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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Interpreting canopy development and physiology using the EUROPhen camera network at flux sites" *by* L. Wingate et al.

L. Wingate et al.

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Overall I think this is a well described and comprehensive study that provides a valuable contribution to the phenology- and flux-related literature. The main point I'd raise is that the manuscript could be improved with a more detailed description of the radiative transfer modelling, as well as a fuller explanation of the function linking the reflectance that is output from the PROSAIL model and the DN values derived from the camera images (i.e. equation 2). This will aid the interpretation of results in Section 3.2.

Response: We thank referee #1 for their positive and constructive comments. Both yourself and referee #2 presented a strong case for providing more details on the model and the function that relates the reflectance outputs of PROSAIL to the DN values extracted from digital images. In order to make the modelling component of this paper more transparent and to encourage the testing of the model at other sites we created a git repository for the documented code and included the data contained in Figure 12 on a Bitbucket account (https://bitbucket.org/jerome_ogee/webcam_network_paper). Should the paper be accepted for publication in Biogeosciences, this code and dataset will be open to public access. Our hope is that by placing it in a git repository the research community will actively contribute to the improvement of the code and tools to assess its sensitivity. We will place the link information for this git repository in the methods section 2.3 of the paper and again in the legend of Fig. 12.

In addition, the assumptions behind this type of RT model should be discussed.

Response: The RT model used here, SAIL, assumes that diffusers are randomly distributed in space (turbid medium assumption). We completely agree that such assumption will not be applicable for very clumped canopies such as sparse forests or crops (e.g. vineyards, orchards…). Also the RT model assumes only one type of foliage, and therefore cannot deal with species mixtures. For mixed forests, PROSAIL can still be used to interpret RGB signals if the ROI used on the images is dominated by one single species (e.g. 90% Oak cover for the forest simulated in Fig. 12). These caveats have now been raised in the revised version of the manuscript.

Finally, the use or possible applications of these data and the modeling framework could be expanded upon in the discussion or conclusions. Perhaps it is worth mentioning here what complementary information these data will bring to phenology metrics derived from satellite data. For example the data are available smaller scale at the location of flux towers which opens up the opportunity to understand differences between growing season length and carbon uptake period, in addition to linking the trends in phenology with net C fluxes in order to determine the impacts of greening on the balance of C uptake and respiration. Or this could be added in the discussion.

Response: We thank the referee for pointing out the advantages of seasonally resolved camera data for interpreting fluxes and differences between GSL and the CUP. We have

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added a paragraph covering these topics in the conclusion.

P8 Lines 14-16: Please can you describe why this is necessary in a bit more detail – for readers who are not familiar with more technical aspects of cameras?

Response: We are not sure we fully understand this comment or more exactly what parts of the manuscript it refers to. Our statement on page 8 lines 14-16 is that it is necessary to have the same ROI between images and we clearly explain why by saying it is otherwise problematic (for seasonal characterization) and impractical (would require to check the ROI on every single image). Maybe the comment refers to another page as the next comment referenced to a page below (page 9 line 18) clearly refers to page 8 line 18. . .

Also are the same lenses used on each camera? Are the aperture size, shutter speed, ISO and sensor gain kept constant? What is the difference in sensor size between the two types of cameras?

Response: Unfortunately the network currently uses different types of cameras, and manufacturers do not always give detailed information on the optics, the sensors or the image processing of the cameras. As part of the ICOS infrastructure, a protocol has been proposed to homogenise as much as possible the settings of these different cameras and is briefly described in section 2.1 of the manuscript. Also, based on the results shown in Fig. 12 and S7 (now S8) we believe that, as long as we are interested in colour fractions over large ROIs, details regarding the physics and signal processing of the camera (sensor size, aperture, shutter speed. . .) can all be accounted through the spectral response of the camera (GRBG(λ)), the color balance factor (BRGB) and the UV and IR cut-off wavelengths defined in equation 2.

P 9 Line 18: For the calculation of the mean color fraction is the mean value of ncolor, nred etc is calculated for all pixels in the ROI, and then the Color Fraction is calculated? Or is the color fraction calculated per pixel and then the mean taken?

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Response: In our analysis the mean values for nred, ngreen & nblue were calculated over all pixels in the ROI and the colour fraction was computed from these mean DN values.

Did you investigate what the breakpoint-derived dates might correspond to in the images? For example, the first breakpoint may be 30% leaf out, instead of 50%? Does it correspond to a particular change in photosynthetic parameters, or C fluxes?

