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Surface kinematics of periglacial sorted circles using Structure-from-Motion technology

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Final Response to Referee Comments (Interactive Discussion)

This letter is a response to referee comments and describes how we will revise our manuscript. The exact actual changes will then be listed in an attachment to the submission of the revised manuscript. All figures are initial drafts only. In this letter we combine the responses to both referees, Bernhard Hallet and Ernst Hauber, as they are closely related.

Most important, we would like to thank the two referees for their exceptionally constructive, thoughtful and detailed comments. Their big effort will certainly improve our study and is greatly acknowledged.

Comments by Ernst Hauber (italic):

(1) … Clearly, better resolved time series including different seasons, and supporting observations (e.g., on subsurface motions) should complement the SfM approach in future field campaigns and would lead to improved hypotheses on sorted circle development.

We absolutely agree and will expand on this in the outlook of the manuscript

(2) A note on the terminology used in the manuscript: Sometimes the reading is complicated (at least it was for me) due to the varying use of terminology, e.g., "rings", "ridges", "circles" etc. are used. I suggest to have a figure with clear definitions of the individual morphologic elements of a sorted circle (in principle, inner domain and outer ridge), and then use this convention consistently throughout the manuscript. Perhaps the labels with terminology could easily be added to Figure 1.

We agree and will define and use a uniform terminology ('gravel rings' for the outer part and 'fine domain'/'inner domain' for the inner part; 'circle' for the entire form; Hallet, 2013) and mark accordingly in the figures.

(3) Another comment: I miss a discussion of possible local effects that could be responsible for differences between the three sorted circles. For example, from Figure 1 it appears that the three sorted circles are located to the southeast of Geopolen hut, near the lake and its outlet where the artificial little dam is located (and where one of Hallet's fenced field site is located). I am quite familiar with this site, and I wonder if local gradients (for example, vicinity to ponding water) may be large enough to account for such differences, e.g., due to varying soil water content or else. Perhaps the authors could elaborate on this possibility?

We are grateful for this hint, and admit that we neglected this possibility. We will elaborate more on this.

(4) Block #6049: A note on the reference rock at Geopolen not being bedrock: Would it have been possible to use bedrock elsewhere? For instance, there are large outcrops about 1.5 km WNW of Geopolen, which I am sure the authors are aware of. Would this distance be too large?

This, and all other problems with absolute georeferencing, are clearly among the 'lessons learned' for us. There is not much we can do for this study, but we will more clearly identify potential improvements to our measurement setup in the conclusions/outlook. We learned that cm-accuracy is not sufficient for absolute georeference in order to detect overall changes of the circles within a few years. A suit of measures is necessary to arrive at mm-accuracy, among which certainly a reference point on bedrock. Also, GPS alone is not sufficient and will have to be supplemented by, e.g., optical levelling. (See also 15).

(5) Block #6058, line 4: Areas do not "constitute" volumes. I guess the authors want to say that the volume change associated with areas of increasing and decreasing elevations is not equal, so that there is a net elevation decrease. Is that correct? If so, perhaps rephrase to make it clear.

Agreed. We will rephrase, and expand significantly more on the volume turnover in the circles as far as can be learned from our measurements (see responses (32)).

(6) Block #6059, lines 11-28: This is an example where perhaps local factors may be responsible for differences in the displacement patterns between the middle and the northern circle. I am not implying that this is necessarily the case, but I think the authors should discuss it (and discard this possibility if they don't think it is not plausible.

Absolutely agreed. We will discuss this (see comment 3).

(7) Technical corrections

We agree with all suggestions, and will change the manuscript accordingly.

Comments by Bernhard Hallet (italic):

(8) … What these (findings) represent for the longer term for the dynamics of active sorted circles, and what causes these significant changes over a few years are open questions. They can be addressed through additional photographic surveys on both shorter and longer time scales, respectively, to document the large changes that occur during individual warm seasons, and to define the longterm changes. Additional, ground data on the subsurface spatial variation in soil texture, soil thermal evolution (including active layer depth variation), moisture conditions, and heaving & settling would be needed to shed light on the causes of the observed changes.

See also (1). We absolutely agree and will make clearer in the discussion and outlook what type of measurements and data would be needed to go further from our study. In particular, we will relate our findings in a number of aspects to Hallet (2013). The latter study is crucial to better analyse and interpret our findings, but appeared just 3 weeks before submission of our study and was unfortunately not known to us at this time.

(9) Clarify what is known in an absolute sense about the changes in elevation and horizontal position averaged over your study domain over the three years. This is discussed in detail, but the technicalities are less important that your conclusions, which need to be articulated has clearly as possible. Whether there is absolute subsidence is im- portant in relation to your discussion of the active layer getting deeper. Interestingly, I don't know of any soil temperature data showing this but documenting overall subsi- dence would argue strongly for deeper thaw of ice rich permafrost. Absolute motion to the N-NE, on the other hand, would not be surprising in view of the overall slope of your study area (down toward the stream and lake).

We will discuss these absolute changes in a bit more detail, but the bottom line is that the cm-accuracy we achieved for absolute georeference appeared to be not sufficient to detect such absolute changes. This is clearly a 'lesson learned' that we didn't anticipate well enough during the field work. See also (4) and (10).

(10) Can you infer anything significant about the growth of the circles (increase in diameters of the inner domain and outer ridges) and a corresponding systematic decrease in the intervening areas.

