

Biogeosciences Discuss., author comment AC2
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Reply on RC2

Maiju Linkosalmi et al.

Author comment on "Tracking vegetation phenology of pristine northern boreal peatlands by combining digital photography with CO₂ flux and remote sensing data" by Maiju Linkosalmi et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-58-AC2>, 2022

Response to Referee #2

We thank the reviewer for assessing the manuscript carefully and for the constructive and supportive comments. We have addressed all the comments and questions that were raised (in italics), and the responses are listed below:

The current manuscript uses digital repeat photography for three peatland sites and compares this to other seasonally-variant observations, i.e. on precipitation, temperature, CO₂ fluxes, and Sentinel-2 reflectance. The manuscript was clearly written, was scientifically sound, and provides interesting analyses. Nonetheless, I have a few considerations that may help to improve the work. My main comments are:

1. Some of the methodological choices are not sufficiently justified and evaluated against other approaches. For example, L100 indicates that daily GCC averages were used. However, with similar input data many reference studies use the 90th percentile of GCC values over 3 days (e.g.: <https://doi.org/10.1016/j.agrformet.2011.09.009>) (SONnentag), which may help to reduce illumination effects and consequently smoothen the GCC profiles: why was this approach not followed here? Another question is why a 25% threshold for SOS/EOS was chosen (L166-167): although not an uncommon value, it would require justification. (see further issues in "specific comments")

Response: We have previously (Linkosalmi et al., 2016, <https://doi.org/10.5194/gi-5-417-2016>) used the daily GCC averages after determining the optimal time window for deriving the GCC from the images, based on an analysis of grey reference plates. Also, evaluation of the daily GCC values enables even the detection of rapid changes in the GCC values. This was further emphasized in the text. The 25% threshold is in accordance with Richardson et al. (2018) (<https://doi.org/10.1038/sdata.2018.28>). In addition, at peatland sites the probability of excess surface water after the snowmelt is high, and thus a relatively high threshold is preferable to reduce the effect of water surfaces. We added an explanation to the revised manuscript.

2. While the EC method is an important reference, it remains unclear how the EC footprint relates to the various ROIs and how it can thus effectively represent the variability of CO₂ exchange as caused by the different vegetation elements (and how this relates to the analysis provided here). We note that to better describe the phenology around

AmeriFlux/NEON tower footprints, recently a paper was submitted to Scientific Reports that uses 3-m resolution Planet data to extract 10x10 km phenology for each EC tower.

Response: It is clear that the EC measurements represent an area larger than an individual plant community area and that the vegetation varies within the flux footprint. However, we related the CO₂ fluxes to the general ROI (including many plant communities), not to the smaller ROIs defined for specific plant community types.

3. L339-342: "to our knowledge" may require some careful checking of literature also, although "with this precision" gives room for interpretation. It would be better if the authors could relate to other studies that also separate different elements in phenoCams. Examples exist (although "with this precision" may need to be clarified!): <https://doi.org/10.1016/j.rse.2020.112004> take different parts of the phenoCam image to look at grass/shrub/tree signals in a savanna. <https://doi.org/10.1016/j.agrformet.2014.08.007> divides the camera image into small subsets for which SOS is calculated. <https://doi.org/10.3389/fpls.2015.00110> looks at individual tree crowns in a single image. I would expect other studies to do this too. Perhaps this is not what authors mean, but I'd highly recommend to expand the "to our knowledge" to better clarify the innovation here and put it in perspective.

Response: We agree. The text was edited and references were added.

Specific comments:

- L32-41: the authors correctly indicate that a change of abiotic conditions (particularly warming) affects the C-balance due to increased take up of CO₂. While this is correct, warming in peatlands also causes high CO₂ emissions. While not a topic in this study, this aspect of the carbon balance could be highlighted here.

Response: The authors thank the referee for the suggestion and added a sentence about the effect of warming on the peat soil C losses.

- L86-87: could the authors also indicate the height on the pole where the camera was mounted? This is quite crucial information in my view. The reported angle is probably the depression angle?

Response: The angles and heights of the cameras were re-estimated and the text was updated to include this information.

