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The Cryosphere Discussions

Interactive comment on "Ribbed bedforms in palaeo-ice streams reveal shear margin positions, lobe shutdown and the interaction of meltwater drainage and ice velocity patterns" *by* Jean Vérité et al.

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This manuscript presents results from laboratory experiments, whereby bedforms are produced in fine sand under a small-scale 'ice sheet' analogue model made of silica gel using different water injection scenarios. The authors then compare results against real landform assemblages observed at three selected natural sites. One key aspect of these experiments is the ability of the gel to deform, in response to the water injection, in a way that produces a corridor of flowing gel that resembles an ice stream; bedforms in the sand are produced in the process. The authors thus make the case

that their analogue model is a scaled-down representation of an ice stream and that the landforms produced in the sand under the deforming silica gel are proportional and directly comparable to landforms associated to the ice stream landsystem. All the observed analogue landforms are described and mapped, but the analysis and interpretation focus on the oblique to transverse ridges, which are here considered analogous to ribbed moraines. The authors then find several similarities (e.g., morphometry, orientation, location) between the features produced by the analogue model and the features observed at the selected sites. The authors then suggest this finding has important implications on our understanding of ribbed bedforms in the specific context of ice streaming (i.e., how, and where they may form under ice streams), and that this work provides new criteria for palaeo-glaciological reconstructions of ice streams.

Analogue modelling is not new to the geosciences as it has been extensively used in the field of geodynamics to study folding and faulting, as well as to investigate larger scale problems in tectonics. To my knowledge, its application to ice sheet dynamics is quite novel, and based on the references cited, it was first applied just a few years ago to investigate tunnel valley formation. The experimental setup is interesting and seems to be done according to the state-of-the art. It would be useful to provide more details about scaling and how exactly the analogue model is similar in terms of geometry and dynamics to the natural system. The authors simply refer to earlier studies, but I think it is important given this is still relatively new to clearly explain scaling in this case (i.e., ice lobe/stream, landforms). It is also difficult to understand how a single lobe or stream is initiated from a central injection of water under a circular and uniform convex-up silicon cap. Why does it lead to a single lobe in one specific direction? I think an explanation about how the injection induces discrete deformation and movement along one specific direction in the silicon cap would be useful.

Analogue modelling is used for its advantages, which include 1- an ability to observe certain processes from beginning to end, 2- control the parameters to study different scenarios and, 3- record changes and map new features at any point in time during

the experiment. Analogue modelling can help scientists develop new ideas and help test some hypotheses. Therefore, I think they can be useful and the work presented in this manuscript is valuable and opens up new and interesting research avenues. However, any type of modelling is a simplification of the settings and processes that are taking place in real systems. Direct use of analogue modelling results to interpret natural phenomenon is a risky exercise and must be done with great caution. My general impression is that the authors make a direct link between the analogue and the natural cases too quickly, without properly explaining the main assumptions and the possible limitations of their model and of such comparison exercise. They do mention a few limitations later in the discussion (e.g. such as near line 555), but this should be more comprehensive and presented earlier. A full list of model simplifications and limitations, as well as assumptions for the comparison to natural phenomenon should be provided in the methods section. For instance, it seems the modelling ignores thermal effects. Lateral shear margins are not just wet/dry boundaries, but they are also thermal boundaries and several thermal/hydrological effects have been investigated and modelled (e.g. Haseloff et al. 2015, 2018; Meyer and Minchew 2018; Meyer et al. 2018).

Here is a list of more specific questions or problems that I think need to be addressed:

1) Four selected areas within only three paleo-ice streams represent a small sample to be confident about the degree of similarity between the analogue model and the real landsystems. The natural sites appear to have been based on a 'search and find features' strategy. I understand the rationale of doing this, but it introduces possible bias that may have an impact on the analysis and conclusion. This limitation should be acknowledged and discussed. It would be useful to identify some strategies to further assess the validity of the comparison exercise.

2) In the analogue model, ribbed bedforms developed obliquely to the flow direction near the lateral margins. Similar oblique features are also described such as near the lateral margin of the Amundsen Gulf ice stream. The authors cite previous work that

had interpreted these features as palimpsest glacial lineations; that is, older drumlins that got overprinted and ribbed following a shift in ice stream configuration. Considering the results of their analogue modeling experiments, the authors propose a new interpretation (see discussion near line 510), which is that these oblique lineations formed under the same ice stream configuration than for the younger glacial lineations that crosscut them. The crosscutting relationship between these features clearly indicate the oblique ribbed bedforms must have formed at an early stage. Furthermore, this two-stage process also brings the question of preservation. Are they observed near lateral margins because that is where they formed as oblique ribbed bedforms, or because older drumlins were better preserved there (i.e., only partially overprinted) due to lower flow velocities and patchy overprinting? It is an interesting idea to suggest they may have formed during a single phase (it would require at least a two-stage process) without any change in the configuration of the ice stream, but it remains to be tested. I would argue that there are more of these oblique ridges than shown on their figure 12b because the degree of overprinting and reworking increases toward the center of the trunk ice stream. So, they seem to have covered a wider portion of the bed than shown in Fig. 12b. Adding the water bodies could help visualize this better because some of the swales in-between the ridges have elongated lakes in them. The authors do recognize that some of these interpretations are preliminary and could be further tested (see lines between 530 and 533). Detailed ice flow reconstructions using independent measures like striations would help test these ideas. In summary, if the oblique bedforms formed in an earlier phase and were overprinted and drumlinized later by the streaming bed, their spatial distribution could reflect more the area of better preservation potential (erased more in the middle of the trunk than on the lateral edges). Are they ribbed bedforms from a single phase or palimpsest/overprinted drumlins turned into ribbed bedforms following a shift in ice stream configuration? I think the question remains open in my opinion.

