

Earth Surf. Dynam. Discuss., author comment AC2
<https://doi.org/10.5194/esurf-2021-58-AC2>, 2021
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Reply on RC2

Andrew V. Bradley et al.

Author comment on "Identification of typical ecohydrological behaviours using InSAR allows landscape-scale mapping of peatland condition" by Andrew V. Bradley et al., Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2021-58-AC2>, 2021

We thank the reviewer for the time and effort in reviewing this manuscript and the recognition of the significance of this work. We have considered the comments and recommendations and provide our responses in this discussion thread. Reviewer comments followed by responses.

Reviewer #2

This is an interesting study about the applicability of InSAR data for mapping and assessing peatland elevational changes and peat condition, which could provide a far more time-effective method for assessing remote peatlands worldwide. The results could also be used to enhance peatland restoration strategies.

That said, I have some reservations about the manuscript in its current form. The methods section is very difficult to follow and could benefit from some rewriting, possibly with some parts of the results section relocated there (see specific comment on L284-314 and 370-390 below). Additionally, throughout the paper, there seems to be an effort to use the technical terms for measured factors (e.g. multiannual average velocity). Given that these terms are defined (albeit quite late on in some cases), it would make it far more understandable to the reader in some cases to use the definitions in text rather than expecting the reader to constantly refer back (or in some cases forward) to the definitions. For example, L334-335 "Where bare peat is dominant, the most negative velocities occur." Could be written as "Where bare peat was dominant, the greatest peat subsidence occurred."

I also have two concerns relating to the methodology and interpretation of results:

- This study used four and a bit years of data (see below to comments on actual length) yet only really considered data from 3 years due to 2018 being classed as a drought year. If data had been taken over a different period (e.g. 2017 to 2021), I am not sure that 2018 would have been excluded quite so readily as May 2020 was hot and dry and April 2021 was very dry but cold. Therefore, particularly in the context of climate change and dry summers likely to become more prevalent in the UK and parts of Europe, I would like to see much greater consideration of the 2018 drought otherwise I have doubts that this method would prove particularly useful for regions where the climate is changing (and see more specific comments below).
- Whilst I understand that pool systems will (should) always have Sphagnum and are

easy to identify, I question whether assessing the predictive accuracy using just one PFT and hydrology cluster is wise (L255-264 and 380-394). Surely this is the category that is most likely to be accurate anyway, given that pools will mainly be on similar topography and slope. Why was predictive accuracy not verified by ground-truthing, since field visits are mentioned elsewhere, across a range of clusters? Also, to claim that "[Sphagnum pools] correspond to the part of peatland systems most unequivocally associated with 'near-natural' ecohydrological condition" is possibly a dangerous assumption. Whilst this may be true for the bogs that you included in your study, it is not necessarily the case for all bogs across the world, nor even across the UK (there is quite a bit of peat formed from sedges for example). Relying on this assumption could seriously limit the applicability of your method of peatland mapping and condition assessment for global use, as you have indicated you think the method could be used for in both the abstract and discussion, and therefore I feel that further validation is necessary.

General:

Some of the difficulties in understanding the terminology have been addressed with the restructuring, so we now define terms earlier. We also feel that the terms should remain technical as they are relevant to the signal processing, and use of more familiar terms may cause misunderstanding with respect to the actual data being analysed and discussed. We agree with the restructuring comments and moved methods that were mixed in with the results L284-314 and L370-390

Comment 1

This approach could be used to analyse climate, etc, but this is beyond the scope of this paper. We have omitted the drought year as this removes additional complications in the time series analysis in particular the potential for anomalous results if the rate of subsidence exceeds what the InSAR technique can measure. The scope of this paper is illustrating the method of characterising peatland using InSAR measures of surface motion and there is considerable potential for future research from this point.

Comment 2

The final peatland state map presents the condition of the whole peatland system, based on the physical response of the peat. We are aiming to validate the mechanical response most likely to be occurring where ideal peatland conditions are found, i.e. on 'soft', wet peat, which in this case is expected to be Sphagnum rich and usually associated with pools. We initially described the classes as sphagnum and shrub classes, as this is what our ecohydrological investigation clarified for the Flows, and this is what restoration experts feel comfortable to target and understand in this area. The classes could equally be ascribed appropriate plant functional groups in other (global) locations. The inclusion of vegetation in the classification description appears to give provenance to the method so we re-describe the classes as 'soft' peat, 'stiff' peat and 'modified/thin' peat states, since our classification is based on surface motion behaviour not land cover (more easily derived from optical images) thus also demonstrating an interchangeable classification for peatland globally.