- Figure 1: red lines and numbers on a green background are not very clear. Particularly also for 10% of male who are red/green colorblind. I suggest changing color and increase size of the numbers.

Response: The colour palette was changed to more a colourblind-friendly one and the font sizes were increased.

- L126: why was a base temperature of 5 degrees used here, and not 0 degrees for example? Could authors provide justification for this in the manuscript?

Response: In a sense, the choice of the base temperature is arbitrary. Here we followed the convention adopted by the Finnish Meteorological Institute (FMI). This explanation was added to the text.

- L129-131: I would request the authors to rewrite these two sentences: I could not

understand it. "Monthly average" of what and how can an average be divided in 3? What is the "value just before the increase"?

Response: The sentences were edited to improve clarity.

- L145-146: the minimum of two days is because there are overlapping orbits: this should be mentioned. Also I would like to read about how many cloud-free observations were available on average.

Response: The sentences were rephrased.

- L183-184: why not 15th, but 17th of June in Kaamanen?

Response: After checking the image directory again, we found an image from Kaamanen for 15 June 2015. The image was changed and the figure caption was corrected.

- Table 2: please indicate in caption why those data are missing. In addition, explain why some entries are in bold font. Possibly the highest/lowest numbers? But then by for Lompolojänkka there are two (different) bold values for Max GP week?

Response: The caption was edited and the bolded values were corrected to the table.

- L200: could somehow the significance of these differences be indicated in the table?

Response: The significance of these differences is presented in Table A2, which is referred to in the text.

- Figure 6: the figure now suggests that Lom for June > 10C is not significantly different from the others? Just to be sure that I interpret correctly, because the error bars suggest no overlap with the other two.

Response: The authors thank for the remark. There was a mistake in the figure, which was corrected.

- Table 4: I could not find a clear explanation for the low R2 of 2018a at Halssiaapa: or is this because of what is written in L260-261?

Response: The explanation was added to Section 3.3.

- L371-375: great that the authors manage to also use the GCC levels; this is probably because the StarDot is a stable camera, whereas for cheap cameras (such as in <https://doi.org/10.1016/j.jag.2020.102291>) this is less the case.

Response: The authors agree and thank for the comment.

- L394-396: this seems a relevant discussion. I suppose that the authors imply that for the vegetation that they study less of such non-photosynthetic biomass is present? In addition, the depression angle used by Vrieling et al (2018) is much smaller (i.e. less towards nadir) than in this study.

Response: The authors did not mean to take a stance on the amount of non-photosynthetic biomass, discussing instead the differences between camera and satellite data derived phenological phases. We rephrased the text for clarity.

- L401: please specify "typically" every 5 to 10 days is for Sentinel-2 in general without overlapping orbits, but not for non-cloudy satellite images.

Response: The sentence was rephrased to clarify the image availability.

- L405-409: in this framework the RS mapping with PlanetScope could also be mentioned; several efforts exist at present, and the satellite constellation offers very frequent imagery at fine spatial resolution (3m).

Response: The text was edited. We added a reference to PlanetScope.

- L419-420: this statement is a bit vague "more satellite data would be needed". The authors probably mean a finer temporal resolution resulting in more frequent cloud-free observations? Again, see also the previous comment.

Response: The sentence was improved for clarity.

- General: are the camera-data and/or GCC data somewhere available on a repository and/or part of a network like <https://phenocam.sr.unh.edu>?

Response: Unfortunately not at the moment

- Figure A2: WTD is missing for 2019? Please report why in caption.

Response: The explanation was added to the caption.

- Figure A3: GGDS: S is for "sum"? Add to caption for clarity.

Response: Thank you. We corrected the caption.

- Figure A5b: caption: I believe that only no temperature data in class <5 for August (July should be deleted here).

Response: "July" was deleted.

Edits:

- L40: "has been verified" is somewhat vague here: could authors be more specific on the findings of those studies?

Response: We added a brief description of these findings.

- L89: "in all cameras" should read "for all cameras"

Response: Corrected.

- L114: "on an" should read "at the"

Response: Corrected.

- L139: "filtered", but also "discarded" in the subsequent analysis?

Response: The sentence was edited.

- L351: remove "those" and replace "which" with "that"

Response: Corrected.