Response: In some cases where information was available yes. From Figure 3 we see that the first breakpoint is usually detected before 50% of the ROI contains green leaves (our criteria for leaf out detection). The hardest breakpoints to investigate were those of the evergreen needle-leaf canopies for example in Hyytiala (Fig. 5). We described in the manuscript how these were often linked directly to the appearance and disappearance of snow and indirectly to changes in temperature. In the case of grasslands and evergreen broadleaves, many of these breakpoints were linked to management practices such as mowing (eg. Neustift and Fruebuel), appearance and disappearance of snow and importantly flowering events. In the deciduous broadleaf forests the breakpoints nearly always picked out leaf on and leaf off, but in addition the often detected the maximum and minimum dates of the 'spring hump'. As discussed in the paper and after simulations with the model we see that these breakpoints are linked strongly to the relationships between pigment concentrations shown in Fig 11. Thus these extra breakpoints in deciduous ecosystems are providing indicative dates of how quickly the photosynthetic apparatus is being assembled and when it reaches its photosynthetic maximum.

It may also be interesting to investigate if there was as consistent a (or similar) bias if: a) A different breakpoint was analysed for each phenophase (for example using the 2nd breakpoint for leaf out). b) A different automatic detection method was used, such as the DOY at half maximum which could be site specific and therefore might account for the issue of different camera set-ups?

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Response: We fully agree that further analysis of these additional breakpoints may vary consistently with the leaf on dates and coincide with key parameters derived from other automatic detection methods. However, for the time being consistent information about key phonological phases is not available at all the sites, hampering a comprehensive analysis as the one suggested by the referee. This is certainly a study that should be conducted in the future.

4) Also for sites where more than these 3 breakpoints were detected, did you investigate what the other breakpoints might correspond to?

Response: Yes when possible (see our response two comments above).

Do you think the same procedure should be used for all vegetation/canopy types?

Response: I think it really depends on the question to be tackled within a particular study and the ecosystem under investigation. For systems with management and ecological studies interested in all the phenophases displayed by canopies including flowering and fruiting, I believe this type of breakpoint approach could be very useful. For studies that want to study snow dynamics and vegetation responses this again could also be an attractive choice for example in conifer sites or in semi-arid grasslands where phenology is regulated by rain pulses. In most canopy phenology studies or remote sensing studies, the start, 50% max and end dates are often the most important dates that need to be resolved and I believe this approach may be as good as others on deciduous ecosystems and non-managed grasslands to obtain such dates. In our case, using this approach identified significant breakpoint changes in the green signal during spring that were consistently found at all the deciduous forests studied. This was part of the motivation to understand the emergent properties of the green fraction time-series during spring, explain it mechanistically and understand better how it related to the peak in GPP.

One final point here: perhaps this validation could be added as a section to the beginning of the results, given it is an analysis, rather than a description, of the method

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used?

Response: We agree that figure 3 shows results from an analysis but it is needed here to show the success of our method to automatically detect break points in the RGB signals. In that sense we felt it should rather appear in the methods section, allowing the results section to focus on more ecological aspects.

P 12 Line 10: How is the spectral efficiency defined? Is it just the spectral response curve that is used? A fuller explanation of this and the derivation of the BRGB parameter would help to explain the function (here I will call it an observation operator or OO for the sake of brevity) define in Equation 2. It would be valuable to detail the issues with the camera specifications such as the response functions and how they impact the derivation of GRGB. Also, an initial sentence explaining that you need this type of function to match the DN values measured by the camera sensor to the reflectance simulated by the PROSAIL (taking into account the camera/set-up specific characteristics) model may help readers that are not familiar with this topic. Are any of the camera sensor characteristics not taken into account? And if so are these mostly lumped into the BRGB parameter?

Response: We have added a sentence to the manuscript as suggested by the referee to clarify the link between the camera and PROSAIL outputs. In addition we have provided the derivation in the documented model code and placed it in a git repository (https://bitbucket.org/jerome_ogee/webcam_network_paper).

5) P12 Line 21: Perhaps for the general reader it may help to detail that the SAIL part of the model essentially scales from leaf to canopy. A bit more information on what type of RT model it is (i.e. turbid medium) and the assumptions that are made about the canopy structure for this model.

Response: See our response to the second comment of referee 1.

6)It would be good to explain why this type of RT model was used and not another (e.g.

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a Geometric-Optic model, or a multi-layer or 3D RT model).