We will discuss that shortly as we in fact tried to detect such changes. The bottom line is that the definition uncertainty of the boundary between fine domain and gravel ring turned out to be much higher than potential changes of this boundary (which is the clearest type of boundary in the system) within 3 years. Also, horizontal displacements turned out to be governed by local spatial variations with potentially limited representativeness for overall long-term changes in the circle forms. In sum, 3 years of measurement interval and an absolute georeference at cm-accuracy turned out to be not sufficient to detect overall absolute changes.

(11) - Refer to Washburn's paper that show displacements increase radially in the fines, but that increase cannot continue otherwise circles would be growing at cms/yr, which would make it very unlikely to find discrete circles like yours that have yet to coalesce.

Agreed. See also our new computation of strain rates (32).

(12) Provide more information about the surface and terrain characteristics, including proximity of a body of water, of the Janssonhaugen site of the ground temperature data presented.

Agreed. Will be done. See also (3).

Bernhard Hallet provided also annotated manuscripts, and we summarize here all substantial comments. We gratefully accept all technical, grammar and terminology corrections and minor suggestions, if not otherwise stated below.

(13) In my view, the feedback between texture, heat flow and freezing front geometry is the most important second ingredient of the model (and Kessler named at least one other). To this day I remain skeptic of the lateral squeezing.

This is a good and important point that we will stress in a revised version of the model description. We will also touch upon the lateral squeezing of the gravel rings, but our measurements do not support or reject this hypothesis. The relative surface lowering (Fig 5 of manuscript) together with longitudinal horizontal compression (see response 32) on the inner slopes of the gravel rings could both be due to long-term lateral squeezing of the gravel rings or short-term effects related to thaw subsidence or material submergence.

(14) If GNSS this is different from differential GPS, please explain the difference; if there is no difference use GPS.

The US GPS is one out of several Global Navigation Satellite Systems, but there are others. In particular the Russian GLONASS satellites are also important at high latitudes, and also used by our receivers. We prefer thus to stick to GNSS but will clarify.

(15) Lines 146-163 (and 223-229) could be deleted with little science loss.

We prefer to keep, but will try to shorten and clarify why we keep: we would like to describe what we did in terms of absolute georeference and that this (in principle high accuracy set-up) was still not sufficient. This is an important 'lesson learned' for us or others to continue with such measurements because reaching a significantly better georeference accuracy involves significantly more survey logistics, analysis effort and even more or different high-precision instruments.

(16) On using the gravel rings as overall relative horizontal reference for the southern circle: I hope this stability will be supported by measurements in part because of the occasional cracks that manifest large displacement near ridge tops (including those in fig 3)

We will describe better what we did and the potential impacts of uncertainties on our results. Basically, we based the co-registration on a large number of points and thus their mean displacement.

(17) On three-dimensional soil motion on the surface: This question comes up because, in my mind, particle motion implies the motion of individual particles (in an absolute sense, or relative to the soil or to other particles). If the particle motion turns out to be spatially coherent, I'd probably call it soil heave or settling. I suspect that your technique does not really image the soil; hence you may not wish to elaborate on this here.

Correct. Our method is able to track individual mineral grains large enough to be detected at the image resolution. For this study, the soil matrix (and its motion) is not detected. In frost processes, stone motion might not exactly represent soil particle motion. Agreed. We will modify.

(18) On lowest point in measurement area: How do they compare to the areas beyond the outer gravel ridge (which I called inter-circle areas)? It looks like your 4th and 5th northern-most control points in fig 3 are in these areas beyond the outer ridges

The outermost parts of the fine domain are in fact by far the deepest in our coverage, almost 10 cm deeper than the deepest parts of the inter-circle areas. We will describe and try to visualize. We will experiment with contours on top of the hillshade Fig 3 of the manuscript. See also below figure.

