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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 #2

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Review of tc-2014-32: Surface depressions (Lacunas) on Bering Glacier, Alaska: a product of downwasting through differential ablation P. J. Fleisher

This paper describes a zone of lacunas (basin-like melt depressions) that has formed after each surge of the Bering Glacier in the same location. The author offers a theory for their formation that suggests that a trough beneath the glacier holds stagnant ice, which over time saturates unevenly due to sub-glacial standing water. Once exposed by post-surge melt, differential ablation creates the depressions, the saturated and refrozen zones melting slower than the other zones.

The paper describes an interesting and curious phenomenon, and the writing in the

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paper is clear, even elegant in places. However the phenomenon described is limited to just a few glaciers, and perhaps the theory provided is germane only to the Bering Glacier. This topic really only warrants a short note. . .not a 22-page paper. I think shortening the paper would actual result in having more people read it. I also had some questions about the proposed theory.

So in summary, my recommendation is that this interesting paper that deserves to be published, not until it is reduced in length by about 35%, The author also needs to address to the extent possible whether the theory of formation is solid and if it applies to other glaciers.

a) How to reduce the length: The paper contains far too much detail about the Bering Glacier, including too many specific named locations, and an overly detailed listing of events. It reads like a report. Other than the group working on the Bering Glacier, this level of detail is not warranted and it doesn't really make the proposed theory stronger. My suggestion is to greatly shorten sections 2 through 7, and let the maps and photos carry much more of the story than the writing.

b) Too Specific: Lacunas form on other glaciers. How does the formation theory for the Bering Glacier apply to these others, or does it? Is there any indication the theory is general? For most readers, this is likely to be a key point of interest.

c) Is the theory correct? It seems like the paper establishes reasonably well that the lacunas form in the same location during each surge. . .. though a two-part photo showing them some time ago and now would be useful and interesting. I suspect people (like me) would find it interesting to see just how similar the fields are. The exposure of the underlying ice by ablation is solid too. That there is a subglacial trough where they form is a reasonable inference from the orientation of the lake. But the rest of the theory seems a speculative: 1) is subglacial water saturating ice an established phenomenon? How much can the bulk density be increased? 10%?? And would that change in bulk density really manifest in a Swiss cheese pattern, and produce localized faster/slower

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ablation rates. Also, and this wasn't clear to me, are the high areas the saturated zone or the dry zoens? Isn't the saturating water turbid and dirty, so that these higher density zones are also of lower albedo? If so, then the increased density could easily be offset by the increased solar melt rate. In short, I am not sure the theory total holds together. But since the lacunas are there, clearly something happens. . ...perhaps a careful re-thinking of some of the details of the processes involved. . .and maybe a simple model to test albedo vs. density contrasts as a mechanism, would strengthen the theory.

Specific Notes

Abstract: Good

Introduction: Good, but much of what is Section 3 could and should appear here instead, allowing for some condensation. Also, the key point here is that lacunas form in clean ice in contrast to sinkholes, and potentially by a different process. This could be stated more concisely.

Section 2: Good. The foliation information is supplied to buttress the argument for saturation(?). . ...if so it would help to state explicit how the data bears on that issue.

Section 3: Condense and move into the Introduction. Since the paper is about lacunas and not sinkholes, the distinction can be made very succinctly.

Section 5: The evidence for the sub-glacial trough, while plausible, seems inferential. Its location could be indicated by dashed lines on Figure 2 as an aid. I found Figure 6 of little additional value—the photo is taken from too far away. Sections 5.1 and 5.2 could easily be combined and condensed into one.

Section 6: It seems unnecessary to have two sections on ablation, and since the theory does not really hinge on the ablation rate (but rather differential ablation), it might be just fine state that post surge ablation was initially 12- 18 m/yr, then dropped to about 10 m/yr and lose the glacier profiles, which don't show lacunas

Section 7: Figure 6 is good. . ..but would be better if a series of cartoons were drawn

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that showed the whole theory, from surge, to saturation, to differential ablation. This would really capture the essence of the whole paper succinctly and allow of reduction in the text.

Sections 7.1, 7.2 and 7.3: There is good information here and if a sequential Figure 6 was developed, the writing could be simplified and significantly condensed. There is a lot of confusing discussion in these sections of nearby glaciers, but no general statements about how the Bering situation for lacuna addresses what happens on other glaciers.

The key question here, however, is whether the theory is correct and the most important or only effect: "Lacking established literature on the specific effects of saturation, one can only speculate on how the properties of stagnant ice would be altered. A reasonable modification would be the development of randomly distributed zones of slightly lower density."

The author raises the issue of differential ablation as well. But first, what is the direct evidence of differences in density and color/albedo, and second, would one or both be enough (using an energy-balance model) to produce the lacunas. Second, if these have been observed, can it be shown quantitatively that they could produce the depressions? It is not fatal to the paper to say that the theory is speculative and to go on to suggest what evidence would be needed to pin the theory down.

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