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The Cryosphere Discussions

Interactive comment on "Characterizing supraglacial lake drainage and freezing on the Greenland Ice Sheet" *by* N. Selmes et al.

Anonymous Referee #1

Received and published: 4 March 2013

Review of the manuscript titled: "Characterizing supraglacial lake drainage and freezing on the Greenland Ice Sheet", by Selme's et al.

First, I want to focus on the positive aspects of this paper. This study deals with a topic of great importance in terms of understanding the response of the Greenland Ice Sheet to future melt scenarios. Following the publication of Das et al. (2008) it was widely assumed that all lakes would drain rapidly to the base of the ice sheet by hydrofracture. Selmes et al., (2011) demonstrated that only 13% of lakes on the ice sheet drained rapidly and that there was considerable regional variability in lake drainage. Clearly, not all lakes drain by hydrofracturing.

The submitted manuscript follows up on the earlier work by Selmes et al by explaining what happens to the remaining 87% of lakes that do not drain rapidly and has

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important implications for understanding links between melting and dynamics as well as mass balance. The key discovery in this paper is that some of lakes refreeze each year whereas others drain more slowly. Slow draining lakes probably drain to the bed further downglacier and thus contribute to ice dynamics through modulating subglacial processes as well as mass loss through meltwater losses. In contrast, lakes that refreeze do not affect ice dynamics or contribute to mass loss as meltwater is retained within the basin. The data set is impressive, both in spatial and temporal scale and it is clear that a lot of hard work went into this study.

Research such as what has been presented here is critical in order for a complete understanding of how lakes will ultimately affect the ice sheet. That said, it is not clear why the new information presented in this manuscript could not have been presented in Selmes et al (2011).

Now, on to the more negative aspects of the manuscript, and there are sadly many. I would like to preface this section with the statement that my goal here is not to discourage an early career scientist with a long list of generally negative reviews but to try to help him improve his manuscript. I think the research is important and should be published but this paper has long way to go before its impact can be fully realized.

The manuscript is poorly organized. Many of the paragraphs have too many unrelated ideas tossed into them, which makes reading very difficult. Critical information is presented far too late in the paper for it to be effective. To cite just one of many examples, the authors begin discussing how lake drainage varied by ice sheet sector in the Results section, but this is the first time that we've even heard that the ice sheet is being investigated in sectors or even why sector analysis was selected over an ice-sheet wide approach. In short, this manuscript reads more like a mid-stage draft than a manuscript that is being submitted for review.

Neither the Abstract nor the Introduction do an adequate job of establishing the research problem or the time scales over which the problem is being investigated. The

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Abstract doesn't tell us anything about the spatial area of the study, the timescale or duration of analysis and fails to describe the major implications of the research for the ice sheet mass balance and dynamics. It also doesn't mention that the data was obtained using remote sensing techniques. The introduction similarly needs to be rewritten. I suggest the following order which would help guide the reader into the research problem: 1) There are documented links between hydrology and motion of the ice sheet (Zwally et al., 2002); 2) explain how lakes form and are part of the glacier hydrological system; 3) explain how they connect to and interact with the subglacial hydrological system (hydrofracturing); 4) discuss prior work on how lake drainage into the subglacial drainage system affects ice motion (large volume of water supercharges drainage system and causes ice velocity to increase). The introduction could then introduce the research problem associated with lake drainage events – that is that much prior research has assumed that all lakes drain quickly and locally to the glacier bed. It would be extremely helpful if the authors introduced the different lake fates (fast, slow drainage and refreezing) in the introduction, discussed the implications of each fate for ice dynamics and mass balance, and included a conceptual figure showing each process.

There needs to be a better effort to make this paper different than Selmes et al 2011, which I read immediately prior to reviewing this article. To that end, there needs to more emphasis in the Discussion and Conclusions about why understanding which lakes freeze and which lakes drain slowly is important for understanding ice dynamics and mass balance. There is some attempt at this, but it is weakly implemented and lost in the paper's generally poor organization. The authors also need to do a better job of situating their research in the published literature.

Line-by-line comments are included at the end of this review. The list is long but I hope the authors will undertake the revisions as I sincerely hope to see this impressive body work published.

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The paper could use a snappier title that more clearly delivers the message that not all lakes drain locally and rapidly.

1-5: The abstract is awkward and should be rewritten. The only reason lake drainage and hydrofracturing garner as much attention as they do is because delivery of surface melt to glacier beds affects ice dynamics. Incorporation of this information will make the paper more easily accessible to researchers that are not already familiar with the interest in lake drainage events.

