

Interactive comment on “Digital photography for assessing vegetation phenology in two contrasting northern ecosystems” by M. Linkosalmi et al.

Anonymous Referee #1

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This study presents two years of phenology and CO₂ flux data from two contrasting northern wetland and forest sites to explore the usefulness of digital repeat photography to monitor phenology and how phenological patterns relate to those of gross primary production. The research topic is timely and important and the study significantly contributes to our limited understanding of the role of phenology in the carbon balance of northern ecosystems. Overall, the manuscript is well written, the methodology is generally sound and the results are well presented and discussed. Nevertheless, I have some critical remarks that I would like to see addressed before the manuscript may be accepted for publication.

General comments:

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1) The authors spend much effort (Figures 1-4) in demonstrating what seems rather obvious: images obtained during low light levels are not suitable for extracting color indices. The illumination is reflected by the total digital numbers (DN) in the image and it is recommended to remove images below a certain total DN threshold (~50 -125 depending on camera model/sensor; check by plotting gcc vs total DN) prior to the image analysis. This filtering will naturally remove all images obtained during too low light conditions including night time, winter, and other low light periods. Moreover, all analysis of cloudiness, season and daytime, sun angle and global radiation effects presented in this study essentially repeatedly address the same issue of finding the critical illumination level that allows for extracting robust color indices. Applying a total DN threshold and using midday images only (see Sonnentag et al 2012, AFM) commonly removes variations in gcc due to the various effects on illumination changes. I suggest that the authors should considerably condense the discussion of this topic (one Figure could be enough; more could be shown in a supplementary part).

2) The use of multiple ROIs within the same image is a very interesting analysis. One concern however is that the distance, viewing angle and amount of background impacts for the different ROIs vary largely, specifically in the peatland (e.g. Menyanthes and Carex vs Andromeda and birch ROI). This has effects on the absolute magnitude of gcc. The authors recognize this issue (Page 7, Line 29-32) but do not take it sufficiently into account when interpreting the results of Fig 5. In addition, the conclusion stating the homogeneity of the pine canopy gcc severely suffers from subjectively choosing three rather similar canopy parts for this analysis. Including the contrasting areas just left or right from ROI B and C (canopy gaps) instead would have likely resulted in a different conclusion.

3) Since the authors state that ‘Especially dynamic vegetation models and simulations of C cycle could be improved by more accurate information on the timing of budburst and leaf senescence, as simple empirical parameterisations, typically based on degree-days’ (Page 2, Line 21-24), I think it would be valuable to include an analysis of

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how the growing degree day sum might affect phenology in this study and if the greenness data provides additional information on the phenology which cannot be captured by using the growing degree day sum used to describe phenology in current models.

Specific comments (Page and line numbers refer to the Discussion online pdf version):

Title: The title could be revised to clarify that this paper is not limited to evaluating the method of digital photography only but that it also explores the link between phenology and GPP

Abstract Line 23-24: I don't think that the 'short and pronounced growing season,' is the reason for the success of using digital repeat photography in this study. This method also would work for longer growing seasons given that the change in vegetation is strong enough to cause a signal in the image information. Overall I think the last sentence could be replaced by a stronger message highlighting the importance and implications of this study relating to the link between phenology and GPP in contrasting northern ecosystems.

Intro: P3, L11ff: One gets the impression that the hypotheses were written after the results were known, I suggest rewording these to more general but concise hypothesis based on the knowledge provided in the introduction.

Methods: P5, L13-19: The authors should motivate why they chose a different approach from the common flux partitioning approach. Actually, their method is also a partitioning approach (of day and nighttime fluxes to obtain GPP). How and why was the PPFD threshold of 600 chosen? How are fluxes during PPFD > 20 and < 600 defined then in this approach? The authors provide a reference for this method, but a short description should be still included here as well.

Conclusions: The conclusions (specifically the last two paragraphs) merely repeat results. The last sentence is in my view not the main interesting finding from this study (see also my related technical comment on the abstract below). Instead, I wish to

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see some more cognitive conclusions here and links to a broader picture related to phenology and ecosystem carbon cycling.

Technical corrections:

Abstract: Line 10-12. The first two sentences seem repetitive, I suggest merging them to one.

Line 14: replace 'are' by 'were'. Keep result in the past tense.

Line 15-16. The sentence does not appear logic, what does 'for which' refer to?

Line 18: consider replacing 'developed' by a more informative word, e.g. 'increased'

Line 20: replace 'flux data' with 'GPP' or 'GI' or similar defining the flux more precisely.

Line 20: replace 'temporary' with specific information, e.g. in autumn, in July etc

Line 20-21: What are the differences= This sentence is a bit vague, could be made more concrete, e.g. 'colder temperature coincided with lower GCC'.

