Earth Surface Dynamics Discussions

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Reply on RC1

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-AC1, 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 #1

This is a fascinating and useful study using novel InSAR techniques to assess blanket bog condition. The results have important implications for the application of restoration methods in peatlands.

However, the manuscript in its current form is very challenging to follow, and would benefit from a re-write of the methods section. In particular, the first few paragraphs of Section 3 (lines 284-314) should be much earlier in the paper, as they contain a useful and straightforward explanation of the metrics used. Consistency of terms and presentation throughout the manuscript would also improve readability, see detailed comments below.

The results are well written, but the discussion would benefit from more work. In particular, comparison to previous studies and an assessment of the limitations of the method would improve this section.

I have two major concerns with this article, relating to the analysis and interpretation of results:

- Areas of forestry are largely classified as either wet sphagnum or shrub-dominated bog (Fig. 4). This is clearly an issue with using only 3 (or 4 with irregular) categories of peatland type. The authors should definitely discuss this, and could further consider masking areas of forestry in mapping.
- The validation of the method only tests one category, that of wet bog. At lines 380-394 the authors suggest that the method 'is converging towards 100% accuracy in identifying Sphagnum dominated pool systems in a near natural ecohydrological condition'. It must be noted, however, that a method classifying the whole study area uniformly as wet bog would give the same result using this method of validation. The authors have visually inspected the results with regard to the other categories, but the lack of quantitative validation suggests that the threshold has the potential to be set to

favor the wet bog category.

General

The paper has undergone restructuring as recommended and paid attention to comments on consistency. We will discuss further our methodological limitations and refer to relevant studies where necessary.

Comment 1

The forest blocks are plantations on peat rather than on mineral soils, so we do not exclude them from the analysis. The classification under the forest is likely to be remnants of bog breathing on deep peat that is damaged in the process of tree planting, there are also open rides between the forest blocks that still support bog vegetation, and quite often there are isolated undrained pool systems within forest plantation, these can show as the different condition classes. There are generally more irregular points in forested areas as the trees do influence InSAR coherence, creating signal noise (which the MSSA can help reduce).

Comment 2

Our aim was to present practitioners with a remote method of validation. We acknowledge that the validation is quite one sided, although we did not intend it to be that way, as explained in the text this is the one identifiable feature that will represent wet peat characteristics. For the other classes, degraded/thin and dry (stiff) peat it is much more difficult to characterise remotely using a landscape feature that will consistently relate to this class. We have considered using features such as presence of drainage channels, but these do not represent a consistent peat condition due to variations in, age, length, maintenance, and channel gradients. Similarly, with topography by considering steeper slopes >5 degrees peatland should be drier. This is problematic as these slopes naturally display variations in wetness and drainage, which is difficult to assess without a high resolution DEM. These slopes are also geographically confined to the south west of the study area creating a spatial sampling bias within the study area. So pool systems are the uniquely widespread and easily identifiable condition class and validation of more degraded classes would require fieldwork which is beyond a remote assessment and the scope of our current study. Our best solution is therefore to acknowledge and discuss this limitation. We also believe that now we have made an enhancement to the detailed classified maps in the original Fig 4, this goes some way to evidencing the potential quality of our classification.

Detailed comments:

Is the multiannual average velocity the overall/total velocity of the whole timeseries for each point? Line 298 seems to suggest that only part of the timeseries is used. It could be beneficial to add a figure showing this metric.

L298 - It would be misleading to represent velocity on this graph (which would look like a regression) as velocity comes from the interferometry processing not the time series. It was stated in the caption that the velocity is not shown and that the source of velocity is outlined in the methods.

Technical comments:

Fig 1 – I would suggest moving figures 1b-f to later in the manuscript when the three metrics have been fully explained.

Fig 1 – split as recommended

Line 37 – The authors may wish to mention recent work on peatland hydrology using SAR backscatter, particularly work by Asmuβ et al. 2019, and Lees et al. 2021.

Line 37 – We have looked at these references and consider them to be relevant to SAR backscatter rather than SAR Interferometry techniques.

Line 104 – More explanation of the point threshold could be useful.

Line 104 – sentence now provides more detail

Line 118-121 – This PCA method could benefit from more explanation. My current understanding is that, for each time series, a 12-month moving window was applied to split the data series into multiple timeseries of length 12 months, each new time series starting with a time step of 12 days. This would give approximately 60 12-month time series over a 3-year period. The PCA analysis was then completed using this dataset – is that correct?

