Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2022-756-RC1, 2023

Comment on acp-2022-756

Anonymous Referee #1

Referee comment on "A view of the European carbon flux landscape through the lens of the ICOS atmospheric observation network" by Ida Storm et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-756-RC1, 2023

The manuscript 'A view of the European carbon flux landscape through the lens of the ICOS atmospheric observation network' by Storm and co-authors discusses and characterises the ability of the ICOS atmospheric monitoring network to representatively observe the European carbon budget. The study is based on atmospheric transport model simulations, land-cover maps and pre-calculated biospheric fluxes. The paper introduces a few new metrics that can be used for network design/characterisation and help with the identification of underrepresented land cover types and regions. The study is mostly based on established methods (transport simulations, flux calculations, emission inventories) and while some of the conclusions as to where the network is currently lacking additional sites seem obvious, the authors offer a more quantitative tool to support such assumptions. Language, structure and technical quality of the manuscript are mostly adequate for publication. There are a few aspects where the clarity of the manuscript could be improved and at least two major points which should be addressed to strengthen the conclusions of the paper. Once these and further minor comments are addressed the manuscript should be fit for publication.

Major comments

Comparison to real world data:

The study is solely based on bottom-up anthropogenic, oceanic and biogenic CO2 fluxes combined with atmospheric transport simulations. Table 1 presents mean model simulated contributions to CO2 mole fractions at the different sites of the ICOS network, but there is no validation of how well these mean mole fractions compare to observed mole fractions. Even more interesting would be an indication of how well the simulations fit the individual afternoon observations. Demonstrating the general ability of the model to capture the observed mole fractions for one or a few sites would strengthen the credibility in the model system and all following analysis and conclusions on the network. If such comparisons already exist for the presented model system, it would be sufficient to refer to these results. Otherwise, I would suggest to include a comparison for at least the site

that is already discussed as an example, Hyltemossa.

Respiration fluxes:

A major part of the discussion is focussed on the network characterisation with respect to summertime gross primary production (or here gross ecosystem exchange, GEE). However, ignoring respiration for the network characterisation seems like abandoning the study half-way. Respiration fluxes show a distinctly different temporal evolution as compared to GEE fluxes. Relative contributions from different land cover types may differ between respiration and GEE. Average footprints differ between summer and winter (as shown in Fig. 2). All these points indicate that conclusions drawn in terms of representatively monitoring GEE do not necessarily apply to respiration or NEE. This is already demonstrated by the flux contributions at the site Hyltemossa (Fig. 3), where contributions by land cover differ strongly between GEE and respiration as well as between summer and winter. If the aim of the ICOS atmospheric network is the observation of the complete carbon budget and not just the summertime CO2 uptake, the present analysis should integrate a discussion of respiration and/or NEE.

Minor comments

L12, L75: When introducing the footprints for the first time, it may be helpful to call them 'concentration footprints' to distinguish them from 'flux footprints' as used in the eddy covariance community.

L13: 'European flux landscape'. Not very specific. Better 'carbon flux' or even 'CO2 flux'.

L16: Same as previous: 'anthropogenic CO2 emissions'. In general, a note in the conclusions could be added that the results obtained here are exclusively valid for the CO2 flux landscape. Other parameters observed by the network (CH4, N2O) exhibit a completely different flux distribution and a similar analysis could be carried out in order to characterise their gaps and monitoring potentials.

Fig 1 (and others): Although the spatial reference system is noted in the legend, their are no axis labels which would allow for geographic referencing. Please add.
In caption, please mention source of land cover classes given in the figure.

L57: Be more specific. What kind of measurements?

L66: European climate zones stretch form sub-tropical (dry summers) through temperate (Central Europe) to sub-arctic. Please rephrase accordingly.

L69: Replace 'has more' by 'dominated by'.

L95: This their is no absolute measure of sensing capacity, I would argue that the metrics introduced here should be called 'more quantitative' instead of 'quantitative'. Or at least add that the introduced metric remains semi-objective as you could have used a slightly different metric instead.

L107f: Please refer to the 'Code availability' section here. I tried to follow the links and get the jupyter notebook to run. However, I got a 'page not found' error when trying the link for Storm 2022 in the list of references. Hence, I was not able to check whether there are working tools behind this or not. In the ideal case, getting your tools to run should not cost a whole day to set it up.

Sect. 2.1: In this section it does not become clear for which period the analysis was done. Later it is mentioned that the analysis was done for summer (winter?) 2020. How representative is a single summer? Was the summer 2020 rather typical are characterised by extremes?

L122: What was the spatial resolution of ECMWF data used?

Sect 2.2: Maybe change title so that it includes the description of the utilised fluxes, for example "CO2 fluxes and simulated signals at the stations"

L130: This is a rather old version of EDGAR. Why is it used instead of the more previous releases?

L135ff: Not quite clear how this was done? Was VPRM run twice (once with SYNMAP and once with HILDA land-cover) or simply on SYNMAP and the remaining land-cover analysis was done with HILDA only? How does resolution of these two land cover datasets relate to the output resolution of STILT? Where is SYNMAP data published? In a quick search I could only find the paper but no link to the data (as claimed in the data availability section).

L145: It could still be interesting if by choosing a lower inlet height one could focus the view on specific land types. However, I agree that by selecting the more convective afternoon situations, differences my not be too large as compared to the uppermost inlet height. Maybe something for future analysis.

