The Cryosphere Discuss.,
doi:10.5194/tc-2016-1-AC1, 2016

The Cryosphere
Discussions

Interactive comment on "Frozen debris lobe morphology and movement: an overview of eight dynamic features, southern Brooks Range, Alaska" *by* M. M. Darrow et al.

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Received and published: 5 April 2016

Response to Reviewer 1 (Dr. Haeberli) for manuscript TC-2016-1, "Frozen debris lobe morphology and movement: an overview of eight dynamic features, southern Brooks Range, Alaska"

Dr. Haeberli, first of all, thank you very much for your kind overview and constructive comments. Many of them serve as a teaching tool, bringing some relevant references to the authors' attention. In what follows, your initial comment is presented, followed by the authors' response.

Abstract, line 18: Information on rock strength is certainly interesting. In the present

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case, more important, however, would be information on the strength and creep properties of the moving material. A recent review on such questions is provided by Arenson et al (2014). Many of the same authors from this paper presented information on the strength properties of the soil from FDL-A from frozen direct shear tests in Simpson et al. This article has been accepted but is not yet published; a PDF of the draft is available at this link: http://eeg.geoscienceworld.org/content/early/2015/11/04/EEG-1728.full.pdf+html?sid=0019d85f-9d60-4481-a3d9-21bc44167e04. We did not include the strength properties here so as not to repeat the previous paper. We have not run creep tests on the material.

Abstract, line 26: True but acceleration seems to be predominant – a phenomenon which parallels the recent trend to increasing flow speeds observed on Alpine rock glaciers (see discussion in Deline et al 2014). A completely synchronous development is hardly to be expected as thermal conditions are not the only factor influencing flow velocities. The text has been revised to reflect the spirit of this comment: "Analysis of historic imagery indicates that movement of the eight investigated FDLs has been asynchronous over the study period, and since 1955, there is an overall increase in movement rates of the investigated FDLs."

Page 2, Line 7: Better use "global warming", "atmospheric temperature rise" or so instead of "warming climate". The term "climate" is defined as a statistical average of meteorological conditions and as such cannot "warm" (the expression is popular but not really scientifically correct). Revised as suggested.

Page 2, lines 12-15: A more recent and excellent overview is given by Deline et al 2014. Revised the text to include related material from reference as follows: "Warmer temperatures lead to deeper active layer depths resulting in increased water infiltration; ice within the soil or debris melts, causing loss of soil strength, accelerated movement, and potential debris flows or total collapse (Deline et al., 2015, Geertsema et al. . ."

Page 2, line 30 to Page 3, line 1: The introduction of the term "frozen debris lobes" is an interesting step, especially as the term “rock glacier” has always been questionable (the corresponding phenomenon is neither a rock nor a glacier). In fact, the suggested new name could also be appropriate for what is usually called “rock glacier”. The aspect of movement should, however, also be expressed in the new nomenclature. This new term came about through the review of the paper Daanen et al. (2012) (I believe you were involved in that review). I spoke with Ronald Daanen about how to include movement into the term. These features move mostly by shear, with secondary or minor internal flow/creep. What would the reviewer suggest here? We are concerned about including too much process into the term.

Page 3, lines 5-22: The high subsurface ice content enabling steady-state creep deformation should also be mentioned (cf core drilling by Krainer et al 2014 and discussion by Arenson et al 2014). The term “glacier-cored” should be reconsidered carefully. It relates to a long-outdated geomorphogenetic speculation, which is hardly supported by adequate field measurements (geophysics, core drilling). Of course, buried massive ice can be preserved within permafrost. For simple size reasons, however, such buried ice is in most cases remains from ice patches, avalanche deposits or glacierets rather than real “glaciers”. Making a full stop after “Pleistocene glaciation” would help avoiding such discussions. This text was moved to the Discussion, and the last reference was revised as suggested. The remaining text was heavily revised, including a comparison to the data presented by Krainer et al. and a summary of movement through creep.

Page 4, lines 16-18: Where are the temperature data from? What depths and times do they cover? In which (lower, upper) parts of which FDL were they taken? And to what sites in the adjacent permafrost were they compared? This important information should be precise. This data is included in the Simpson et al. paper, and so again, we avoid repeating that content here. We have updated Figures 2a and 7b (now renumbered to Figure 5b) to show the location of the 2012 borehole in FDL-A in which the temperature measurements are taken. We also revised the text to describe more about the measurements and when they were obtained (which changed the values initially provided): “The significant movement within the shear zone severed the instrumentation approximately one month after its installation; however, we are still able to collect subsurface temperature and movement measurements from the upper 20.6m of the M-IPI. Temperatures measured from 15 to 20.6m from January 2014 through August 2015 were stable at -0.85°C, whereas the temperature of the adjacent permafrost at 3m from the same time period was -2.1°C.”

Page 8, line 32: Should the high frost susceptibility of such silty sand be mentioned here? It could be a key factor concerning subsurface ice content and creep mode. Based on field observations, I do believe that the debris lobe soil is frost susceptible; however, we have not yet performed frost heave tests on the soil. I also believe that the soil has a significant unfrozen water content, but this is also speculation as we have not yet tested it (there are plans to do this in the near future). While frost heave may contribute to the creep/flow component, this mode of movement is secondary to the tremendous shear that these features experience. Added the following lines to illustrate this point within the new Study Site and Background section: “Sub-surface measurements within FDL-A indicate that this frozen debris lobe moves predominantly through shear in a zone 20.6 to 22.8m below ground surface (bgs), with temperature-dependent internal flow as a secondary movement mechanism (Darrow et al., 2015; Simpson et al., in press). For example, between September 2012 and August 2015, FDL-A moved 13.8m through shear and only 1.9m through internal flow, for a total displacement of 15.7m at the main borehole location.”