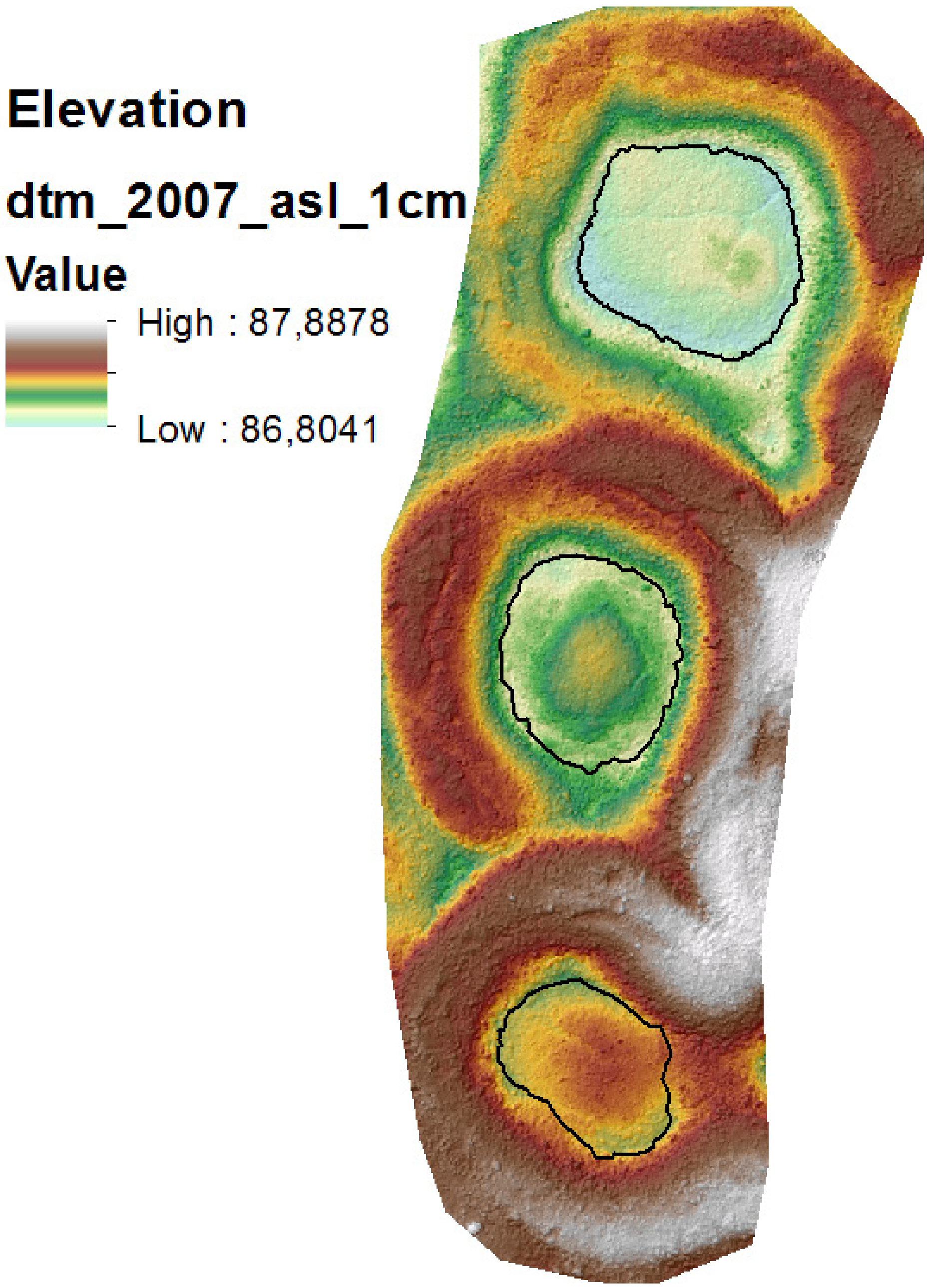


Figure: Colour-coded elevation

(19) Shouldn't you comment on this (the elevation difference between the circles) being due to the regional gradient, sloping down toward the lake, or is it different?

Completely agreed. See also (3).

(20) *Indeed, and the cracks are much more variable in the location and orientation than I realized. … Here or later it would be good (& helpful) to clarify the relationships between the crack orientation and location and the local slopes & micro-relief.*

We will elaborate a bit more on the cracks and try to visualize them better. We did not find, however, a clear relation between crack location & orientation and slopes and micro-relief. There are two types of cracks, those on the gravel ring tops, and those on the fine domains. Both are on rather flat sections, not on the gravel ring slopes. Both are mostly in areas of divergent flow (32). The orientation of cracks on the gravel rings is perpendicular to the direction of extension. There is no such clear relation on the fine domains, where some cracks are perpendicular to the direction of extension, some not. This could be a hint that in particular the horizontal 3-year displacement field on the fine domains is less representative of average long-term displacements.