Response: Our choice of the RT model was driven by simplicity and efficiency. More complex RT models such as ray tracing and 3D models require a very heavy parameterisation that would be un-applicable within a network covering so many different ecosystem types. It would also result in an over-pararameterisation of the system with too many degrees of freedom to address ecological questions.

Section 3.1.1 How might the automatic detection of phenophases be improved for coniferous sites do you think? Might it be improved if all the color fraction time series were included in the analysis?

Response: Detecting the phenophases of conifers is a real challenge as they do not produce very large shifts in any of the colour fraction signals that can be easily related to phenology. However, we have not done any breakpoint analysis on these other colour signals but we intend to systematically implement such analysis in the phenopix package (http://r-forge.r-project.org/projects/phenopix/) in the future.

By visual comparison do the breakpoints detected for evergreen sites correspond to any obvious phenophases? Is the leaf flush and browing that you describe on P 15 Line 19 onward evident when you examine the images, or are the dates of this leaf flush etc (i.e. arrows in Figure S4) for each particular site/year known from field measurements?

Response: In the case of the Hyytiala conifer site shown in Figure S4, the shoot elongation and needle flushing episodes are visually identified in the photos but are not detected by the breakpoint approach (i.e. 5 breakpoints maximum).

P15 Line 24 – it would be easier to examine Figure 4 if you gave rough dates for the phenological events you describe in the text (as you have done for leaf flushing) given the multiple points of increase and decrease for the evergreen sites.

Response: In the case of Hyytiala it was possible to identify the dates of shoot elongation and leaf flushing manually from the digital images (see Figure S4). However for most of the other sites the distance between the camera and the trees is large making it extremely difficult to resolve such detail and identify the phenophases. Locating cameras closer to the canopy at these sites would certainly improve phenology measurements. However, we are aware that there are some logistical constraints often imposed at flux sites given also that the camera should not be placed too close to the canopy to still get images that are representative of the flux footprint.

Figure 4 (and 6): given these are latitudinal comparisons it may be nice to have the approximate latitude given on each plot.

Response: We have added the latitude coordinates on each panel.

Figure 5: it might be beneficial to extent the dashed lines up through the temperature and PPFD?

Response: We have extended the lines through all the panels on Fig 5.

7)P16 Lines 16-18: Interesting that although the green fraction increased around the time of a short spell of increased temperature, the GPP did not change (bp4). How might this be explained?

Response: We think this increase in the green and red fractions and decrease in the blue signal is linked to the very fresh re-growth of grass shoots after the mowing event and the gradual build up of LAI and pigment content (see Fig 11). However, at the very beginning of this particular period when the green increased the temperature in fact seems to be decreasing and rises again after a few days. This may be the reason that the GPP increase slows down a little during this period but then increases again as temperatures become higher.

Overall what's your advice for using these data for evergreen trees? Are the difficulties regarding detection of phenophases using piecewise regression a limitation? Do you think a slightly different detection protocol is needed for evergreen stands?

Response: As mentioned above our first advice would be to install cameras close enough to the evergreen canopy so that phenological events can be verified visually. However from our experience it seems very difficult to derive phenological events automatically from digital images. But if phenological events in the conventional sense (i.e. needle elongation) do not cause large variations in the green signal perhaps we should understand better the breakpoints or changes that are picked out, such as the snow-free periods, the dates when the 're-organisation' of pigments occur with acclimation to leaf temperature and when the maximum green signal is observed in the season. Perhaps these seasonal landmarks are more important for understanding the inter-annual variability of evergreen CO2 sequestration?

Section 3.1.2 P18 Lines 4-6: The fact you can detect the impact of flowering and cutting is indeed interesting. What type of grass and flowers are at this site? Do you have any photos that you could add into the supplementary information (as for Figure S5)?

Response: The vegetation at this site (Neustift) has been classified as Pastinaco-Arrhenatheretum and is characterized of a few dominant grass (Dactylis glomerata, Festuca pratensis, Phleum pratensis, Trisetum flavescens) and forb (Trifolium pratense and repens, Ranunculus acris, Taraxacum officinale, Carum carvi) species. On day 131 one sees the yellow flowers of R. acris and T. officinale and on day 181 the white flowers are from C. carvi. A figure (S6) has now been added to the supplementary information showing the mowing and flowering events.

P18 Lines 9 and 10: "Even more challenging" is repeated. I am unclear as to why having 8 breakpoints made it more difficult to detect the start and end of the growing season. Please could you explain this in more detail?

