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Comment on egusphere-2022-1173

Heather Reese (Referee)

Referee comment on "Permafrost degradation at two monitored palsa mires in north-west Finland" by Mariana Verdonen et al., EGUsphere,
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Reviewer comments

The paper by Verdonen et al., is a study on degradation of palsas and permafrost plateaus over two sites in northwestern Finland, where Active Layer Thickness and ground elevation has been measured systematically and annually since 2007, and aerial photos exist from 1960 and onwards (including UAS images) to measure lateral degradation. Verdonen et al user linear regression to relate changes in palsa area and height changes to climatic parameters. This paper contributes to a better understanding of the changes in the sensitive palsa mires of Fennoscandia, and their association with climate variables. It's well written, and represents a further step in understanding palsa dynamics and associated influences, both climatic and otherwise.

Main issues are marked with a *.

Questions and comments

Line 4 – It would be informative with a short form of the Latitude after the names.

Line 5 – I don't think that is true that your study focuses on the time period covered by your UAS data. It focuses just as much on your ALT measurements from 2007-2021 (albeit only within the non-degraded area of the palsas), as well as your RTK-GNSS ground elevation measurements, which to me is the more interesting data set since it should be more accurate. Even the next line in the abstract mentions using the longer time series of aerial photos. So I would take away this sentence ("The emphasis is on detailed change detection …").

Line 8 – Mention that the ALT data are annual from 2007-2021.

L61 – name the years that your study is looking at, rather than using the more vague "the investigation period" phrasing.

L66 – It would be better if you indicated what kind of sensor you are using for data collection from the UAS platform. Is it an RGB camera, an NIR camera, and/or a Lidar sensor?

L91 – Describe the area of the two palsas in the same way and give their dimensions, as the shape of the palsa may affect how it reacts.

Figure 1 d and f – If these are 1 or 2 m high, are these palsas or peat plateaus? Or were these taller some decades ago? Just double checking, seeing as you made a point about the difference between the two.

*In general – I think it would be better if you used the terms Digital Terrain Models (DTMs) and Digital Surface Models (DSMs) instead of the umbrella term DEM, particularly since your article refers to both kinds of elevation models. Or at least conscious use of the terms. Your RTK-GNSS created DTMs while your drone images will create DSMs.

*L120 – Do you mean that if there was lateral degradation in any year from 2007-2021 that you did not include this in the ALT measurements used in the regressions? If so, that should have some effect on your result (and maybe this is why you don't see strong relationships between ALT and climate parameters at the larger of the two palsas). Can you motivate your choice and make clear how using only the "Top of Palsa" mean ALT measurement can affect your results in the Discussion section. It seems like you would be missing the bigger changes. You can see the points you are missing when looking at Fig 2.

Section 3.2 – More details are needed on the sensors and specifications used to create these data. A Table could be useful here. What camera? What scale (or GSD- ground sampling distance) are the original images taken at? What full date? Which photo dates were the panchromatic, and what were the others? With the UAS, what platform (since this helps indicate which GPS was used)?

*L145 – You listed a number of issues that you ran into. In addition to this, the UAS-based data result in elevations that include vegetation heights (DSMs) and are therefore not completely reliable for showing accurate elevation from year to year, and therefore subsidence and volume changes over time. How tall is the vegetation on the palsas? In any case, this should be a primary reason why you can't calculate reliable volume changes

from these data. I would reword this section so that this is acknowledged. However, the orthophotos are useful. It will be much better when you get the UAS-Lidar data for calculating volume changes!

L145 – I think you should also indicate how you geo-referenced your UAS data. You mention problems with the equipment.

Since you have RTK-GNSS data taken annually, couldn't you calculate an RMSE for elevation of the UAS DSMs, indicating their potential error? Then again, that would mean you are comparing DTM and DSM. But still, you might be able to observe systematic errors across the UAS DSM. When you mention in results that you see a trend from southwest to northeast (Line 225), I wonder if it is due to a tilting of the UAS DSM, which can easily happen when good georeferencing isn't possible.

My main point here is not to re-do a lot of work or invest a lot more time in the UAS DSMs, because frankly these will always include uncertainty due to 1) including all surface heights and 2) poor geolocation accuracy if not fixed with RTK-GPS control points. I think you just have to admit and realize the weaknesses of that data set for accurately measuring subsidence.

L165 – Again this is a DSM and not a DTM (or DEM), with vegetation included. Finland has a national Lidar scanning – why didn't you use the DTM from that for the snow model (or even better, both)? Too coarse? Can this account for the rather large differences between modelled and the reference snow depth measurements (10-30cm difference)? Also, where was the vegetation classification from? Your own? In any case, what classes were there?

*L170 – I don't think the explanatory parameters are clearly given. A table could help here, or else you could more clearly state it in the text. For example, did you not test any precipitation variable, besides snow?

Fig 2 – I found it hard to see the outline of the palsa. Maybe a little thicker. Also, you should mention what your image is in the background of the 2021 images, and what date it was taken.