Page 9, lines 6-7: Can more information be given on this drilling? How representative is the information on extremely small ice contents? How was this ice content determined? Was melting of core-ice during drilling prevented by cold-air cooling or so? Were temperatures at depth measured here? Where was the exact position of this drilling? As previously mentioned, the drilling program and results obtained are described in greater detail in Simpson et al. (in press). We are concerned about repeating too much of that content in this paper. Revised the text regarding excess ice content as follows: "Boreholes from the 2012 subsurface investigation intercepted no massive ice, and all samples obtained from the drilling were ice-poor (i.e., samples contained no excess ice and volumetric moisture contents (averaging 31%) were less than the calculated porosity of the soil)."

Page 10, line 16: This occurrence of massive ice and ice-rich soil here seems to be in strong contrast with the extremely low ice contents found in the drilling on FDL-A. Is this contrast real, and if yes, can it be explained? Yes, it is real! This also was a surprise to us. The soil within the lobe is indeed ice-poor, at least in our boreholes. We try to explain that the massive ice forms in the cracks open at the surface (i.e., infiltration ice), which then become covered and thermally protected until re-exposed.

Page 12, lines 17-27: References should be made to the dated permafrost core through an active rock glacier described by Krainer et al 2014, which documents a similar evolution for rock glaciers in the Alps. The authors should also have a look at the concepts developed on the basis of core drilling and borehole measurements already in the late 1990s for rock glacier evolution over time (Haeberli et al., 1998). These concepts are comparable to the ideas presented here but provide more detail about flow physics and the internal layering of the creeping body. They especially also consider the phenomenon that material from the more rapidly moving surface falls down over the steep front and is then overridden by the more slowly advancing lower parts of the front. This text has been significantly revised, to include the suggested references.

Page 13, line 16: Again, compare with rock glacier datings (Krainer et al., 2014 and other references provided). After reviewing the Krainer et al. (2015) paper, we are looking at two different things. That reference discusses the continual accumulation of the rock glacier and periods of permafrost instability in the past. It indicates a continuous stratigraphy within a small feature. What we are trying to communicate with this date is a time when FDL-A may have started to move out of its catchment and move quickly downslope. This is after its growth period within the catchment (similar to the reference). In fact, it would be difficult to reconstruct a similar chronology, since the lobe geometry is dramatically different now from when it was smaller within the catchment.

Page 13, lines 28-30: The possibilities of geophysical soundings could be mentioned here and primary results from such measurements on rock glaciers could be summarized. We have experimented with a few geophysical techniques already. Seismic refraction did not get us deep enough with the available equipment to discern the shear zone, and we were not able to drill where the seismic lines were located, thus not ground-truthing the results. We also tried the passive seismic method, but this method was unsuccessful since the subsurface layers are either thin or have similar seismic properties. We do intend to try an induced electromagnetic method this year. We anticipate that Induced Polarization Tomography (IPT) will be most successful to penetrate to the depth of the shear zone, and to locate water within the lobe; however, its employment depends on funding. The text was revised to include the references on geophysics.

Page 14, lines 6-7: Why are debris flows increasing the surface temperature? Provide a brief explanation of the physical process involved. This text was revised to: "The meltwater forms debris flows that cover a larger area of the lobe, changing the moss-covered surface to bare mineral soil, which increases the surface temperature and repeats the cycle…" The debris flows change the surface thermal regime, eventually causing a shift in the vegetation.

Page 14, lines 10-12: Ikeda et al. (2008) document and discuss detailed field evidence on this process chain from drilling and borehole measurements. Added the following text to a different but also relevant portion of the Discussion: "Ikeda et al. (2008) document a similar process in a rock glacier in the Swiss Alps. In the rock glacier, movement formed tensile cracks, allowing snow melt to penetrate into voids, decreasing effective stress and increasing movement rates."

Page 15, line 16: Better write "…study of eight FDLs near the Dalton Highway in the

Brooks Range, which. . .”for readers who primarily look at the conclusions. Revised as suggested.

Page 16, line 25: In view of the still strongly limited temperature data and the evolution in time, it could be more appropriate to write: “. . .movement changes which may be tied to changes in air temperature.” (The movement itself is not tied to air temperature in a straightforward way but rather via a complex process chain). Revised as suggested.

Page 16, lines 29-30: Concerning geophysical soundings and drilling refer to the general comments at the beginning of this review. See response above to related comment. This text has moved into the Discussion section. Added suggested method to this sentence.

Caption of Figure 1: Is there only one blue rectangular inset in (a)? Yes, the rectangles are small, but there are two (one to the north for the (b) frame, and one to the south for the (c) frame). They tend to merge together because of the scale. If you increase the size of the image, you can see two.

Caption of Figure 5: What exactly is meant with the term “deflation”? This term usually stands for erosion by wind. Is this meant here? Here we did not imply erosion by wind, but rather how a hot air balloon may deflate. We tried to describe the center portion reduces in thickness and flows out the middle (like a tube of toothpaste) while the sides remain relatively intact. Is there a more appropriate geomorphic process term that should be used to describe this?

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