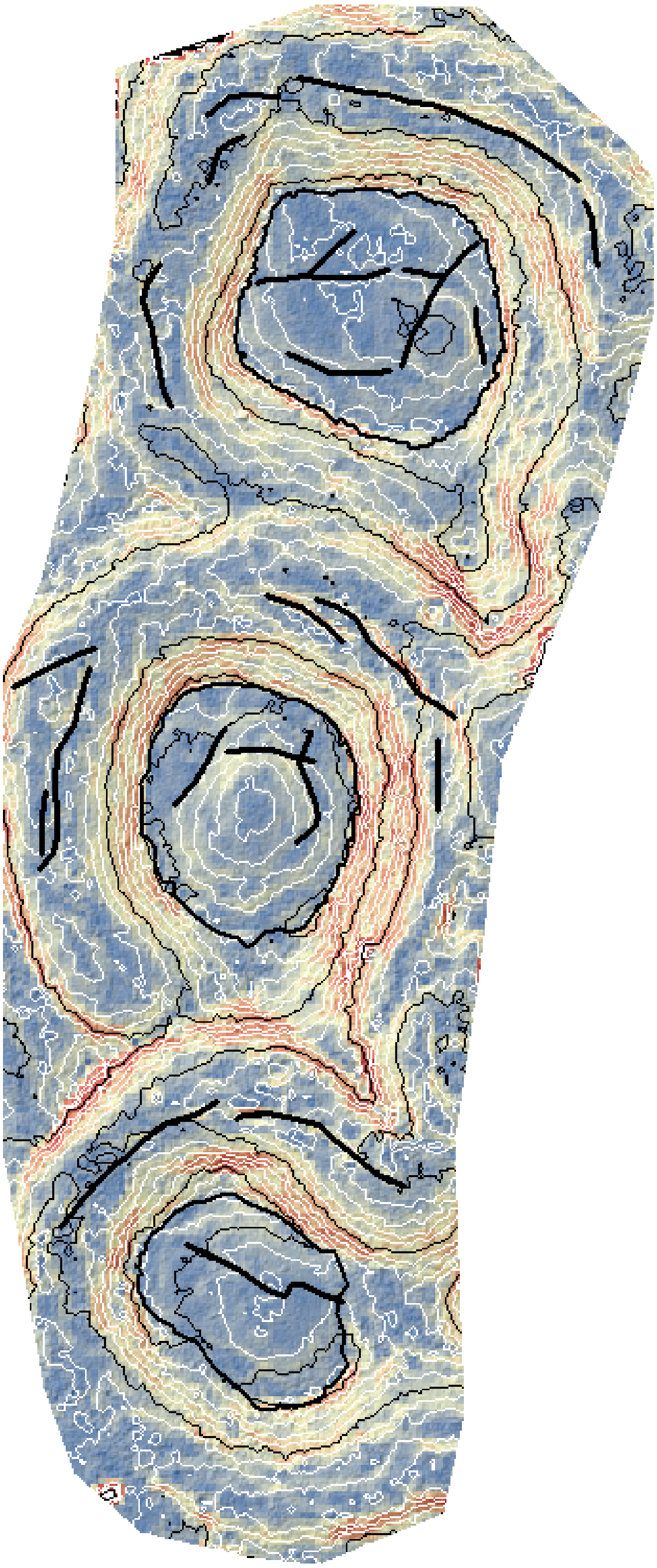
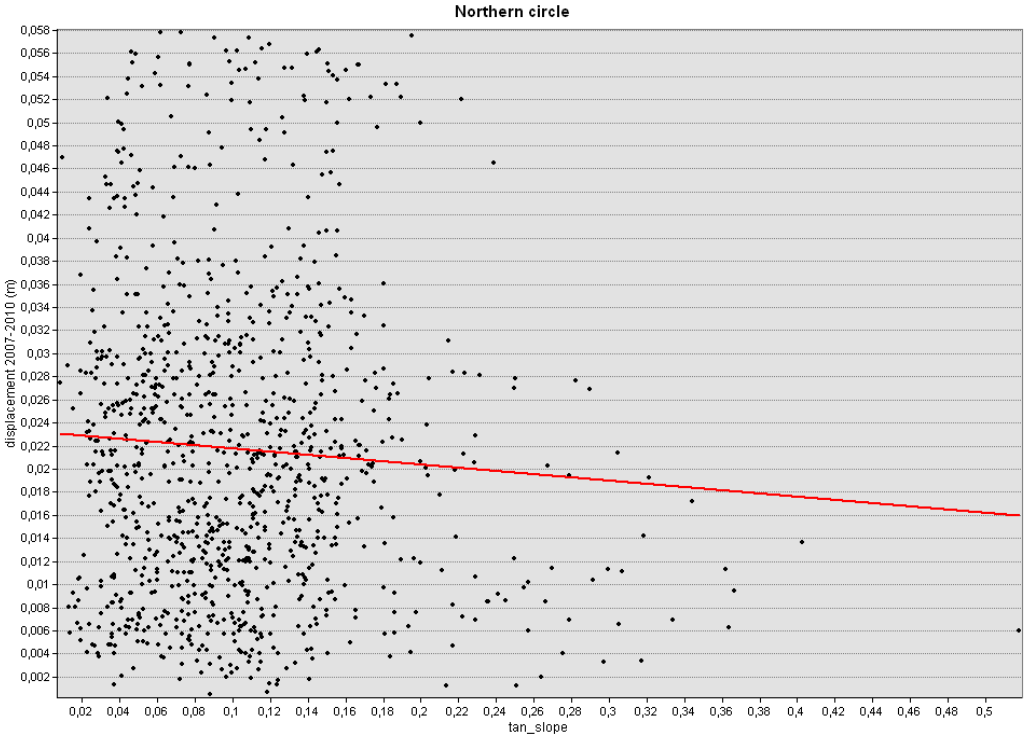
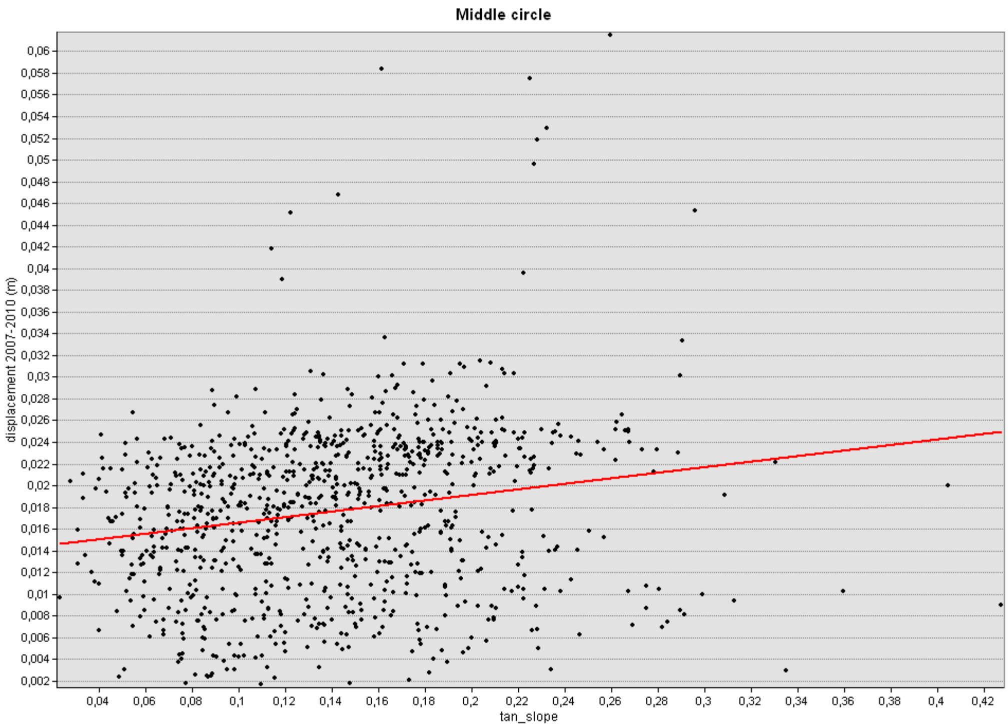


Figure: Slope (blue: flat, red: steep), contour lines (white: 2cm, black: 10cm), cracks (thick black), outer border of fine domains (thin black).

