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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.

Anonymous Referee #3

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In their study, Chmura et al. analyse long term CO2 mixing ratio records in two central European stations (KAS and HUN) to evaluate trends in the seasonality of CO2 and of the net surface fluxes. They find a decreasing trend in the seasonal cycle amplitude of CO2 and propose that this can be explained by decreasing fossil fuel (FF) emissions in winter. I find that the manuscript in the present form suffers from several problems, including poor referencing and discussion and some methodological issues. I explain my points below.

I find the introduction unfortunately too poor in terms of framing the current study in

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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). The introduction focuses instead on crop studies (Schauberger) and the impact of extreme events. While the extreme summers are relevant for this study, I do not understand the choice to discuss crop productivity or the underestimation of summer impacts by land surface models (not used in this study). I recommend a full restructuring and appropriate referencing of the introduction to address clearly: - 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...) - what is the value of long term monitoring sites used in this study - what novel aspects are brought by this study - perhaps one sentence or two on key findings

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). Still, Penuelas et al., 2017 has pointed that a slow down in SCA at Barrow was observed, and this could be possibly due to increasingly negative impacts of extreme summers. Yin et al., 2018 also found a strengthening of the negative relationship of SCA with temperature.

There are in addition some points that I find problematic:

1) 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.

2) The comparison with CTE could in part address this issue, but unfortunately I find

several problems with the methodology. 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). This methodology has been used for example by Piao et al., 2017 to perform attribution in SCA changes from factorial simulations by land surface models. 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. 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.

3) 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-significanly 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.

4) The discussion of extremes is generally interesting, but I find a similar problem as with the comparison with CTE above. First, the authors present results for the climate anomalies in the whole European region. I do not think Figures 8 and 9 should be in the main text, but they can be provided in supplement. I would have found it more interesting to see the anomalies in T and water content from the sites' footprints for 2003 and 2010 to understand how representative they are, and whether one can see differences between stations because of somewhat different footprints.

The manuscript is well structured but I find the writing sloppy, with many grammar errors/inconsistencies which sometimes make it difficult to understand the message. Examples of sloppy/unclear writing include "an aggregated gridded spatial maps of area of influences", "annual air temperature and soil anomalies for summer and winter seasons", "periods of interests", "as functions of time". Moreover there are many examples where "the" is missing, and other small grammar inconsistencies are found.

Line 242: shouldn't the rate of FF emissions be -0.7 rather than 0.7?

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