

Earth Surf. Dynam. Discuss., referee comment RC1  
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## Comment on esurf-2021-58

Anonymous Referee #1

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Referee 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-RC1>, 2021

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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.

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.

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

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

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

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?

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.

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

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.

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

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 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.

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.

Technical comments:

Line 27 – Consistency of terms: peatlands vs peatland

Line 97 – Define DInSAR

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

Line 133 – Define MSSA

Line 192 – grammar error ‘had were’

Line 246 – no comma needed after ‘case with’

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

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

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