3) Based on my above comments, I think it is premature to conclude that we now have new criteria for palaeoglaciological reconstructions.

4) The link to abrupt spatial variations in subglacial shear stress/basal drag/drainage has been proposed in previous models of ribbed moraine formation; perhaps not in the specific context of ice streams, but as a general process (i.e., ribbed bedforms develop under sticky areas of the bed in the presence of pre-existing till). The authors do recognize that ribbed bedforms and abrupt lateral and down-ice transitions with glacial lineations have been documented and interpreted to record large velocity gradients across the bed (near line 640). However, they say that these previous interpretations were only for very local sticky spots, which seems to suggest that they are of limited significance or that they could not apply to their case. I think that the ideas presented in this study are in many ways quite close to what was presented in these earlier publications. For me, this new study is interesting because it may provide a new way of testing these ideas. So, it is not so much a completely new explanation, but a new approach at testing previous interpretations, which does also seem to have the potential to bring new insights into processes. Another take home message is that there has been an emphasis on mapping and using flowsets in paleo-ice stream studies, but ribbed bedforms also provide key insights and thus deserve more attention because they can help understand the spatial patterns of sticky versus slippery portions of the bed, which is critical to understand ice stream dynamics and evolution.

5) This is more about the content and structure of the paper. I am wondering if the long section that reviews the paleo-ice stream landsystem is necessary. The rest of the paper focuses more on ribbed bedforms. Figure 1 is great; it is a high-quality conceptual model. So, I would suggest keeping that figure, but the text of section 2 could be considerably reduced. I think it would be sufficient to just summarize the conceptual model in the paper with appropriate references and use that space to present and discuss the assumptions, advantages, and limitations of analogue modelling for glacial dynamics problems. I think this is new/recent enough to justify more explanations.

This type of studies often brings more questions than answers, which is not a bad thing. It is hard to do innovative research, and I would thus like to commend the authors for

trying something like this. In summary, I think the results and the analyses are useful, promising, and will be of interest to the scientific community, but the authors must provide a complete list of assumptions and limitations and they also need to discuss interpretations and significance in a more balanced way.

Minor comments:

Line 34: Literature from the last 20 years is missing here.

Line 184: I agree with this. In most places I am familiar with, ribbed moraines tend to be variably overprinted/reworked, mostly by drumlins that have formed at a later stage (most cases).

Line 203: So, it is four processes; not three as listed above.

Line 214: I note this is the density of water at 20deg. C. Any implications for modelling ice-bed interface near the pressure-melting point?

Line 218: "...within the bed and along the silicon-bed interface"; in all directions or along one particular direction? Same question also for line 278...

Line 289: The feature is referred to as a 'delta' here, but it is not controlled by water level in a frontal water body. It would be more accurate I think to refer to it as a 'splay' or a fan.

Line 326: "similar"... Data would be useful here rather than having to rely only on visualization and terms like "similar".

Lines 515/16: That is excluding the possibility that on the lateral edges there could have been drumlins instead of MSGLs. I think without local information about ice flow phases (e.g. from striation data) the previous interpretation of palimpsest landscape cannot be eliminated.

Line 570: This is interesting. It would be important to link to the real case examples to support that statement.

Line 614-15: Yes, I agree with that.

Line 657: I would like to point out that long corridors or tracks of ribbed bedforms alternating with narrow corridors of drumlins occur in places like northern Manitoba and in mainland Nunavut. They form some kind of 'bar code' landscape (see Fig.1 and Fig.3 in Trommelen et al. 2014). The banding is probably too narrow and laterally repetitive to represent separate ice streams and ice stream margins, but it does suggest lateral and regular variation in basal stick-slip conditions.

Line 708: "development". Development or preservation? In real cases, they could be distributed like that because they were crosscut by channels. In other words, perhaps they were more widespread and laterally continuous in an early phase and then later crosscut/eroded by meltwater channels.

References: Haseloff, M. et al. (2015). A boundary layer model for ice stream margins. J Fluid Mechanics, 781: 353-387 Haseloff, M. et al. (2018) The role of subtemperate slip in thermally driven ice stream margin migration. The Cryosphere 12: 2545-2568 Meyer, C.R., Minchew, B.M. (2018). Temperate ice in the shear margins of the Antarctic Ice Sheet: Controlling processes and preliminary locations. Earth and Planetary Sci L, 498: 17-26 Meyer et al. (2018) A model for the downstream evolution of temperate ice and subglacial hydrology along ice stream shear margins, J Geophys Res, 123: 1682-1698

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