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Interactive comment on "Surface depressions (Lacunas) on Bering Glacier, Alaska: a product of downwasting through differential ablation" *by* P. J. Fleisher

Anonymous Referee #1

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This paper addresses a little-known subject, a rare class of supraglacial hollows known as 'lacunas'. It is potentially of some interest to those concerned with processes of ablation on surge-type glaciers. However, it is very poorly written and referenced, and the proposed mechanism of formation is unconvincing. The subject is probably worth pursuing, but this will require much more thought and study of the literature, and a thorough re-write of the paper.

Here, I comment on some of the major misconceptions and weaknesses of the paper, as pointers on how the re-write could be approached. It may also be useful to team up with a co-author, to help with interpretations and writing.

Introduction: at the outset, it needs to be stated who introduced the term 'lacuna' into the glaciological literature, and how it was originally defined.

Section 3. Glacier karst. This section is not well written or referenced, and it is unclear what point is being made. Is the author suggesting that sink holes in glacier karst are not of collapse origin? Or to define some specific relationship between lacunas and sink holes? Clayton (1964) is of course the classic reference on glacier karst, but things have moved on since then. See references in Benn and Evans (2010), p. 353, especially Kruger 1994, Kirkbride 1995, and Gulley and Benn 2007. The latter reference is especially relevant, since it reports observations within englacial conduits beneath stagnant ice, some of which had partially collapsed roofs.

line 89: this reference to saturation (and subsequent instances in Section 4) implies an incorrect conception of glacial drainage. The presence of englacial conduits does not imply 'saturation' of the surrounding ice any more than the presence of a system of pipes in a building implies saturation of the walls. The concept of an 'englacial water table' is incorrect, as almost all intact glacier ice is essentially impermeable. Consistent water levels in sink holes just imply that they are all connected via some open drainage system and have a common base level.

line 112: these arguments about 'increased saturation of ice' are inconsistent with modern conceptions of glacier drainage (see Gulley et al., 2009: Quaternary Science Reviews, and Benn and Evans 2010, chapter 3). The data do appear to show that fractures in the 'lacuna band' are hydraulically connected to the bed, but this does not imply saturation of the ice.

Section 5: the association of the lacuna band with the basal topography appears to be robust, although it could be stated more clearly and succinctly. There are a number of unsupported statements such as 'For all practical purposes [what does that mean?] ice held deep within the trough persisted in a passive and stagnant condition beneath overriding ice...'. Figure 6 illustrates the concept, but what is the evidence?

Section 6: The argument inferring ablation rates from the depth of debris in crevasses is not strong, and rests on a crevasse depth value plucked from a general textbook. The survey data referred to in Section 6.2 provide a firmer basis, and although it is probably correct that initial ablation rates on the crevassed surface were a bit higher, it is not possible to say by how much.

Section 7: The discussion of the effects of stagnation on ice density and 'saturation' is mostly unsupported speculation. How can there be 'ongoing intracrystalline deformation' in stagnant ice? if the ice is stagnant, by definition it cannot be deforming. And why should this 'compromise ice texture' and result in 'infiltration' and 'saturation'?

From the evidence presented, it can be said that there is a consistent association of the lacuna band with an area of fractured glacier ice likely occupying a transverse trough. It is possible that the fracturing influences patterns of surface ablation. Presumably, fractures allow drainage of meltwater into the ice, otherwise the lacunas would be full of water. How might the presence of open fractures encourage development of depressions? Does some kind of feedback process amplify ablation and lead to formation of the closed hollows? Are there any systematic differences in the density or characteristics of fractures in ice with and without lacunas?

With a more thorough, critical approach, it is possible that a sound explanation can be found for these odd features.

Interactive comment on The Cryosphere Discuss., 8, 2403, 2014.