I might also suggest that the authors state that while it was previously thought that all lakes drained locally to the base of the ice sheet to affect ice motion, recent work has suggested that only 13% of lakes drain by hydrofracturing (Selmes et al., 2011) and not all drainage events correspond to increases in ice velocity (Hoffman et al., 2011).

Hoffman MJ, Catania GA, Neumann TA, Andrews LC, Rumrill JA. 2011. Links between acceleration, melting, and supraglacial lake drainage of the western Greenland Ice Sheet. Journal of Geophysical Research 116 : F04035. DOI: 10.1029/2010JF001934
9: Change around to about or approximately; also, I can't tell from the abstract over what spatial or temporal scale this study was conducted.

11: Please be consistent in using references to fractions. My preference would be to see percentages used throughout. The combination of "around half" "a third" and "7%" is a confusing combination, particularly when combined with the statement that "the remainder drained suddenly." The "remainder" are the lakes in which most people are interested and it would be useful to know up front what that fraction is.

12: fast and slow drainage types have not yet been introduced, so this is confusing. Please introduce the concept of fast and slow drainage, and retain that specific terminology, when discussing the different percentages of drainage type.

12-14: This sentence contains too many pieces of information. I had to read it four times to figure out what the authors were getting at. This was not helped by the state-

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ment "...despite having similar or longer life spans to lakes..." Perhaps "longer life spans than?" or "longer life spans compared to?"

Additional general comments about the abstract: What implications does confusing fast drainage with slow drainage have for understanding ice dynamics? Also, what implications does the finding that most lakes do not drain rapidly have for understanding ice dynamics? Finally, because there aren't many continuous rivers coming off the ice sheet, many of these lakes probably drain to the bed, but that drainage may occur further down glacier and may not be as rapid as locally draining lakes. This concept is mentioned later in the paper but it would be useful to mention it in the introduction as well.

26: I'd also suggest citing Sergienko, 2013, who modeled lake formation.

SERGIENKO OV. 2013. Glaciological twins: basally controlled subglacial and supraglacial lakes. Journal of Glaciology 59 : (in press)

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This is a purely stylistic comment, but I generally prefer papers that present information and then have a citation supporting the statements as opposed to a list of studies that were published by different researchers. For example, in the first paragraph of 477 (and throughout the text): The drainage of supraglacial lakes in Greenland was first noted by Thompson et al. (1978) who.... The sudden drainage of a supraglacial lake was instrumented by Das et al. (2008).... These findings are supported by further field observations from Doyle et al. (2013)... It would be possible to streamline this section by making process-based statements, such as "field observations of sudden lake drainage suddenly (Das et al., 2008; Doyle et al., 2013) support models of hydrofracturing (Alley et al., 2005"

Line 9: I'd also refer the authors to van der Veen, 1998 and Benn et al., 2009.

Benn D, Gulley JD, Luckman A, Adamek A, Glowacki P. 2009. Englacial drainage

systems formed by hydrologically driven crevasse propagation. Journal of Glaciology 55 : 513–523. Van der Veen C. 1998. Fracture mechanics approach to penetration of bottom crevasses on glaciers. Cold regions science and technology 27 : 213–223. Line 12: It still hasn't been established in the context of this paper that lake drainage events can trigger increases in ice velocity or how ice velocity is triggered.

Line 20: Bartholomaus et al., 2007 argued that it is the efficiency of the subglacial drainage system relative to the rate of meltwater delivery that is important in controlling subglacial water pressure and velocity response. The argument that large fluxes of water could overwhelm a developed subglacial drainage system would be in keeping with the spirit of this paragraph.

Bartholomaus TC, Anderson RS, Anderson SP. 2008. Response of glacier basal motion to transient water storage. Nature Geosci 1 : 33–37. DOI: 10.1038/ngeo.2007.52

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2-5: Similar to many other paragraphs in this manuscript, there are too many disjointed ideas in this paragraph. First, the authors discuss Greenland. Then, without any transition, they mention a study in Svalbard. It is only at the last sentence that we understand that we find that the ideas from Svalbard are being related to Greenland. The information is conveyed, but without transitions, it makes for extremely frustrating reading. I suggest that after the authors tell us that most studies have emphasized fast-draining lakes, they then explain that other lake drainage mechanisms are also possible and use the Finsterwalderbreen study as an example. The authors should then tell us why slow or fast draining lakes is important. I suggest using the Bartholomaus et al., 2008 paper as a springboard.

7: Is this a total of 2600 lakes over 5 years? If so, was there any variability between years?

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12-13: Was melting in 2007 greater than melting in 2012? If not, it would no longer be a record.