Fig 3 – Very nice information! This figure raises a lot of questions for me, such as What happened in 2012-2014 to cause this change in ALT?. Also, why the divergence in responses between the two palsa sites after 2014? I interpret the large error bars on Peera to indicate the faster degradation in process, likely due to the small size of the palsa, and the high edge-to-core ratio. Do you think the 2014 ALT measurement is correct for Peera? What causes it to be the biggest thaw measurement in case it is correct?

Line 191 – Give the R2 value of the few mentioned correlated variables in the text.

Line 212 – I find this paragraph to be confusing due to the mix of observing what I interpret you to mean lateral degradation as well as subsidence. It would be good to be clear here. The heading is about subsidence or volume change with the RTK-GNSS and the top of the palsa measurements. Otherwise, did you use RTK-GNSS to map the area loss (lateral degradation)? It is unclear, due to the heading, and then the mix of different vaguely worded "degradations".

A thought: Since you have measurements in both places, what is the relationship between the RTK-GNSS measured annual subsidence and annual change in ALT? You wouldn't expect (intuitively) to see a fluctuation in ALT at the same time as you have a constant loss of palsa height. Would be a very good figure to include, since you have the data. (OK, I see in the Discussion you mention this, and try to explain it).

Table 1 indicates that your volume change measurements using your DTM from RTK-GNSS is based only on the "Top of Palsa" area. Good to make sure that is clearly stated in the methods.

Line 220 – Include in the sentence that this is height change measured by the UAS DSMs. Also, are you measuring only the "Top of Palsa" area, or what area are you using? To try to figure that out, I read back in methods, where it sounds like you have used the 2016 extent of the palsa, as delineated from the very detailed orthomosaic, so it will be I guess, a different area than "Top of Palsa". Do I interpret that correctly?

A thought: you would be able to confirm whether subsidence of 20 cm between 2016-2021 found using UAS DSMs corresponds with the subsidence measured by RTK-GNSS from the same time period 2016-2021, given that you looked at the same area.

Line 229: Well, you can't measure the internal permafrost with the RGB images which only show the surficial extent of the palsa. Also Line 294 you refer to how UAS data can lead to overestimation of permafrost. The aerial photos, or any surficial representation of the palsa is only that – the representation of the geomorphological form of the palsa. To find the permafrost, which is an internal characteristic, so far the ALT measurements are needed. Also in Line 294 – it wouldn't be only UAS, but also any aerial photo, or even Lidar that would "overestimate permafrost".

Line 239/240 – Include in the sentence that this measurement is derived from manual delineation of palsa area from the aerial photos from 1960, …2021.

Line 240 – that's quite a sad loss of area…

Fig 6 – Legend text is pretty small. Also, I was confused about which legend belonged to which square. Maybe better to make the figure a little bigger, and clearly divide the two sides of absolute and relative change maybe with some lines or column names and Legend heading.

Fig 7 – Nice map, I like this a lot. Is there a way to make it larger in the publication?

Line 259 – Much better description of the results is needed here. What was the R2 of the most correlated climatic variables? Without proper description of the result, it is hard to have any discussion, and hard to compare to other studies (eg Olvmo et al. 2020).

Why not have a figure similar to Fig 4 for your area loss? If not, then I think you should at least bring Table A2 into your main text, as I think it is more important than Table 3 and Fig 9.

Table A1 – Put that the ALT and RTK data are annual from 2007-2021 in the Table Text.

Table A2 – Put that the area loss data is from 1960-2021.

Line 299- The palsas in Olvmo et al 2020 are also larger than those in your study. Would be good to put the size of the palsas from Olvmo et al in the discussion. As you write, Borge et al (and I think Seppälä too) talks about the importance of the morphology in relation to degradation.

Line 315 – are they "more important" than climate? Or merely "also important factors"? I think the latter.

*Also, do you think your use of only "top of palsa" area measurements of ALT has led to a lack of a strong correlation with climatic factors, particularly in the larger of the two palsas you study?

Line 351 – rather than say "the permafrost area in 2021 was less than 25% of that in the 1960s" I would say that "the palsas in 2021 have shown a lateral degradation of 75% *(or whatever the number is…)* the 1960 areal coverage", since that is what you really

assessed that with the aerial photos. What area exactly the permafrost is (an internal characteristic that you aren't seeing with the images), isn't necessarily the same as the extent of the palsa at the time you image it.

Corrections and text improvements

Line(L) 17 – "its extent" is vague. Replace with a better geographical noun – whether " the Arctic" or "the Arctic permafrost region".

L23 – Write so it is more clear… "The main difference between peat plateaus and palsas are in…"

L31 – mires'

L49 –"ALT varies from a …"

L146 – "UA system settings" should be "UAS settings"

L86 – palsas'

L132 -aerial

Fig 5 should appear before Table 1, according to the earlier reference to it in the text (at Line 212).

L332 – "…in which November …" Delete "the". Or, do you even need this clause?

L333 &359 – Arctic (I think it should be capitalized when used as Arctic region)

L334 – …ground's thermal…