject 8, and the discussion in the answer to RC2, subject 2).

Subject 6:

“The authors compare the results of site-level SCA with continental averaged CTE surface fluxes, which I do not think it correct. First, the authors do not define how the European continent is defined. Secondly, the authors should only compare the fluxes from CTE that are within the site’s footprint, which the authors then show in Figure 10 to be quite variable, and to not cover the full European continent. I think the appropriate method to attribute changes in SCA to FF or Biospheric fluxes would be to transport forward the fluxes from CTE in order to calculate the resulting concentrations at HUN and KAS."

Response:

We thank the reviewer for this suggestion.

A partial answer to the raised issue has already been given in subject 5 of this comment (RC03), but a more detailed discussion addressing it can be found in our responses to reviewers #1 (RC1, subject 6) and #2 (RC2, subject 2).

Adding to the discussion raised there, in their revised manuscript we will add a precise description together with the definition of the European continent according to TRANSCOM (together with reference to Baker et al., 2006).

Subject 7:

"Thirdly, the authors use only one dataset for FF emissions and one atmospheric inversion system. However, Gaubert et al. 2019 has shown that there is large disagreement in hemispheric fluxes between different inversions systems (and smaller regions should be even more difficult to constrain), and that a large fraction of the disagreement between inversions could be attributed to the FF emission data sets used. Therefore, it would be advisable to include more atmospheric inversion datasets to obtain an uncertainty range for surface fluxes. "

Response:

We thank the reviewer for making this point and for the reference. We agree that the results of the inversion systems need to be carefully discussed. We will expand the discussion and add the reference. We would also like to refer here to our comments to RC01 (subject 7) and RC02 (subject 2).

Subject 8:

“Finally, in Fig. 7 the authors compare apples and oranges: in-situ CO2 mixing ratios in ppm/yr with continental net biospheric exchange. By doing this, the authors assume that trends in [CO2] SCA are directly linked to net annual CO2 exchange, but trends in SCA could be found even if the net annual balance would not change, for example if increased uptake in summer would be offset by increased release in autumn and winter (see Piao et al. 2008 and Figure S9 in Bastos et al. 2019 ACP). Decreasing winter amplitude could also be explained by increased photosynthesis under warmer winters (which the authors indicate in Fig. 11). As mentioned above, the only way(s) to make the attribution to different processes would be to translate CO2 surface fluxes into concentration space using an atmospheric transport model, or else to invert CO2 concentrations into fluxes, and comparing the site footprints with CTE.”

Response:

We thank the reviewer for this remark.

For clarification, in Fig. 7a we are analyzing only the minimum (i.e. the departure from the trend line) of the annual concentration against the European net ecosystem exchange (NEE, estimated with CarbonTracker-EU), and we only interpret this minimum as being linked to NEE (and not the full SCA).

We agree that the relationship is not strong, which could stem from the mechanisms pointed out by the reviewer, however, on the annual basis, weaker uptake by the biosphere has to lead to less negative concentration in the summer. The mechanism is given by the reviewer as an example (i.e. that of increased summer uptake offset by increased release in autumn/winter) would indeed allow for constant annual NEE, but would also result in a stronger minimum of the summer CO2 mixing ratio, which would be visible in Fig. 7a. This, however, would have the opposite effect on the long-term trend line, which we do not observe.

Decreasing winter maximum (not ‘winter amplitude’), shown in Fig. 7b indeed might partially be a result of increased winter photosynthesis during winters, however, this

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effect is certainly superimposed on the reduction of fossil fuel emissions in Europe (see Fig. 6, Le Quere et al., 2018).

The relative influence of this photosynthetically increased uptake is extremely difficult to quantify, even using the state-of-the-art models. For example, in their work Piao et al. (2008) estimated that northern terrestrial ecosystems "may currently lose carbon dioxide in response to autumn warming". Such a mechanism would increase, rather than decrease, the winter CO2 peaks reported in our Fig. 7b. In turn, Yin et al. (2018) reported "negative to positive switch between fall/winter ΔNBP-ΔT, [which] challenged the 'warmer winter-larger carbon release' assumption", while "highlighting both the complexity of the carbon processes outside the peak growing season that requires further studies, and at the same time, the resultant uncertainty arises in our ability to project future carbon-climate feedback."

It has also to be noted that these studies, similar to Bastos et al. (2019), discuss SCANBP (seasonal amplitude of CO2 Net Biome Production), which can only be used to compare against seasonal cycle amplitude (SCA) at (i) larger spatial scales, and (ii) where the influence of regional fossil fuel emissions is negligible (as in high northern latitudes). As all the aforementioned studies were focused on high-latitude areas, their results might not be directly applicable to our study region in mid-latitude Europe.

The approach most similar to ours (with data including observations from mid-latitude Europe) was adopted by Piao et al. (2017), who reported that CO2 fertilization and climate change drove the increase in SCA for sites above 50 degrees N. However, at mid-latitude sites, land-use, oceanic and fossil-fuel fluxes, as well as trends in atmospheric transport "also contributed to the SCA trends". Flask measurements from Hegyhatsal (HUN) site were included in their analyses, but reported long-term statistical trends were smaller than reported here (0.011 ppm/year peak-to-through, -0.055 ppm/year through-to-peak), but statistically insignificant (Figure 1 in Piao et al. 2017). In the convention used by the authors, these would signify increasing annual SCA, albeit they deemed it to be statistically insignificant. The second site in Central Europe included in that study, Baltic Sea (BAL), located at 55°21'N latitude (KAS 49°14'N, approx. 700km north; HUN 46°57'N,), yielded an opposite trend, with -0.025 ppm/year peak-to-through and +0.063 ppm/year through-to-peak, indicating a decreasing trend in SCA, albeit also statistically insignificant. Neither of those trends was exactly captured by the models used in that study, which is not unexpected due to their lower resolution and overall focus on global trends

Unfortunately, while their results are quite valuable for the ongoing discussion on the critical issue on northern-latitude SCA, it is difficult to directly compare their results with ours, as (i) the scale of their study was global and not regional, (ii) only two flask sites used in their study was located in Central Europe, and (ii) the period of the analyzed dataset was different (1980-2012).

We certainly agree that the transport and inverse models could be helpful in distinguishing between various contributions to the observed SCA signals. However, given the still significant uncertainties associated with using those, we would be hesitant to treat the models as the only way forward. This is particularly true on the regional scales.

We will expand our discussion in the revised manuscript taking into account the literature stated above. Literature: at the end of the document.

Subject 9:

“The use of statistics. The authors overstate confidence in some results that are non-significant, e.g. Lines 240-243 "as well as the growing net CO2 flux of the continental biosphere" - which is 0.03 +- 0.03, and therefore non-significantly different than zero. On the other hand, when discussing trends in Mace Head the authors state that trends are not discernible (Lines 198-199), but the value is 0.05+-0.04, which could be considered significantly increasing.”

Response:

We thank the reviewer for raising this important issue, which we admit was treated too briefly in the original version of the article.

The values of the confidence level for the given ranges will be clearly indicated in the text. The discussion of the significance of individual parameters will be conducted more explicitly and systematically. We also consider adding the p-values in the text where necessary and to graphically mark the uncertainty of the fits on the charts.

Literature:

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