

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

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We would like to thank Referee #1 for his/her careful reading and constructive comments, which helped us to improve the manuscript. Below our response (AR) to each referee comment (RC).

RC: 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

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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.

RC: 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 recommend to remove images below a certain total DN threshold (åĹij50 -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).

AR: As a response to this comment, the material dealing with the reference plate data (Figures 1-3 and corresponding text) were moved to the Supplement section. The figures and discussion related to cloudiness and sun angle were kept in the main text, as they concern not only radiation levels but also possible differences in GCC between different sun angles and differences between direct or diffuse radiation.

RC: 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.

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AR: We agree and thus added text to emphasize the problems in comparing GCCs of different ROIs: "While the seasonal patterns of the GCCs of different ROIs can be compared, the same may not be true for the absolute GCC values, which were affected by different viewing angles and distances of the target. To gain a better insight into the quantitative differences between different ROIs, these ROI-specific GCC data should be investigated in conjunction with direct vegetation analysis (LAI, biomass) and small-scale (chamber-based) CO₂ exchange measurements."

RC: 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.

AR: It is true that selecting an area with more bare ground in ROI would give a different GCC. However, our approach was to deliberately choose similar ROIs, thus avoiding major canopy gaps. This made it possible to assess if the GCC of the canopy, which we are focussing on here, is sensitive to such a subjective selection process.

RC: 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 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.

AR: This citation is from the introduction and is provided as a general motivation for phenology monitoring with digital repeat photography. It is based on three papers referred to in the text, so it does not represent a conclusion of the present authors. While sounding an interesting topic, such an analysis would not be within the scope of

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this manuscript.

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

RC: 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

AR: The title was changed to: "Digital photography for assessing the link between vegetation phenology and CO₂ exchange in two contrasting northern ecosystems"

RC: 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.

AR: Expression "short and pronounced growing season" was omitted.

RC: 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.

AR: Last two paragraphs of Introduction were changed to state the objectives of the study more clearly. The hypotheses were removed as unnecessary.

RC: 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 PPF threshold of 600 chosen? How are fluxes during PPF > 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.

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AR: The partitioning method used in the manuscript was selected as it provides robust estimates of the maximal photosynthetic rate with a limited set of data. It scales well with the results obtained from traditional methods despite the fact that it does not take into account the day-night variation in respiration. The PPFD limit of 600 micromol m⁻² s⁻¹ was chosen as a compromise of obtaining enough data for the daytime flux average and being well above the saturation point of the radiation response curve (i.e. the CO₂ fluxes are rather constant when radiation is higher than 600 micromol m⁻² s⁻¹). The fluxes corresponding to the PPFD range of 20-600 micromol m⁻² s⁻¹ are not needed in this analysis as we are only interested in the difference between the daytime NEE and the night-time NEE, i.e. the potential gross photosynthesis in optimal light conditions during each DOY.

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

AR: This section was largely rewritten and more general conclusions added.

Technical corrections:

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

AR: Text changed to remove repetition.

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

AR: Tense corrected throughout the text.

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

AR: Sentence rephrased for clarity.

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

AR: Sentence rephrased for clarity.

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

AR: Replaced by 'CO₂ exchange'.

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

AR: Text changed to specify the period.

RC: 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'.

AR: Text changed as suggested.

RC: 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?

AR: Text changed: "GCC was shown to respond to environmental changes on a time scale of days."

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

AR: Text changed as suggested.

Introduction:

RC: Page 1, Line 30: Provide references for this sentence

AR: Reference added.

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

AR: Deleted as suggested.

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

AR: Text changed as suggested.

RC: 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.

AR: References divided to forests and peatlands.

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

AR: Text changed as suggested.

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

AR: Reference removed.

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

AR: Text rephrased for clarity.

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

AR: "In particular" removed. The objectives clarified.

RC: P3, L11: With CO2 fluxes you mean GPP?

AR: This paragraph was removed.

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Methods

RC: P3, L22; add unit for LAI value

AR: Unit added.

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

AR: "Obviously" deleted. (The paragraph consists of two sentences that refer to two previous paragraphs, so could not be merged.)

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

AR: Header changed as suggested.

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

AR: Reference added to a user manual available on the web.

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

AR: "indices" changed to "digital numbers"

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

AR: "cloudiness" changed to "fractional cloud cover". Sensors specified for the data actually used.

Results:

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

AR: Removed

RC: 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

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July. The comparison might have given different results for September when illumination reductions during cloudy conditions are further amplified by low solar angles.

AR: Section 3.1.1 (and Figures 1-3) was moved to the Supplement section. Section 3.1.2 was kept in the main text, as explained above.

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

AR: The GCC differences refer to Figure 5 which gives the context (reference to this figure was added to text).

RC: P6, L23: clarify 'lushest'

AR: Clarified: "...July represents the peak growing season (for both radiation levels and LAI...").

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

AR: In fact, a wrong time window (summer rather than winter time) was indicated. The correct time window is 10.00-14.00 local winter time. This has been corrected throughout the text.

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

AR: Clarified by specifying the date. The standard deviations of the hourly mean values are shown in Figure 4.

RC: P7, L7 the subheader title 3.1.1 is vague, should it say e.g. 'Sensitivity of gcc to selection of ROI'?

AR: Changed as suggested.

RC: 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

AR: Text and figures moved as suggested.

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

AR: Changed as suggested.

RC: 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.

AR: Text modified to address the comment.

RC: P7, L22-23. Motivate this decision

AR: Text modified to better motivate the decision

RC: P8, L5: the spelling of this reference varies throughout the text;

AR: Spelling corrected throughout the text.

RC: also don't use the terms wetland and peatland interchangeably, I suggest sticking to 'peatland' in this paper.

AR: "Peatland" is only used in Introduction where appropriate. "Wetland" is mainly used when referring to one of the measurement sites.

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

AR: Modified: "...started to assimilate CO₂".

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

AR: Changed as suggested.

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

AR: "Very similar" was quantified by adding the value in question. "Remarkably close" is used to describe the match during the spring. The match is sufficiently quantified by the correlation coefficients for the whole snow-free period provided in the previous

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sentence.

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

AR: We included a brief discussion of the phase difference in the forest subsection, but cannot explain the similar difference observed for the wetland.

RC: P8, L30: add reference for this statement

AR: Reference added.

RC: P9, L5 add 'in peatlands and deciduous forests'

AR: Changed as suggested.

RC: 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?

AR: Replaced

RC: 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.

AR: See above (P8, L26).

Conclusions

RC: 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.

AR: Rephrased.

RC: P9, L29: examined the 'stability of the digital camera system' sound odd, rephrase

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e.g. 'the robustness of gcc'

AR: Rephrased.

RC: 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?

AR: We do not believe that reduction in leaf area or chlorophyll concentration explain this reduction. We added a brief speculation of the possibility of measurement problems.

Figures

RC: 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.

AR: Changed all to "shortwave radiation".

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

AR: Clarified as suggested.

RC: Define LWT

AR: Defined in figure captions

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

AR: Fig 5b removed

RC: 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.

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AR: The grey circles indicate the wintertime data that are influenced by an insufficient light level. All time series are marked grey during the wintertime to emphasize the period which is not usable.

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

AR: Comment added to the text.

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