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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.

Łukasz Chmura et al.

lukasz.chmura@fis.agh.edu.pl

Subject 1:

"The authors did not analyse all the potential contributions that can lead to changes in the seasonal cycle amplitude, they only discuss reduced photosynthesis for an increase in the summer minimum and reduced fossil emissions for a decrease in the winter maximum. They do not even mention all the other influencing factors such as changes in ecosystem respiration, changes in the growing season length, changes in

land use, CO2 fertilisation (surely plays a role over the 24 year period) other changes in fossil fuel emission than reduced emissions from heating due to warmer winters."

Response:

We fully agree with the reviewer. In fact, our discussion looks insufficient taking into account the complexity of the CO2 balance on the continental scale. Our aim was to simplify the processes but we admit that the reduction of detailed analysis might confuse the readers. We will rewrite the text with appropriate references to each part of the budget. As our work is directed toward interpretation of measurement data, we will try to justify the influence of the particular processes using databases information, isotopic analysis of CO2, in particular 14C information.

The doubts expressed in this part of the review coincide with the question asked by the first reviewer. The answer was fully included in the commentary to the first review, subjects 1, 3 and 5.

Subject 2:

"The author also do not sufficiently support their hypothesis with other data sources. They only use results from CarbonTracker, however, integrated over the Transcom Europe region, which covers the whole geographical Europe extending eastwards to the Ural. This is a much larger region than what the footprints of the two analysed stations cover. Both the biospheric fluxes as well as the fossil fuel emissions used in Carbon-Tracker are available per grid-cell and analysing those data for the footprint regions of the stations would be more meaningful. Additional data sources (e.g. fossil fuel emissions from EDGAR, biospheric fluxes from the Global Carbon Project) for analysing the changes in emissions in Central-Eastern Europe are available, for instance, at the ICOS Carbon Portal."

Response:

The issues raised by the reviewer are mostly addressed in the responses to RC1 (see

subjects 2, 6 and 16).

Adding to our comments available there, we would like to point out that our footprint analyses were focused only on the years 2003 and 2010 and did not cover the full study period, which we have also clarified following a different comment (c.f. subject 6 below). Also, in order to properly account for the area of influence, single-trajectory analysis is not sufficient. One would need to take into account the dispersion and mixing of the air parcels, e.g. by performing ensemble calculations. Another issue is that in order to perform proposed analysis, only the areas over which simulated air parcels were actually influenced by the surface fluxes (i.e. inside the planetary boundary layer) should be counted in the footprint estimation.

To overcome these issues, our model framework would have to be significantly expanded, and it is considered as a future study. However, in the current paper, we wanted to use observations as the main source of analysis and our intention was to use Hysplit results only as a support.

Subject 3:

"Another aspect that is not discussed at all in the manuscript is how their findings of a reduced seasonal cycle amplitude relates to previous publications reporting an increased seasonal cycle amplitude in the northern hemisphere, e.g. Graven et al., 2013, and Forkel et al., 2015. Since this manuscript is rather contradicting these previous results, the changes in the seasonal cycle amplitude at the two stations subject of this paper need to be set in context with the other studies."

Response:

In Graven et al. (2013) and Forkel et al. (2016), one can find that SCA of CO2 is rising in northern ecosystems since the 60's last century. This rising trend of CO2 SCA is much stronger in the case of Barrow station, Alaska (BRW, 71oN) then in case of Mauna Loa, Hawaii (MLO, 20oN), what was shown in Fig. 1. (Graven et al., 2013).

Forkel et al. (2016) point out that "arctic and boreal regions have experienced strong warming in recent decades and a "greening" trends have been detected from satellites, indicating enhanced plant growth". But lower latitudes do not show so rapidly growing trend in CO2 SCA which was presented in both publications by comparison data form BRW and MLO (Fig. 1 in Graven et al., 2013 and Forkel et al. 2016). What is more, when we look at the SCA from MLO station since 1990 (once again Fig. 1 in both publications), the trend seems to change from positive to negative. This issue was studied for example by Yin et al. (2018). They showed that the beginning of the 21st century indicates that the fertilization process is weakening. The correlation between CO2 seasonal cycle amplitude and the temperature became negative around the year 2000 at most northern stations. It seems to confirm a limit to the "warmer spring – bigger carbon sink" mechanism. This finding highlights a dynamic temperature sensitivity of the terrestrial ecosystem to climate warming and cautions the use of current carbon-climate response to constrain future projections. This issue will be elaborated in the revised version.