We aimed to present a predictive accuracy of the map that could be made remotely. Pool systems are the most identifiable feature where we have most confidence in the 'wet' condition. The opposing category, dry (stiff) peat is most likely on steep slopes, but this condition is less consistent due to complexity of

controlling variables, management / peat thickness / local hydrology etc. We acknowledge that the current map as presented is somewhat one sided, and now discuss the issues and outcomes of remote validation (please see also comments to reviewer 1).

Specific comments:

Figure 1 – This figure would make much more sense if b-f were given later in the paper after they had been explained. The z axis (velocity) is unclear as to which end contains negative numbers and which positive. What exactly does “frequency” (inset) mean? Is this number of obtained measurements? For the explanation of coloured dots, more clarity is needed. Why are there two different colours for front and back? Front and back of what? The graph? Also, why do these graphs only use one year of data when there is over 4 years data available?

Figure 1 has now been split, the plots are shown after explanation of the metrics and time series. There is more explanation to clarify the remarks made above. We explain that one year is shown for clarity and we will make the axis clearer.

L62-65 and L95-97 – these lines repeat the same information and define (some of) the same acronyms. Better to combine and only have once.

L62-65 and L95-97: addressed

L101-104 – whilst this may all be fully understandable and sufficient detail to someone using the software, to someone who has not, it feels as though there are lacking details/explanations. Are these default settings? If not, why were those thresholds chosen?

L101-104: more detail and clarity added

L104 – presumably Wick Airport is not underlain by peat? Worth clarifying.

L104: more detail added

L107-109 – this is not a full sentence and it is unclear where it is going. Consider revising.

L107-109: sentences revised

L118 – it says 4-year 5-month data here but the dates given in L94 do not add to this. Likewise Figure 2 legend starts 12th March 2015 to 1st July 2019 but text suggests June not July. Which is correct?

L118: 1st July correct and made consistent in the text.

L118-121 – this is the only time that PCA channels are mentioned. Why are 10 calculated? Did you use them for anything else? If not, why are they mentioned?

L118-121. We apologise for including the PCA settings. The mention of PCA components is redundant and distracting information, as the reconstruction was based on using EOFs rather than PCA channels. This information is not necessary and has been removed.

L122-128 – whilst I can see from Figures 2 and S1 that 2018 is different from the rest due to the drought (although not that much if just looking at the R2 line in Figure 2), I feel it would be worth giving more consideration to the results given that include 2018.

Unfortunately droughts such as in 2018 are likely to become more frequent under climate change (e.g. May 2020 was very dry and warm, and then April 2021 was incredibly dry throughout much of the UK but it also happened to be very cold) and therefore if you want the model to truly represent peatland condition and apply to restoration endpoints, would drought events not need to be considered within the approach? It is also interesting that the drier bog merely showed greater amplitude in displacement rather than being completely out of pattern as the wet bog was.

L122-L128 The purpose of our paper is to demonstrate the capacity of surface motion measured by InSAR to characterise peatland condition. Detail analysis of the response to climate and anomalous weather patterns would be fascinating as would be the temporal specifics of restoration but is firmly beyond the scope of this paper, we will however include this point in the discussion

Figure 2 – it is rather confusing to have two legends with the same coloured lines in. Either it needs to be specified that one legend relates to horizontal and the other vertical lines or each line needs to be given a more distinct colour, preferably the latter since specifying lines as “grey” (L131) in the legend does not improve clarity.

Figure 2 – Improved clarity by darkening vertical lines.

Figure S1 – the legend states that EOFs 1-4 and 7-8 have periodicities greater than 12 months but it looks as though 7-8 may be less. However, it is hard to tell as the quality of the graphs is poor and the axis labels need to be larger.

Graphs redrawn more clearly as suggested and we report the periodicities as calculated by ‘spectra’.

L147 – why does Figure 4 get mentioned before Figure 3? Also, the relevance of Figure 1 to the exclusion of the irregular time series needs further explanation.

L147 – Figures and text re-ordered during restructuring and it has been made clear that the irregular class is not shown in 3d axis plots

L188-190 – this sounds like vegetation is separate from PFT. Please clarify.

L188-190 - some minor edits to the paragraph reviewing the use of the word vegetation and changed to PTF where appropriate

Table S5 – this needs further explanation. Why is “100% - 5/5ths” by itself and seemingly different from “up to 5/5ths”?

Table S5 – up to 5/5ths is 80% to and including 100% hits, 5/5ths is exactly 100% hits illustrating the number of buffer areas with a perfect scores. Labelling has been simplified in table to avoid confusion.

Figure 4 – the legend for b) states the thin/modified peat areas are hatched but they appear just to be grey (using legend from 4a). I’m a little lost as to the usefulness of 4c, particularly as forested land is the only thing that is clearly distinguishable and yet forested land was not classified on the peat condition map in 4a. Either there needs to be more explanation in the legend/text about it, or it could be removed.