(21) It would be very instructive to show the plot of speed and slope (tangent or sine) for both the inner domain and the gravel ridge. This would not only illustrate what you describe in the text but it would show the reader the scatter in the data and the strength of the linear relationship (through an r-squared value).

In the two scatter plots below we show for the northern and middle fine domains the relation between horizontal displacement and slope.





Figures: Displacement 2007-2010 (m) vs. tan_of_slope. Northern and middle circle.

In the figure below we show the spatial distribution of displacement / slope.

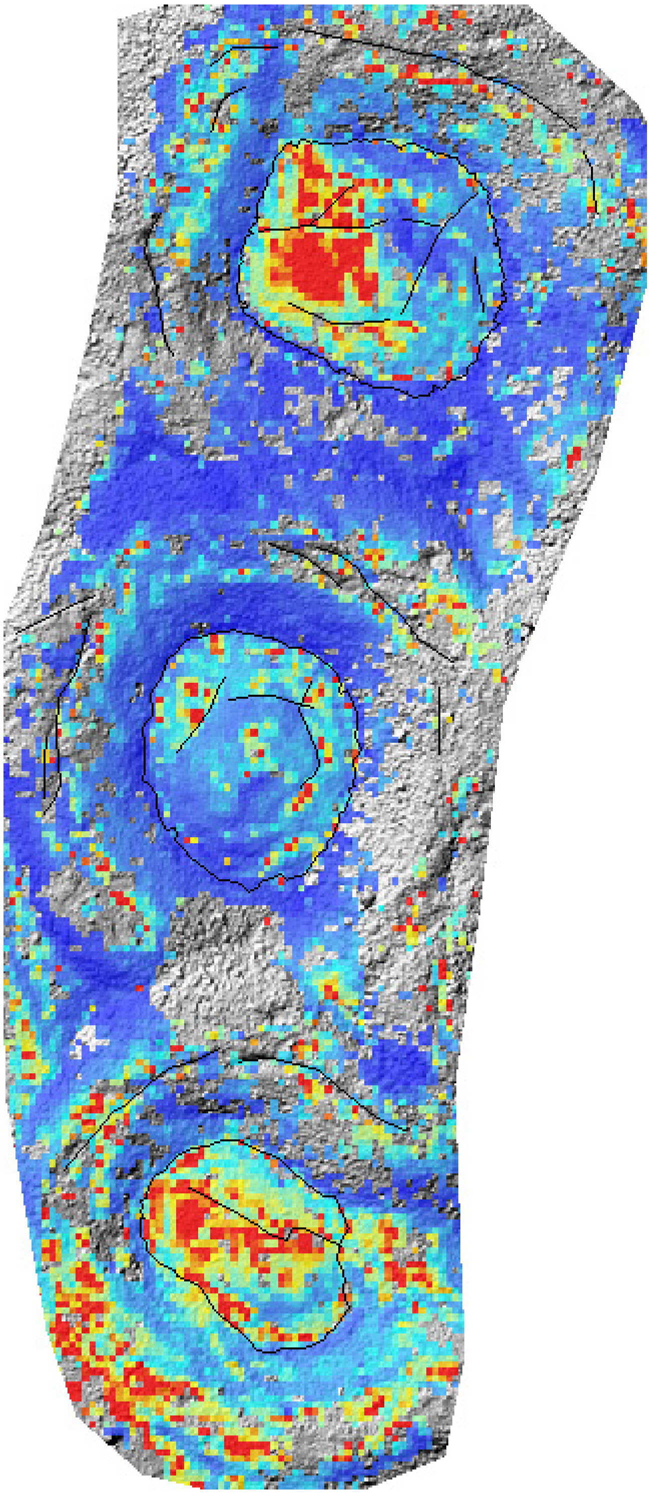


Figure: Displacement (cm/yr) / tan_of_slope. Blue: around 1, Red colours: >= 20. Typical values on the inner slopes of the gravel rings: 1-3; turquois colours on the fine domain and the ring tops around 7; red colours between 20 and 50.

All these above figures show in our view that the relation between slope and displacement magnitude is very weak, and in particular for the fine domains almost absent. (For this reason we even don't give R^2 of trends). This confirms that, at least over 3 years, gravitation alone cannot explain the motion pattern found. We will elaborate on that a bit more, likely adding the latter figure (perhaps as supplement).