Response: The main challenge with using 8 breakpoints when you are perhaps only interested in 2 of them (leaf growth and leaf death) is that you will need to check manually all 8 breakpoints to be sure you have the right date for the right event and this will take time. If there is only one site that has this issue then it is probably not a challenge. However, if there are many sites like this, then this can start to take up some

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time. Surely ecologists working on flowering events would like the choice to have more breakpoints and the software we developed can accommodate this type of scenario. We have added a clarification on this in the manuscript.

P19 Line 2: Migliavacca repeated.

Response: The text has been duly corrected.

P19 Lines 2-4: This is a very modestly described caveat of the method used, but surely it would be the same with any method used? If the grass is buried under litter than no sensor (or method) will detect the start of new leaf growth.

Response: We completely agree with the reviewer on this issue.

P19 Lines 6-25: This analysis is of course true but I suspect that it is not a lack of knowledge of different crop sowing and harvest dates that results in the crops being treated as grasses in models but rather the difficulties of prescribing these dates and different crop management strategies. I do not know but are there not country/EU-wide datasets that give the broad dates of sowing and harvest dates for different crop types? Nonetheless these data do indeed present a good demonstration of this issue.

Response: We fully agree with this comment and re-formulated the sentence to clarify this point.

Section 3.1.3 P20 Lines 4 and 5: For a second I wondered why the red and blue signals decreased, and then I remembered that these are relative signals. Perhaps others will not need this clarification but it may be even more informative to remind readers of that here?

Response: We thank the referee for this reflection and we have added a clarification on the relativity of the signals in the text.

P20 Line 12: It may be useful to give examples of the Mediterranean sites in the text as you have done for the continental sites.

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Response: We are unsure what is required here. There is already a fair bit of discussion about the Mediterranean sites.

P20 Lines 15-17: What do you think is the cause of this variability if not a climatic driver?

Response: It is not entirely clear what variability is being referred to in this comment. If it is the length of the growing season (?) there could be multiple environmental factors at play including the light quality, temperature and moisture status of the site. Another possibility is that the species composition of the forest may introduce additional variability as different species often have different phenological attributes and environmental thresholds. For example, just from looking at the data presented one might also conclude that ecosystems comprised of deciduous Oak species have longer growing seasons than those dominated by Beech, however these sites are also oceanic or Mediterranean, thus we would need some Oak continental sites to test this statement.

Section 3.2.1âĂÍThis section presents very thorough and informative sensitivity analyses. Figure S6: The axes font is very small and may not be readable on paper

Response: We agree with the referee that this Figure has small font. We have re-orientated the figure to landscape for the final version and have improved the ticks for the Hotspot parameter that were too difficult to read.

P 23 Line 15 on: This is a nice extension of the first sensitivity analysis. Was the sensitivity analysis presented in Figure 11 conducted over the same time period as in Figure S6, even though the constraints are only defined for the spring green up? If so, it may be helpful to the reader to put the LAI, Chl and Car columns of the original sensitivity analysis in Figure S6 next to the 3 columns in Figure 11 to see the impact on the sensitivity brought by the extra constraints, but this is not needed. If not, then this should be detailed in the text and caption of Figure 11.

Response: We thank the referee for this comment and we have improved the text to

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prompt the reader to compare the LAI, Chl and Car panels in S6 to Figure 11. In addition we confirm that the sensitivity analyses for Figs S6 and 11 were performed for the same timeframe.

How general are these constraints? Could they be applied for all ecosystems (albeit with different ratios for different ecosystems)? Or do these constaints break down elsewhere? Have these constraints ever been applied to the PROSAIL model before?

Response: We believe these constraints are pretty general over most of the growing season. Feret et al., 2008 show in a number of different ecosystems that there are strong correlations between the concentrations of chlorophylls a/b and carotenoids (see Fig. 3 of Feret et al., 2008). Thus the general opinion is that these pigment characteristics co-vary in nature and they are not statistically independent. However, it does seem that there are some events, particularly senescence, when a decoupling of the chlorophyll from some of the xanthophyll carotenoids may occur. For example Lu et al. (2001) observed in field-grown wheat plants a drop in [Chl] around 20 days after flowering that was also associated with a strong increase in the xanthophyll/chlorophyll concentration ratios. Their results suggest that the degradation of the chlorophyll pigment appears to be extremely rapid whilst the degradation of other leaf pigments can be played out over a few more weeks. Another possible moment in the growing season could be during budburst or early in the season for very cold ecosystems such as in the boreal region. Some studies have indicated higher amounts of carotenoids in comparison to chlorophylls in aspen at the beginning of the vegetation season (Hillker et al., 2011, Fig. 5a) when foliage requires protection from photo-oxidative damage, as the photosynthetic apparatus is not completely mature (Lewandowska and Jarvis 1977). Further studies may now be warranted to verify these constraints during budburst and senescence for different plant types. Lastly, we are not aware of these constraints being applied to the PROSAIL model before. Most previous studies have used PROSAIL in the inverse mode to solve for LAI or Chl content. Typically in these inversions each of the parameters is solved independently without any such constraints.