We retain the optical image for comparison as this adds context and understanding to the classification and restoration work in the area. Many of the forest to bog restoration programs have occurred since 1997, so we are observing a space time picture of recovering bog from felled forest to either

'soft' (wet) or 'stiff' (dry) bog. Some blocks appear to still be forest as the ESRI image database does not have cloud free recent images where forest has now been felled. We recognise that the image does not clearly explain that the polygons were thin peat, forest etc. The polygons have now been simplified and labelled to indicate their status at the time of the analysis e.g., forest, forest to bog, thin peat and open peatland. We are also searching for a better contemporary image to replace this. The forest areas are not masked out as they are on peat – please see response to Reviewer 1 for details.

L284-314 and L370-390 – these lines are not really results. They are mainly methods and should thus be in that section. However, they explain what you did far more clearly than the majority of the current methods section. I would suggest either beginning the methods with this and then continuing to fill in more details after or using these lines as the basis for the methods and expanding each step out from this more simple explanation. There are also some details which are given the methods section (e.g. about HCA) that are then repeated in the results. You need to decide which is which and avoid so much repetition, which should make it much clearer.

L284-314 and L370-390 – restructuring of the paper has eliminated the repetition and issues described

L319-329 – it is very unclear as to what “monthly frequencies” refers to. Is this when the maximum peat height occurs or the number of up and down oscillations? Or something else?

L319-329 – Monthly frequencies refers to an aggregation of maximum peat height by month not day. This aggregation into months has now been made clearer in the method section.

L343-344 – I disagree. Whilst the first year appears to show strong linear relationships, the other two are far less convincing. As a minimum, I would suggest a linear regression on these data and providing R^2 values in the text to back up this statement.

L343-344 – We are investigating the advantage of using a regression and different diagram formats to present our observations more clearly.

L348-350 – again, I disagree. To me, it looks like the middle of the three years shown had the greatest amplitude. Making sweeping statements across only three data points (effectively each year is a point here) is not advisable. Also, what does this mean? That some years are wetter than others?

L348-350 – Our explanation for this has been written more clearly and we have added a precipitation graph to the manuscript as further evidence in figure 2.

L355-357 – this is the first time I have understood exactly where this bimodality is coming from and what it means. It might be worth explaining this earlier in the text.

L355-357 – the document has now been restructured, which should make the bimodality and meaning more obvious. We retain the position of the sentence as this is a result.

Technical corrections:

L57 – missing comma after “(Fiaschi et al., 2019)”.

L57 – addressed

L97 – define DInSAR.

L97 – addressed

L102 and 105 – the (54) and (55) feel like they should be references but the reference list is not numbered.

L102 and 105 – addressed

L105 – define/give external reference to SNAPHU.

L105 – addressed

L108 (and some other mentions of this paper) – why is this “L. Alshammari,”? Referencing should be consistent. Check reference software input/settings.

L108 - addressed

L110 – “Each motion time series was processed as follows, to quantify the specific peatland surface motion metrics.” Would read better as “Each motion time series was processed to quantify the specific peatland surface motion metrics as follows.”

L110 – addressed

L114 – Same for “R. Hyndman” about referencing.

L114 – addressed

L117 – define MSSA.

L117 – addressed

L152 – “in in” at end of line.

L152 – addressed

Section 2.2 and 2.3 appear to be a continuation of one another so would make more sense combined into one section.

The restructuring has eliminated the heading of 2.2 of the original submission.

Supplement 1.2 – this is not referred to in the main text; should it be? It also refers to Figure S3 but should to S2.

addressed

L182 – what does (64) refer to?

L182 – addressed

Table S1 and S3 – define PFG. Also define all letter in PFG column as not all are obvious (e.g. LoA; G in brackets).

Table S1 and S3 – addressed

Table S2 and S4 – what is the purpose of Rank Name/Name when there is highlighting to

give ranks and Hydro Name (presumably final class name) as given? Needs more explanation in the legend.

Table S2 and S4 – addressed

L188 – Typo of “category’s”. Should be “categories”.

L188 – addressed

L203 – Typo of “group’s”. Should be “groups”.

L203 – addressed

L258 – Figure 4d is not a map. Did you mean 4a?

L258 – addressed

Section 3.2 – there needs to be consistency in the way variables are referred to. In the first paragraph of this section, all variables are given as words. Why change to capitals for a variety of different lengths for the rest of the section, especially since most variables are one, or maximum two, words long? This makes it look very much like these parts were written by different authors who had not discussed the approach. Also, there is no need to constantly redefine abbreviations in brackets – if it is done well once, that is sufficient.

Consistency in the text has been addressed with the re organisation of Figure 3 diagrams under seperate headings.

L321 – “September and or October”. Is it “and” or “or”?

L321 addressed - and