(22) It would also be good to show that these displacements are "roughly towards the direction of steepest descent"

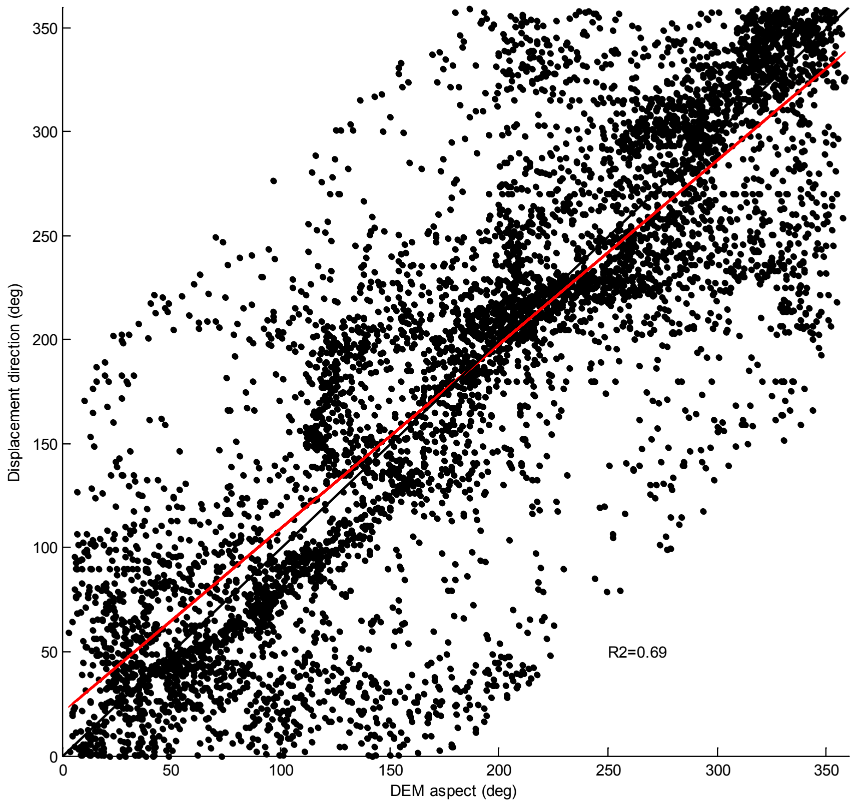


Figure: Displacement direction vs DEM aspect, middle and northern circle. Trend (R^2 = 0.69). The missing data to the corners are due to the ambiguity of differences that occur at the transition from 359 to 360=0 degree. Differences larger than 180 deg have been corrected to be smaller than 180 deg.

The above figure confirms the 'rough' agreement of displacement direction and aspect. We will consider to add such figure (as supplement?). The fact that displacement direction agrees well with aspect, but displacement length not very well with slope (21) can be interpreted as a confirmation that (as the referee pointed well out; see (27)) the surface material transport is only partially governed by gravitation directly, but rather by upwelling of material. Consequently, our findings seem to confirm that the soil dynamics govern the microrelief, not vice-versa. We will add this interpretation. Thanks for this remark!

(23) Please quantify the increase in relief between 2007 and 2010 if you can do so simply. ... I would say that the microrelief is clearly stable over decades and longer time scales based on much more than an assumption, it is consistent with measurements spanning a decade or more, and the simple observation that you mention (Hallet & Prestrud 1986). My hunch is that your rich results suggest that on shorter time scales of seasons to several years the microrelief changes quite a bit but is systematically regularized as the years pass.

We will quantify and clarify what we meant, i.e. the elevation difference between the outermost parts of the fine domain, the top of the gravel rings and the centres of the fine domains. We will stress that our related 3-year results are not in line with the stability of the microrelief over longer time scales. (See also in our Discussion).

(24) Interestingly, both of these items may each contribute to the 2007-2010 increase in microrelief "the degree day sum at the time of photography was larger in 2007 than in 2010.. and the extremely warm winter-spring of 2005-2006" because that for each season the microrelief decreases with time since thaw (snowmelt).

This is a subtle but important point that we will discuss in more detail. The point is that there is a certain temporal evolution (decrease) of microrelief over the melting season. In 2010 this might not have proceeded as far as in 2007. We discussed degree day sum in terms of potential for thaw of transient layer/ice rich permafrost, but will open up this discussion to also account for published data on microrelief development through a season.

(25) Relief-adjustment by erosion seems unlikely as I know of no sign of erosion by water by rain splash or runoff.

Thanks! Helpful observation! We will downplay.

(26) This should be restated as areas cannot constitute volumes

Agreed. See (5) and (32).

(27) This sentence serves as a reminder to present what you have learned about the relationship between displacement and slope, as you have the best data to date on this. Note I deleted or revised your "Further, in this model surface movement of soil is proportional to the local gradient. Accordingly, increasing surface relief will increase surface movement and therefore amplify the convection cell-like soil circulation within the sorted circles." because of a couple of problems: 1) contrary to your suggestion, the relief and slopes are driven by the soil convection, and 2) faster surface movement impacts the relief by increasing the divergence in flux of material causing highs to subside and lows to rise. This is complex; best to streamline this section.

Agreed, see (22).

(28) I do not recall this being mentioned earlier, hence more explanation is needed. Are you averaging velocity vectors over the whole domain photographed, over the three circles, or what? Isn't the net transport simply down hill toward the water?

The overall displacement is actually away from the water, c. 2 cm to the south. But this average is governed by local 'high-speed' areas, and does thus not reflect any long-term overall displacement of the circles observed. We will mention that.

(29) Do you NOT see convergence between the other two circles? Please clarify, whether the stones are converging or the ridges are converging (with the inter-circle areas diminishing and the circle growing in diameter)?

See (10). We don't see any significant change in the fine domain diametres. Also, we don't see any clear horizontal convergence over the gravel ridges. In our view, local short-term changes over our 3 year observation period are simply stronger than some of the overall long-term changes expected, as is one of the main conclusions from our study. We will shorty mention that we cannot see convergence between circles.

We also had some additional communication with Bernhard Hallet on our work outside of the formal review process. As we value these comments also high, and plan to incorporate them to the extent possible, they are summarized below.