Line 25: It is not clear to me why refreezing will change a lake's area.... Do these lakes freeze solid or are they getting a cap of ice on them each year? Is this merely an apparent change in lake area based on the measurement technique?

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Line 22-25: While I agree that slow drainage is likely to be caused by incision of a supraglacial stream, I think the authors need a short section in the introduction describing each drainage mechanism (fast and slow) and refreezing processes and clearly explaining why incision would result in slow drainage and why hydrofracture would result in rapid drainage. In the methods section, methods used to identify each lake type could have its own paragraph.

Page 484 Line 3: Between 1931 and 2069 sounds too much like calendar references. It took me a couple of times to realize that the references were to the total number of lakes.

Line 4: A freezing lake doesn't really disappear from the ice sheet.... While the argument could be made that a lake that freezes solid disappears, the mass is still in the same place, it merely changed state. Lakes that simply freeze over each year but retain liquid water, however, most definitely do not disappear. The distinction is important in terms of discussing mass balance, as "disappear" makes it sound as if the melt is being removed from the ice sheet and contributing to negative mass balance terms.

Table one is confusing. There are columns of percentages, but the fact that these columns represent percentages is only included in the title. At the bottom of these columns of percentages is a total frequency, so it appears at first glance that the sums of each of the columns should equal the total frequency.... I would suggest including the number of each lakes in one column for each year and the corresponding percent-

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ages in a second column. Each year would therefore have A and B columns. Lastly, because it not known how 4-7% of lakes drained each year, there is some error in the analysis that needs to be included in Table 1 and the discussion.

Line 19: It would be useful to know how many lakes occurred in each area of the ice sheet earlier in the text. These different areas should further be introduced in the Methods section and their locations indicated on a map. There is absolutely no reason why this information should be presented as Figure 4. Additionally, it is necessary to explain why the ice sheet was divided into these sectors, which the authors did in their 2011 paper, but did not appear to do here. Oversights such as this one made this paper extremely frustrating to read despite the interesting nature of the research. . .

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8-12: Here and throughout, there is far too much discussion in the results section.

10-11: Tell us in the text that the duration time scale is per year.

General comment on the results section: There are a lot of really smart analyses done here. The impact of these analyses are lost however because each one comes as total surprise and has not been placed in the general context of the paper. When the authors rewrite the Introduction section, I urge them to introduce the concept of each of these analyses as a unique research problem. Furthermore, much of the information in the Results section is far more rich and interesting than the information that is presented in the Abstract. The analysis of the degree to which lakes drain by the same mechanism each year is very important and needs a more prominent location in the abstract. The spatial variability of lake drainage by sector is equally important. Right now, the abstract doesn't give us much more information than we already know from Selmes's 2011 paper.

20: I am still having a hard time with the idea of calling lakes that freeze over in winter a termination event.

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Section 3.3: This section needs to be expanded and better explained. I've read it (and looked at the corresponding Figure 8) and still can't figure out what is going on. Quantifying what happens to lakes in different years is important, and one of the things that I like about this paper, but it needs to be better presented. Additionally, I think the authors need to expand the Results section to discuss how these relationships vary according to sectors

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9: Please tell us what the supraglacial and englacial drainage mechanisms are upfront. For surface drainage to occur, water needs to flow over a topographic low. For englacial drainage to occur, there needs to be a fracture. Gulley et al., (2009) describe locations of glaciers and ice sheets that may experience englacial drainage and which locations will not experience englacial drainage. You might consider adopting this conceptual framework for explaining different lake drainage events.

Gulley JD, Benn DI, Screaton E, Martin J. 2009. Mechanisms of englacial conduit formation and their implications for subglacial recharge. Quaternary Science Reviews 28 : 1984–1999.

Furthermore, I suggest separating the ideas of englacial and supraglacial drainage into separate paragraphs where you link the concepts of slow drainage (via supraglacial channels) versus fast drainage (via local hydrofracturing). It is important to note, however, that slow draining lakes probably flow into moulins further down the glacier.

19: I would argue that the reason that lakes further up on the glacier are not draining is due to low strain rates.

23-24: If the authors think that small drops in water level indicate some complex process, they should tell us what that is. Simply stating that small drops in water level indicate drainage processes are not so simple is not at all informative.

487 line 3: It is not clear to me how a lake forming on firn is going to provide a buffer

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between the water body and underlying fractures. . . . Firn is porous and water should be able to flow quite readily through it, similar to a porous aquifer. I understand how the authors might be able to say that slow drainage through firn aquifers that do not host lakes might not result in hydrofracturing in stressed ice because the rate of delivery is retarded in the firn relative to bare ice. The idea that firn would somehow slow a lake draining through a fracture in the base of the firn, however, does not make sense conceptually. If the authors are aware of a case where lake-driven hydrofracture has been prevented by firn, they should cite it.