References:

Graven H. D., Keeling R. F., Piper S. C., Patra P. K., Stephens B. B., Wofsy S. C., Welp L. R., Sweeney C., Tans P. P., Kelley J. J., Daube B. C., Kort E. A., Santoni G. W., Bent J. D., Enhanced Seasonal Exchange of CO2 by Northern Ecosystems Since 1960, Science 06 Sep 2013: Vol. 341, Issue 6150, pp. 1085-1089, DOI: 10.1126/science.1239207

Forkel M., Carvalhais N., Rödenbeck C., Keeling R., Heimann M., Thonicke K., Zaehle S., Reichstein M., Enhanced seasonal CO2 exchange caused by amplified plant productivity in northern ecosystems, Science 12 Feb 2016: Vol. 351, Issue 6274, pp. 696-699, DOI: 10.1126/science.aac4971

Yin, Y., Ciais, P., Chevallier, F., Li, W., Bastos, A., Piao, S., et al. (2018). Changes in the response of the Northern Hemisphere carbon uptake to tempera-

ture over the last three decades. Geophysical Research Letters, 45, 4371–4380. https://doi.org/10.1029/2018GL077316

Subject 4:

"L34-35: It is not obvious why future climate predictions from numerical climate models rely on high-quality observations of atmospheric CO2 concentrations?"

Response:

Climate predictions relay on results from numerical climate models. The direct approach to model evaluation is to compare model output with observations and analyze the resulting difference. (Flato et al., 2013) Climate models results have the uncertainty resulting mostly from poorly defined parameters of parameterized physical processes. A typical solution is to correct uncertain model parameters by analyzing the agreement of the model with available observations based on historical data. (eg. Bellprat et al., 2012). Well calibrated models can be then used for climate predictions assuming that respective emission scenarios will be met. Additional references supporting the statement will be quoted in the revised version.

References:

Bellprat, S. Kotlarski, D. Lüthi, and C. Schär, 2012, Objective calibration of regional climate models. JGR 117, doi: doi:10.1029/2012JD018262

Flato, G., J. Marotzke, B. Abiodun, P. Braconnot, S.C. Chou, W. Collins, P. Cox, F. Driouech, S. Emori, V. Eyring, C. Forest, P. Gleckler, E. Guilyardi, C. Jakob, V. Kattsov, C. Reason and M. Rummukainen, 2013: Evaluation of Climate Models. In: Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, S.K. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex and P.M. Midgley (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA.

Subject 5:

"L 50ff: Why 'this' lack of representation, there hasn't been any mention of any lacks before."

Response:

The whole sentence has been changed as follows: "The lack of proper representation of Central-Eastern Europe in present GHG observation networks is partly compensated by Kasprowy Wierch greenhouse gas monitoring station. The station is located in the High Tatras mountain range of southern Poland, at the level of 1989 m a.s.l. This high elevation of the measuring point and lack of strong CO_2 sources in the direct vicinity of the station assure that the measured CO_2 signal is representative for Central-Eastern Europe background."

Subject 6:

"L 121-122: Do you mean by 'periods of interest' that you have calculated footprints over the whole 24 year period? I assume that this is the period of interest."

Response:

The sentence will be rephrased in the revised manuscript: "Assessment of the area of influence (footprints) for Kasprowy Wierch and Hegyhatsal stations has been carried out for the three month periods (June, July and August) in 2003 and 2010 with hourly resolution as described in the figure captions. Each frequency plot was created based on 2208 individual trajectories."

Subject 7:

"L 133: What do you mean by constant 5 cell x 5 cell weighting field?"

Response:

The description was expended to clarify the statement. It now reads:

"Following an approach previously used in Jeelani et al., 2018, aggregated gridded spatial maps of the area of influences were calculated at 0.5° x 0.5° resolution from individual back-trajectories, using the tools available in the Hysplit modeling suite. All trajectory points below 10 km altitude were included in the spatial gridding algorithm. Original footprint output was subsequently smoothed spatially using the focal function from the raster library in R software. Averaging was done using an equal-weight square matrix (5 x 5)."

Subject 8:

"L134-135: Please explain the approach and not only provide a reference, especially if it is only similar and not the same approach!"

Response:

The explanation of the footprint analysis methodology has been modified in the manuscript accordingly.

Subject 9:

"L137: How about consistency in datasets when using ERA-interim for climate extreme detection and NCEP for footprint analysis?"

Response:

NCEP data was chosen to drive the Hysplit model for several reasons: (i) it was readily available for both periods of interest (2003, 2010), (ii) it had low disk space require-ments and was already well established. While it is true that ERA-Interim (or even newer datasets like ERA5) represent the actual atmospheric state conditions better, we believe that the difference between calculated footprint areas would not be sig-nificant enough to warrant extra work necessary for the conversion of the data. At the same time, we recognize that the overall quality of the ERA-Interim dataset to be higher, which is why we used it in the - more critical to the discussion - analysis of the state of the soil during the analyzed period.

Subject 10:

"L 144: Why do you use only the uppermost soil layer? Is this the soil moisture layer which affects plant water stress? Plants usually have much deeper roots and access to soil water at deeper layers."

Response:

Thank you for pointing this out. Indeed, the 2m air temperature was used for the analysis. We modified the text to include the information as per your suggestion.

Following the comment by another reviewer, the calculation of soil humidity was also changed from using the first level only to the weighted arithmetic mean of soil humidity profile obtained by using four levels available in ERA-Interim data. The weighting function was scaled using the depth of soil layers. A detailed description was added in section 3.3.