(30) I really like your Figure 9 and think you should use it earlier in the paper. Some of your arrows trouble me, however. I can believe that a few of your pixels show upslope motion, but I would not include a black arrow pointing upslope (right of center) unless you are sure it is representative, in which case I would help the reader find the corresponding spot on your displacement maps. I am a bit confused by your while arrows because you define them as showing surface elevation changes, and yet the smaller ones also seem to represent lateral divergence across cracks.

These are careful observations on the Fig. and we agree that it should be more precise. We will revise the Fig. accordingly and adapt it also to the revisions of the paper.

(31) Fig 6 in Hallet (2013) illustrates a common observation that radial velocities increase outward from the center but start to decrease before you reach the contact with the gravel border. It would be neat to see whether you see the same tendency.

Yes, we can see the same at many places. We will add a figure, or modify an existing one to include the spatial pattern of surface speeds. The below figure is just a draft. Figure caption, legend and detail description to follow. Blue to red colours indicate low to high displacement magnitudes.

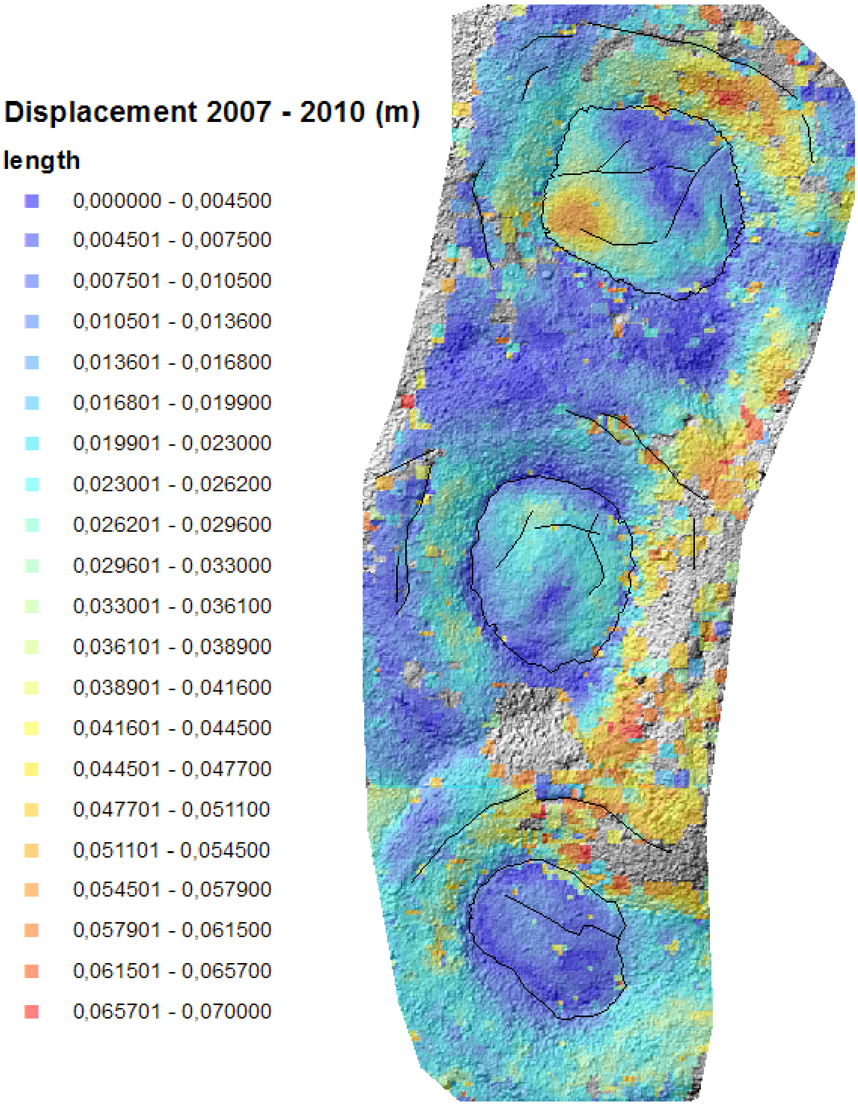


Figure: Displacement magnitude. Displacement vectors have been filtered for erroneous measurements according to Heid and Kääb (2012).

(32) Infer spatial patterns of soil convergence and divergence that you could relate to your topographic concavities (upward) and convexities. In turn, these would enable you to quantify spatially varying rates of vertical soil motion required to sustain the relief in a steady state. Whether the relief varies significantly on longer time scales would add motivation for you to continue along this most promising research line. These tilts give you some information on the subsurface that will complement your surface measurements very well, and that permit soil (and carbon) fluxes to be estimated from surface displacements. This brings up one idea that I didn't see in your paper, as I scanned it, that would permit soil fluxes to be estimated just from the microtopography. Obviously, this has important implications for rates of soil burial and exhumation. If you have not done it already, you might find it very interesting to explore the relationship between two of your data sets: the local slopes from your DEM and the surface displacements. I suspect that your much more robust data set would better define the type of trend that I show in my Fig 7b of the attached. Knowing the surface displacement, the soil flux can be estimated with the help of the tilts and depth of vertical dowels that I report for the Kwadehuk circles.

Below draft figure shows the sum of the horizontal strain rates (1000 / 3yr) (legend will be changed to 1/yr). Blue indicates divergence (horizontal extension) and red convergence (horizontal compression). I.e. for a stable micro-relief, vertical soil transport upwards has to compensate for divergence and vice-versa. For instance, a value of -300 below indicates a divergence of -0.3 / 3yr or -0.1/yr, and would, for imcompressible material, have to be compensated by 1 cm/yr vertical material transport for a 10 cm thick layer (vertical strain), or 2 cm/yr for a 20 cm thick layer (Hallet, 2013).
We will add the below figure and describe the pattern of horizontal convergence/divergence. We did some estimates of vertical soil fluxes **for the fine domains** based on the horizontal strain rates, that are summarized in the below table. We will include a summary of these estimates in the manuscript.

