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Interactive comment on "Challenges in predicting Greenland supraglacial lake drainages at the regional scale" *by* Kristin Poinar and Lauren C. Andrews

Anonymous Referee #1

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Summary

Poinar and Andrews present a new analysis exploring the hypothesised links between supraglacial lake drainages on the Greenland Ice Sheet and the influence of both background and transient stresses. Using remotely sensed lake drainage histories and strain rate fields derived from publicly available velocity products, they find that fast-draining lakes are associated with significantly more-extensional background strain rates than slow-draining or non-draining lakes, although this relationship does not extend to the date of drainage. They show that 16-32 day remotely sensed velocity observations are not useful for identifying hypothesised transient stresses, and make

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Discussion paper

several alternative recommendations as to how data on such events may be collected in the future, and ultimately implemented into ice sheet models.

I believe this paper is a unique and important contribution as it goes some way to addressing questions raised by recent work on supraglacial lakes on the Greenland Ice Sheet, synthesising issues raised by field-based, remote sensing, and modelling studies. The manuscript is well written and logically structured. Furthermore, the authors do an excellent job of explaining the methods and background data, and I see this paper being additionally useful as general reference for those wishing to take advantage of the recent explosion of publicly available Greenland velocity data.

Specific Comments

The authors equate the two surface-parallel principal strains to the maximum and minimum principal strains (ϵ_1 and ϵ_3), assuming that the principal strain normal to the surface (with a value of 0 yr^{-1}) is always intermediate between the two surface-parallel values (and thus is always ϵ_2). However, Vaughan (1993) identifies that on an ice surface with open fractures (which is thus not incompressible) there are situations where observations can show surface-parallel principal strains to be both positive or both negative. As such, the zero normal stress may be any one of the maximum, intermediate, or minimum principal stresses. Consider instead explicitly defining the surface-parallel components as simply ϵ_1 and ϵ_2 (or, is more precision is desired, ϵ_{1surf} and ϵ_{2surf}), disregarding the vertical component (see also Hooke, 1998 or Doake et al. 1998 for examples of this).

The authors separate lakes into completely and partially draining types (L200-205) following Chudley et al. (2019). However, Chudley et al. make no explicit recommendation as to parameters that may separate these lake types, and as such the 10% threshold has been chosen by the authors. Given the established sensitivity of lake drainage studies to chosen parameters (Cooley and Christoffersen, 2017), it might be desirable to include, perhaps as a supplement, data showing the sensitivity of the classification to varying this threshold by some percent.

I have some queries regarding Section 4.1.2, in particular the statement 'fast-draining... and bottom-draining are not synonyms' (L514). Probably originating from the binary described by Tedesco et al. (2013), I have always considered 'fast-draining' and 'bottom-draining' to be synonymous (i.e. to indicate a lake that has drained in a matter of hours following hydrofracture of the lake-bed), as well as 'slow-draining' and 'overtopping' (i.e. a lake that has drained in a matter of days following progressive incision of an outlet channel). Indeed this synonymy is made explicit in definitions included by e.g. Banwell et al. (2012), Selmes et al. (2013), Fitzpatrick et al. (2014), Koziol et al. (2017), and Williamson et al. (2018). My reading of nearly all remote sensing studies is that any 'fast-draining' threshold (e.g. <6 days for this study) is simply the best available method of trying to differentiate the underlying physical mechanisms (hydrofracture vs. overtopping). If I were to observe that '40% of the lakes we classify as fast-draining are not bottom-draining' (L494), I would see that as evidence of classification error (e.g. the lakes drained slowly via overtopping but in 4 days, so were missed by the 6-day threshold) rather than evidence that the two terms are not synonymous. The only situation I can imagine to the contrary would be a situation where an overtopping lake induced non-local hydrofracture and drained in a matter of hours - however, I cannot see how the data presented in this study supports such an inference, as 'fast-draining' is defined using only the 6-day threshold. Of course, this debate could be seen as rather academic, as whether people are using these as synonyms does not change the underlying processes - however, considering the importance of these definitions to both methods and mechanisms, most of all perhaps this is evidence that as a community we should be making more effort to ensure we're all on the same page with regards to these terms.

One final thing that I do not believe is commented on is the interannual variation of individual lakes. Are most lakes in the dataset draining in uniform ways every year (e.g. always rapid, always non-draining) or is it more variable? Can this also be related to background strain?

Minor Comments

- L30 - Cite also Doyle et al. 2013 here.
- Paragraph beginning L35 - Mention also Hoffman et al. 2018 here.
- L65-66 - Cite also Sugiyama et al. 2008 here.
- L146 - “These definitions follow Harper and Humphery (Harper et al. 1998)”. Surely just “...follow Harper et al. (1998)” or “...Harper and Humphrey ([year])”?
- L206-210 - The methods are largely excellent, but more information should be included as the classification procedure for high, moderate, and low confidence levels, which are irreproducible from this text alone.
- L424 - Can this increase be shown to be statistically significant? I find it hard to believe that it can, especially considering the paucity of data in the days preceding.
- L491-492 - The authors identify bottom-draining moulins as being within 390 m of the lake center, justified as being the average radius of the sample lakes. Whilst I understand that identifying bottom-draining moulins for individual lakes from their respective extents may be too much work, it would be useful to include the standard deviation radius or some other measure of variance, so that the reader can judge the extent to which using the average is helpful.
- L591, and elsewhere - Some errors with bibtex or equivalent citation software are occurring here.
- L636 - 0.3 km^2 seems a bit small for an entire lakebed study?

- L637-639 - Arguably the spatial coverage here is slightly too limiting - Jouvet et al. (2019) have shown that the typical UAVs used in the Greenlandic literature can be effectively upscaled to an endurance of 3 hours / 180 km, able to cover one large study site, or multiple different study sites, at a distance from the operator. In this context, I would argue that the spatial coverage of UAVs in Fig. 13 can be upped to 10 km. This is without considering high altitude, long endurance (HALE) UAVs that effectively blur the line between UAV and aircraft, although of course these are largely beyond the engineering and logistical competencies of an individual glaciological research group. For a convincing application, however, see Crocker et al. (2011), who were able to make glaciological observations over three lakes 100 km away from the comfort of Ilulissat.
- L663 - Perhaps considering whether the recent abundance of low-cost carrier-phase GNSS, as well as recent advances such as the L2C band, make a comprehensive low-cost network more feasible for the next decade than previously.
- Paragraph beginning L674 - This review of surface routing models misses that of Koziol et al. (2017).

Fig 5: It would be useful to add colours to each moulin point to indicate the year of drainage, as well as an arrow indicating flow direction to each panel. This would make it easier to identify recurring moulins as discussed in Section 3.2 and elsewhere.

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