L155: The 50 % are arbitrary. Why not use the complete footprint here to calculate the contributions from different land cover? How large would the difference be?

L159: Why take the maximum instead of the sum? In the real world multiple sites would receive information from the grid cell, which adds to the information available to any inverse modelling approach. By using the maximum here, a general underestimation of the total network sensitivity results.

L163: What is quantified here? Additional area covered? Differences in land cover contributions? Please clarify.

L168: Same question as above: Are VPRM fluxes based on HILDA?

L169: The abbreviations 'GEE' and 'NEE' are never properly introduced.

L173: Also see major comment above: It would still be interesting to see if results would be similar for a respiration view which would need to include a whole year of simulations. As you say respiration is similarly important for the carbon budget and the network hence should equally be representative for respiration. Even for GEE it would seem beneficial to include a whole growing season and not just the summer as uptake will differ in timing between different land-cover, especially crops versus forest, and climate region, early growing season in the South limited by water availability in the summer versus northern climate zones with late onset of growing season. Only focusing on summer may introduce a bias here as well.

L185ff: These equations need to be properly typeset and variables a to h defined as such. It is not clear over which dimensions the individual variables run (space, time, land cover) and over which of these the sums are defined.

L189: To me it is not really clear at this point why you need the footprints here. Is the area mean/total not simply defined by averaging/summing the proxy data over the area? Or do you additionally limit to the 50% footprint? Which I think would not make sense since you would limit the equal view already to what is within the direct view of the network. Or is the total simulated sensitivity redistributed equally to the target area?

L195: Why is this called a mask? Isn't it simply the fraction in each grid cell. Potentially reaching from 0 to 100 %?

L196: So e and f are obtained for each land cover type, correct? Please indicate this by a running index on those variables that are defined for different land cover. Supposedly c and d as well.

L207: Got me all confused here. f > e would give negative h according to equation 4.

Table 1: Some columns are given with 1 others with 2 significant digits. For example the 'Residential' contribution is 0.0, 0.1, or 0.2 everywhere. Difficult to make out differences from this. Should GEE not be given with a negative sign to indicate uptake? I don't think this is ever clearly spelled out anywhere.

L222f, L233f: That conclusion is too general since only GEE is looked at. The picture would, most likely, look very different if NEE would be evaluated. Please consider that the CO2-only observations cannot distinguish between anthropogenic, uptake and respiration. Hence, only concluding from GEE that the network is mostly sensitive to biogenic is not valid. The conclusion may still be correct for summer (but even that is not mentioned here), but you would need to show with a NEE view!

L255: These cites are not indicated in the figure. Please add for reference.

L261ff: Is this analysis based on land cover alone (not fluxes)? Is the land cover weighted by footprint sensitivity before calculating the shares or is just the area analysed? Would be nice to add two bars for the total to Fig 2b (the analysis by direction) as the text seems to be discussing the total rather than any specific direction.

L269f: In other other words this means: the anthropogenic signal is larger than the biospheric signal. Again, this is in contrary to what was claimed before for the whole ICOS network (focussing on the biosphere).

Fig. 4: All the labels are way to small. Also the shading for the footprints is not necessarily well chosen and does not offer many details on the actual distribution as it mainly shows the location of the sites. It would also be interesting to discuss some of the apparent differences between sites. Some stick out with very large sensitivity in the surroundings, others are hardly visible.

L287f, Fig. 4b: I don't see the big difference for coniferous forests. I see differences for grass/shrub and also for urban, but the forests seem to be represented rather well. You could use percentages in the text to underline your point.

Fig. 5: In b, do the upper and lower bars refer to current and extended network, respectively? Please add information to caption or plot.

L304f: Where do we see this? Is this in Fig 5?

L312f: Could you please add references to the figures where the individual points can be seen? 'underrepresented flux' in Fig 5? 'Monitoring potential for Serbia and Croatia' in Fig 6c,d?

Fig. 6: Are the same color scales used for the main plot and the German inset? Were these monitoring potentials for Europe and Germany calculated separately. Why would they look so different in the main plot and the inset? What are the actual values (not indicated on the color scale)? How should these be interpreted?
I think it would be easier to understand if you don't present the European and German case in one plot, but have two separate plots.

L322: Unclear. Do you mean that because of large local fluxes there is little additional signal for regions farther away? Even with the next sentence this is somewhat unclear.

L336: There is no table 3 in the manuscript. Only the single Table 1! Did you mean that? But how do I see relative contributions there?

Figure 8: Do the two different color shadings represent the same value range? Please clarify by either adding a second color palette or clarify in the caption.

L352: What does 'overlap' mean here? Is this with respect to the cropped 50 % footprints?

L360: 'relative underrepresentation' Does this refer to Fig 8b? Please add.

L362: Where do I see this? Compare 4b to 8b?

L365f: 'Figure 8 shows ... that great measurement potential ...' But Fig 8 does not show measurement potential, just footprint. Better refer to Fig. 6 b and d here.

L367f: Same as above: Where do I see this? Country contributions in Fig 8b versus 4b?

L385: Unclear: which scale do you refer to? How does a scale target a country? Scale of what? Please try to rephrase.

L410f: Please give references to the specific QND studies you are referring to.

L423: See above. Larger sensitivity to biogenic fluxes not proven.

Technical comments

L110 and elsewhere: 'Sect. 3.1' instead of just '3.1.