

Review of “Review article: The hydrology of debris-covered glaciers – state of the science and future research directions” by K.E. Miles et al.

General

In this manuscript, the authors aim to give a review of the state of knowledge of the hydrology of debris-covered glaciers and suggest some directions for future research. Debris-covered glaciers are an important topic of current research, since there are many open questions surrounding the response of such glaciers to changes in climatic conditions. A better understanding of hydrological processes is of particular significance due to the importance of downstream water supply from glaciated regions that contain a large proportion of debris-covered glaciers.

While I personally found many aspects of this article interesting, unfortunately I do not recommend it to be accepted for *The Cryosphere* in its current form, for the following reasons:

- 1) Good review papers are more than surveys of previous findings. They provide original insights and analysis that help to crystallize the most important concepts and current state of a field. However, this article reads more like a literature review than a review paper. There is no attempt to frame the discussion by presenting the key physical processes at the beginning of each section, nor is there a clear synthesis of the information at the end of each section. The choice of figures is also not so helpful, as many are photographs taken by the authors, which although original, do not help to elucidate the processes being discussed the way that clear schematic drawings would. These shortcomings limit the utility of the manuscript.
- 2) The manuscript does not clearly highlight the conditions under which hydrological features are expected to be different for debris-covered glaciers. In many places, the results of previous studies on debris-covered glaciers are presented without explicitly discussing whether they are consistent with similar results from studies of clean ice glaciers. This leaves a confusing impression, especially since one important goal of this review paper should be to link well-known results from previous studies of clean ice glaciers to the relatively new results for debris-covered glaciers.
- 3) The section on candidate areas for future research needs considerable improvement. Several of the items on the list are either not well connected to the rest of the article or else difficult to motivate as research priorities. Also, there is no discussion of how the research might be carried out, so the list appears more aspirational than practical.

Example problems

Pages 7-10 (Section 3.3 – Supraglacial ponds)

This section has a lot of good material on an interesting topic but it is poorly organized. No overall discussion of the physical processes is presented but rather bits and pieces that come up along the way. This leads to some confusion in the order of the presentation, as some controls on ponding (lines 265-280) are discussed before the main mechanisms of pond formation are outlined, which seems backwards. There is no

synthesis of the main ideas, nor are any open questions raised at the end of the section. The one figure chosen (Figure 4 on p.43 with caption on p.44) does not help the reader to better understand why/how supraglacial ponds form, grow, or drain.

As an example of a better structure for this and other sections, the authors might wish to consider one of the clean-ice hydrology reviews that they cite as a previous comparable study (p.4, lines 129-130), the clearest being the classic paper by Fountain and Walder (1998).

Pages 11-14 (Section 4 – Englacial hydrology)

An argument is made (line 436, p.11) that “a glacier’s thermal structure determines the water content of englacial ice...” but no studies presented have shown any difference in the thermal structure of debris-covered glaciers compared to nearby clean ice glaciers. Hence, it does not make sense to focus much on this and certainly not as the first main topic of the section.

There is substantial overlap between debris-covered and clean ice glaciers for this section but without a review of the state of clean ice englacial hydrological knowledge, it is challenging to understand what is important and why. There is only limited utility in a detailed discussion of debris-covered glacier observations that are consistent with those observed on clean ice glaciers and these can just be mentioned in passing.

The three formation mechanisms for englacial channels within DCGs (lines 509-542) would be better placed at the beginning of the section, as part of an overview of the key processes. A schematic figure of these mechanisms would go a long way to making this section clearer.

Pages 14-16 (Section 5 – Subglacial hydrology)

There is a very cursory and incomplete discussion of what might be different about the basal hydrology of debris-covered glaciers as compared to clean ice glaciers in lines 576-585 and there is no description of the key processes of subglacial drainage theory here. All of the studies on debris-covered glaciers summarized in the rest of the section (lines 586-638) could be equally true for clean ice glaciers. Although there are no conclusive studies to report, it would be still useful to discuss how differences in temporal evolution of surface and englacial hydrological processes, water storage located throughout the glacier, and the increased likelihood of finding basal sediment might alter expectations for the subglacial drainage of debris-covered glaciers.

Pages 22/23 (Candidate areas for future hydrological research)

Some of the items on this list are not clearly defined and in general, there is no explanation of how this research might be carried out, not even a brief sketch of how one might start to set up future work. It would be good to identify, for example, where existing data or models are weak or incomplete, how this inhibits current understanding, and what explicit steps future researchers should do to address this. For example: what to measure and how; what future theoretical or numerical models should include; and which papers might serve as a starting point for the work.

In addition, some other specific problems are:

Item 4: The focus on small-scale movement of water between ice crystals comes out of nowhere. It does not appear anywhere in the rest of the article and there is no citation, so it is unclear why this might matter or what previous studies have led to this idea.

Item 5: It is still not so clear why one should expect anything different for the subglacial drainage of debris-covered glaciers or how the authors envisage future work that could shed light on this matter.

Item 7: Long-term water delivery from debris-covered glaciers is intimately connected to mass balance, ice dynamics, and the response to climatic changes. It does not make sense to discuss this topic purely in a hydrological context. It is also unclear if future work is meant to be modelled numerically or studied observationally or both.

Item 8: “Research to understand the role of monsoon conditions, and its relationship to non-monsoon conditions, is therefore required” is confusing as this appears to be less about understanding debris-covered glaciers and more about understanding the effects of one particular climate’s local influences on all glaciers. Generalizing to local climate influences on global debris-covered glaciers is similarly unclear.