	2	*3*	*4*	*5*	*6*	*7*	*8*	*9*
Fine domain	*Total area (m2)*	*Area of horizontal compression flux (m2)*	*Mean compression (1/yr)*	*Corresponding vertical soil flux* per cm thickness of the horizontally moving layer (cm3/yr/cm)*	*Area of horizontal extension flux (m2)*	*Mean extension (1/yr)*	*Corresponding vertical soil flux* per cm thickness of the horizontally moving layer (cm3/yr/cm)*	*Total strain rate over fine domain (1/yr)*
northern	2.9	1.3	-0.029	-380	1.6	0.027	432	0.003
middle	2.4	1.1	-0.022	-242	1.3	0.022	286	0.001
southern	1.6	0.9	-0.021	-189	0.7	0.019	133	-0.004

Note: horizontal extension (divergence) at the surface is an indicator for upwards material transport (+ positive sign), and vice-versa (- negative sign), * under the assumption of stable micro-relief and incompressible material.

The above table tells that

- Average horizontal extensions or compressions on the fine domains are 2-3%;
- The soil fluxes in each fine domain are, under the assumption of a constant depth of the horizontally deforming top layer(!), roughly in balance (see columns 4 and 7, or 9);
- The northern fine domain has the largest soil turnover, the southern the smallest;
- For a 10-cm [20-cm] deep horizontally deforming layer, the northern circle has a soil volume turnover of c. 4000 [8000] cm3/yr, the middle on of c. 2500 [5000] cm3/yr, and the southern one of 1500 [3000] cm3/yr. On average over all three fine domains, these numbers are equivalent to a vertical soil volume turnover of c. 0.12 [0.25] m3 per m2 area and per century;
- For a 25-cm thick horizontally deforming top layer and a total depth of convecting soil of 1 m (Hallet, 2013), we estimate an average cycling time of soil of 300-400 yr, slightly lower than previous estimates (Hallet, 2013).

Due to the more erratic movement of stones on the gravel rings, strain rates cannot be computed reliable enough for significant parts of them. Based on some sections, we estimate however a similar rate of volume turnover compared to the fine domains, so that above estimates for turnover per m2 and century hold roughly also for the entire circle areas observed.

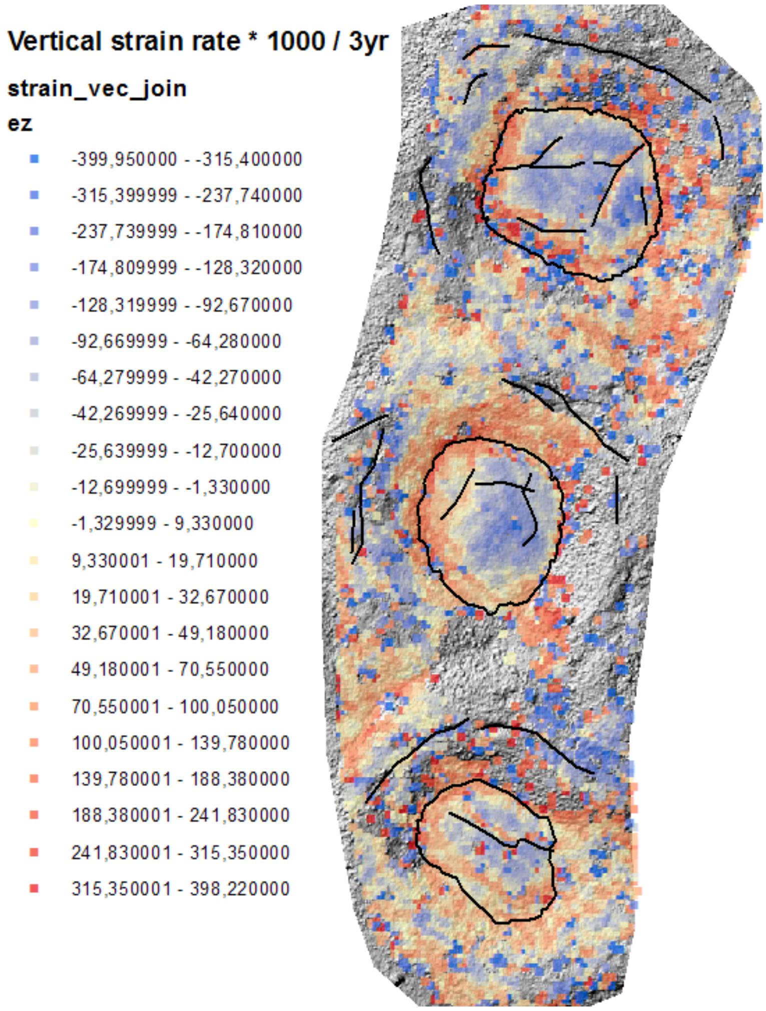


Figure: Sum of horizontal strain rates (*1000/3yr). Negative numbers indicate horizontal extension, positive ones horizontal compression.

References

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Heid T. and Kääb A. (2012): Evaluation of existing image matching methods for deriving glacier surface displacements globally from optical satellite imagery. Remote Sensing of Environment, 118, 339-355

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