Firn could help prevent supraglacial drainage because meltwater flowing into the lake would have access to large storage in the firn. Because of the high permeability of the firn, the firn aquifer would have to become totally saturated before surface drainage out of the lake would be possible. In contrast, all meltwater on bare ice is retained in the lake and there is no capacity for storage in the surrounding glacier ice. All melt contributes to an increase in lake elevation on bare ice, increasing the likelihood for the lake to overflow. In contrast, in firn, a large volume of melt is actually stored in the firn aquifer rather than in the lake.

line 4: We just got done learning that there is no clear elevation above which lakes do not drain by hydrofracturing. Now we are being told that there is a possible elevation restriction on drainage. The juxtaposition of these two sentences is confusing. Further adding to the confusion is that scant attention is paid to making each paragraph consist of a single coherent thought.

Line 22: Catania and Neumann 2010 and Gulley et al., 2012 have both argued that once formed, moulins remain open between years. This data set seems to support those arguments. If a lake drained by hydrofracturing in one year, subsequent drainage events could occur by slow drainage overland in the following years because hydrologicaly active moulins would place limits on the size of a lake (that is a lake would continue to expand until it encountered an open moulin and then drain slowly as the supraglacial channel incised into the ice ridge. In contrast, if moulins were closed off each year, the

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only way to continue draining lakes would be for repeated hydrofracturing events, in which case lakes would drain rapidly every year. . ..

Catania GA, Neumann TA. 2010. Persistent englacial drainage features in the Greenland Ice Sheet. Geophysical Research Letters 37 : 5 PP. DOI: 201010.1029/2009GL041108 Gulley J, Grabiec M, Martin J, Jania J, Catania G, Glowacki P. 2012. The effect of discrete recharge by moulins and heterogeneity in flow path efficiency at glacier beds on subglacial hydrology. Journal of Glaciology 58 : 926–940.

Line 23: The size of a lake is irrelevant if there is not sufficient deviatoric stress to facilitate hydrofracture. There are two necessary ingredients for hydrofracturing, a supply of supraglacial water and stressed ice. If you have one and not the other, there won't be any hydrofracturing (see Benn et al., 2009; Gulley et al., 2009).

25-27: The subhead for this section is "Slow and fast draining lakes: Inference for drainage initiation" but most of the information being discussed is lakes draining in clusters. . . This is confusing. First, the cluster drainage mechanism should have been presented in the results section (and it would have been even more helpful if it was included in the introduction as one of the kinds of lake drainage mechanisms that was going to be investigated in the paper). Second, nowhere in this section did I learn anything about inferences for drainage initiation.

21-24: Krawczynksi et al., 2009 calculate the lake volume necessary for hydrofracturing. . ..

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12-13: Water flowing into a moulin downstream could have virtually the same "pressure" as a lake draining through local fractures if the moulin were nearly water filled. What is different is the rate of meltwater delivery to the bed. In lakes that drain locally through hydrofracture, all lake water is delivered as a slug to the base of the ice

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sheet very rapidly, overwhelming the subglacial drainage system. In lakes that deliver water to moulins overland, the rate of delivery is restricted by the incision rate of the supraglacial channel linking the lake and the moulin. This difference in the rate of meltwater delivery relative to the efficiency of the subglacial drainage system is an important control on subglacial water pressure and ice velocity response (cf. Bartholomaus et al., 2008).

21-27: Again we have another subsection that is not written in a such a way as to be pertinent to the subhead. The drainage mechanism (or lack thereof) of freezing lakes needs to be better couched in the context of ice dynamics.

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6: This 48 hr criteria seems to have been plucked out of thin air and needs to be justified.

6-8: Please tell readers who are not remote sensing gurus the repeat time of Landsat and Aster.

22: There are several references to record melt years, but record melt is occurring in different years. . . This is confusing.

491 6-10: These different lake drainage (or nondrainage mechanisms) should be described in the Introduction and a conceptual figure showing the three different processes should also be included. I think such a figure would be an important visual aid, which is lacking in this version.

17: mechanisms were.

The conclusions need to do a better job of re-hitting the main findings of the paper and then explaining the implications for studies of ice sheet dynamics. It should be made more explicit that simply more melt higher on the ice sheet may not affect ice velocities because lakes in this region to do not drain overland to moulins downglacier or locally through hydrofracture.

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