Line 22: 'GCC was shown to respond to physiological changes on a daily time scale' – not clear. What physiological changes are meant here, leaf area changes? I doubt that any physiological change could be tracked on a daily scale? Or should it say over weekly time scales, i.e. over the course of several days?

Line 22: 'seems' sounds weak. Replace by a stronger phrasing, e.g. 'Our results suggest that' or similar.

Introduction:

Page 1, Line 30: Provide references for this sentence

P 2, L 2: I suggest deleting stomata conductance since it is directly linked to photosynthesis.

P 2, L 16: Reword 'camera monitoring', e.g. monitoring of vegetation changes using

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digital cameras'

P 2, L 26-27 : too many references bulked together; I suggest clarifying which reference supports which statement, i.e. place the relevant references directly behind i) the definition of GCC, ii) studies in forests and iii) peatlands.

P2, L28: replace 'temperate' by 'deciduous'

P2, L32: Sonnentag et al 2007 did not use digital repeat photography in their study.

P2, L32: 'Other types of peatland ecosystems have a more heterogeneous vegetation cover' – to which 'homogenous' peatland system are these 'other peatlands' compared with?

P3, L8-9: Replace 'in particular' with 'in addition'? The following is basically the third main objective.

P3, L11: With CO2 fluxes you mean GPP?

Methods

P3, L22; add unit for LAI value

P3, L29, Delete 'obviously' and link to previous sentence, avoid single sentence paragraphs.

P4, L1: reword sub-header to 'Camera set up'?

P4, L21: provide source information/reference for the FMUPROT software

P4, L 25: channel indices are the 'digital numbers' here.

P5, L21-26: add make and model of all sensors; clarify 'cloudiness data'.

Results:

P6, L2-5: This section 3.1 is redundant and could be removed or moved to the method section.

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Section 3.1.1 and 3.1.2, see main comment above; even under fully cloudy conditions there is still sufficient diffuse radiation to create enough illumination, especially in July. The comparison might have given different results for September when illumination reductions during cloudy conditions are further amplified by low sola angles.

P6, L20: these gcc values are meaningless to the reader without context of a common range in gcc values.

P6, L23: clarify 'lushest'

P6, L26: why was the window not centered around noon (10:-14)?

P7, L1ff: clarify 'these data'; provide standard errors for mean values

P7, L7 the subheader title 3.1.1 is vague, should it say e.g. 'Sensitivity of gcc to selection of ROI'? The first paragraph in each of the section 3.1.3.1 and 3.1.3.2as well as Figure 5, 7 and 8 should be moved into the method section

P7, L20; replace 'fastest' with 'earliest'

P7, L21: Reword this sentence. It is not the features who suggest this analysis but the authors thinking that it could be interesting to do. Also the link from ROI to flux data is not clear.

P7, L22-23. Motivate this decision

P8, L5: the spelling of this reference varies throughout the text; also don't use the terms wetland and peatland interchangeably, I suggest sticking to 'peatland' in this paper.

P8, L6: clarify 'started to grow' – based on an increase in GPP or LAI?

P8, L22: replace 'development' with 'GPP'?

P8, L17 and 25: Try to provide quantitative information instead of subjective terms such as 'very similar' and 'remarkably close'

P8, L26. Very interesting! Any speculations or data indicating why?

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P8, L30: add reference for this statement

P9, L5 add 'in peatlands and deciduous forests'

P9, L7: replace 'in our data' with 'in our pine forest'; would be good to follow up with a discussion why the correlation was so good in this forest compared to other coniferous forests?

P9, L13: 'there was a clear phase difference between GCC and GPI, the latter of which stayed at the maximum level until the end of August.' Interesting, discuss why.

Conclusions

P9, L25: 'The feasibility of digital repeat photography for assessing vegetation phenology was examined'. Actually, this study did not validate the feasibility of gcc to describe vegetation phenology. It merely shows that gcc shows a seasonal pattern, however, how comparison to LAI or similar data was conducted to be able to make this statement that gcc described vegetation phenology.

P9, L29: examined the 'stability of the digital camera system' sound odd, rephrase e.g. 'the robustness of gcc'

P10, L12 and P8, L13: How is it possible that the gcc temporarily decreases during a cold spell? This implies a reduction in leaf area or chlorophyll? Can the authors explain this further?

Figures

Fig.2: could be merged with Fig1. It says 'shortwave' radiation in the y label but global radiation' in the caption, be consistent. Also in Fig 3 then global radiation is used again.

Clarify the (+/-) refers to the error bars displayed in the Figures

Define LWT

Fig 5b is redundant, the ROI could be included in Fig 5a. What are the green and white

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ROI?

Fig.6 is nice! Great to see the earlier greening of the birch, even though absolute magnitudes are likely affected by different viewing angles. Grey 'Winter' symbols continue the 'Wetland symbols in the winter'? Clarify.

Fig 9: Seasonal patterns are the same for crown and canopy, but absolute values shifted due to different viewing angles

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