Section 3.2.2 P 24 Line 22: By adapted PROSAIL model I assume you mean with the fucntional constraints applied in the previous section?

Response: When we refer to the adapted PROSAIL model we are simply indicating that it has been adapted to make the link between RGB reflectance outputs and camera digital numbers. We have removed the word adapted to avoid any confusion in the text.

In Table 2 do you mean to refer to Figure 12 and not 13?

Response: We thank the referee for spotting this typo, it has now been changed to Fig 12.

Figure S7: Do you mean to refer to Figure 12 and not 13?

Response: We have double-checked the Figure S7 legend and it seems to be correct.

P 26 Line 2: Indeed it would be good to re-do this analysis for years where pigment concentration data (or other parameters) are available for extra validation.

Response: We completely agree with the referee and hope this study will encourage the community to measure some of these useful parameters.

Section 3.3 This seems to be a very rigorous assessment of the technical difficulties. P28 Line 20: Mizunuma et al. repeated.

Response: This citation has been removed.

ConclusionsâĂÍ P 31 Line 14: spelling mistake for “archives”

Response: Corrected.

Further possible points for discussion: - I have already mentioned this and do not wish to repeat myself, but what are your overall suggestions of improvements to or usage of the piecewise regression method for detecting phenological events. It seems that that visual inspection may be needed for evergreen and managed ecosystems as you've described. Or would another metric like a threshold useful?

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Response: The breakpoint approach was used as a consistent methodology allowing the extraction of phenophases from a variety of natural and managed ecosystems belonging to the network. We agree that some breakpoints raise questions and require visual validation, but at the same time this approach is successful in identifying the main phonological transitions in natural ecosystems and, more importantly, management practices in grasslands and croplands, where other methods such as curve fitting and threshold extraction would have failed.

The objective of this paper was to apply a consistent methodology across an entire network of very different ecosystems. In other cases, e.g. for an ecosystem-specific analysis, we would not necessarily advocate a breakpoint approach as the best solution, but rather we suggest the use of fitting and extraction methods that are best suited to the data under analysis.

- These data and the RT modeling could be used to see if you can see the same events (and validate) satellite data. Coming back to Section 3.1.3 (P21 Lines 17-21) following the modeling discussion. Perhaps the model could be used to derive NDVI which could be more readily compared with ground-based measurements as well as satllite data products?

Response: We believe this is a good point to raise in the conclusion and have added a brief paragraph on this.

- Of course this is a preliminary exploratory study; however, the thorough analysis could be complemented by discussing the impact of this type of RT (turbid medium) model and the assumptions used. For more complex canopies (vertical heterogeneity and/or a mix of under- and over-story) the PROSAIL model assumptions may break down, particularly as these are near canopy measurements and therefore local-scale effects such as clumping are probably not properly accounted for.

Response: We believe this suggestion could be a relevant and interesting study to make across the network in the future. However given the length and density of the

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manuscript it is currently beyond the scope of this study to go into too much further detail on these matters.

As well as using the modeling to quantify and monitor plant physiological status, how else might you use this framework? You mentioned improving the phenology models of dynamic vegetation models in the introduction what other applications can you see from these data? Are any of the time series long enough for trend analysis for example?

Response: We believe that this camera network along with the other networks in the USA and Asia when sustained over decades will provide a valuable archive of ecosystem responses to climatic change and variability. Presently many of the time-series in the European network are too short to conduct robust and meaningful trend analyses but of course this is the long-term goal of such networks. However, we also believe these networks and these types of analyses can provide us with information on the impact of stress events such as frost or insect outbreaks on canopy colour signals and their impact on the energy and carbon balance of ecosystems.

Interactive comment on Biogeosciences Discuss., 12, 7979, 2015.

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