Line 118-121 – Almost, the mention of PCA components is redundant and distracting information, as the reconstruction was based on the original data using the EOFs rather than PCA decorrelation. This information is not necessary and has been removed.

Fig 2 - The amplitude of the dry bog looks larger than that of the wet bog, but this is the opposite of the explanation in Section 2.4.

Fig 2 – There are three metrics that determine the peatland condition, high amplitude is possible in both conditions (see figure 3 plots), note that the timing and velocity also vary. There are many factors that may influence the high amplitude such as proximity to riparian zones, reed beds, peat thickness and so on, which may be specific to this time series. It would be difficult to illustrate the metrics for a subdued low amplitude time series. These examples have been chosen for clarity.

Line 207 – More information is needed on low, medium, and high classifications of topography.

Line 207 – class boundaries provided

Fig. 3 – This figure is complicated to interpret due to the mix of metrics and groupings presented in different ways. I would suggest presenting the three different metrics in a more consistent way, insofar as that is possible.

Fig 3 – We have split the diagram up into individual parts under thematic headings and will produce the diagrams in a similar format.

Line 240-250 – If I have understood this correctly, 'wet' bog pixels were identified by selecting pixels with the highest amplitude and velocity, and earliest peak. Why were the field observations, sub-sites and random points, not used for this?

Line 240 to 250 – This is not written clearly in the submission as some of the results came before the method. This is now more logically explained in the

method, *'Once we were able to understand the relationship of ecohydrology to the metric values within the two clusters of the 3-axis plot, we used these criteria to identify where in the 3 axis plot restoration practitioners would consider a good condition 'wet' sphagnum and classified the state of the whole system relative to this condition. We identified this to be in the later cluster with a high amplitude and velocity, corresponding to 'soft' peat. The actual reference point was selected by splitting the distribution, to extract the 'back' of the later cluster from the rest of the data, where the extreme point was then searched for (fig. 3axis plots),…'*

Line 321 – is 'highest monthly frequency' the same as the date of annual peak (metric 1)? Sometimes months are used and sometimes date/DoY.

Line 321 – No, this is a further aggregation of peak timing data into monthly categories that has now been explained more clearly in the method.

Line 343 – The graphs for the first year certainly show a strong linear relationship, but I am not convinced by the other two. The authors could consider applying regression models to this data to assess the strength of relationships

Line 343 – we will improve this diagram to show the progression in the linear relationship with respect to the levels of precipitation. We have also added a precipitation graph in the first figure illustrating the interannual precipitation levels.

Line 33 & 409 – It might be worth including some discussion of previous attempts to measure peatland condition using remote sensing, particularly those that focus on the same area, e.g., Artz et al., 2019.

Line 33 & 409 – We have referenced this work. However, without a thorough comparison of this optical to our InSAR derived data, which is beyond the scope of the paper, we would be speculating on the details.

The authors mention early in the manuscript that the data from 2018 were excluded due to drought affecting the InSAR results. It would be good to see this explored further in the discussion, both the negative impacts of this on the reliability of the method for future use, but also the potential benefits of using the method to detect drought impacts.

It is a fair and interesting point but this is not the intended scope of the paper. However, we agree that that it should be mentioned in the discussion section

Technical comments:

Line 27 – Consistency of terms: peatlands vs peatland

Line 27 – we use peatland

Line 97 – Define DInSAR

Line 97 – addressed

Line 103 and 105 - Not sure what (54) and (55) refer to?

Line 103 and 105 – addressed, incorrect reference formats

Line 133 – Define MSSA

Line 133 – addressed

Line 192 – grammar error ‘had were’

Line 192 – addressed

Line 246 – no comma needed after ‘case with’

Line 246 – addressed - no comma needed after ‘case with’

Fig.3 - RAND and SS definitions need to be moved from the caption of (b) to (a).

Fig.3 This has been addressed with the reorganisation of this figure.

Line 336 – ‘least negative/most negative’ – unclear if this means lowest/highest values, or smallest/greatest numbers of datapoints

Line 336 – in the method section we now state clearly that a measure of peatland swelling is a greater positive value and subsidence a greater negative value

Line 374-378 – grammar – consider using a colon to introduce the categories, and semi-colons to separate them, rather than full stops.

Line 374-378 – addressed