Author response to

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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Authors’ replies inline in red   December 29, 2020

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 back-ground 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 ex-tend 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 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 ($\dot{e}_1$ and $\dot{e}_3$), 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 $\dot{e}_2$). 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 $\dot{e}_1$ and $\dot{e}_2$ (or, is more precision is desired, $\dot{e}_{1surf}$ and $\dot{e}_{2surf}$), disregarding the vertical component (see also Hooke, 1998 or Doake et al. 1998 for examples of this).

This idea improves the communication of our strain rates. We have implemented it and explained the reasoning (lines ~150 in the differenced document). We have also updated Figures 3, 6, 7, 8, 9, 10, 11, and 12 , as well as all appearances of $\dot{e}_3$ in the text, to incorporate the improved nomenclature.

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.

Yes, the 10% threshold is our choosing. We assigned it based on the figures in Chudley et al. (2019) and visual inspection of the Landsat images on which (in part) we based our lake-drainage dataset. Exploring the sensitivity of our results to different thresholds, such as perhaps 50%, should be possible by revisiting each of our high-confidence fast- or slow-draining lakes (N=287) and reclassifying any partial drainages, which we identify by estimating the lake area change across consecutive Landsat images by eye. We do not think this sensitivity testing would significantly change our results or interpretation, which center on distinguishing fast lake drainages from slow drainages or non-draining lakes, rather than their completeness. The treatment of these topics in our Results and Discussion sections – lake drainage speed (~5 subsections) and lake drainage completeness (~2 subsections) – underscores this relative emphasis. Nonetheless, the suggestion for analysis of complete/partial sensitivity is potentially meaningful and is something we will consider for future work.

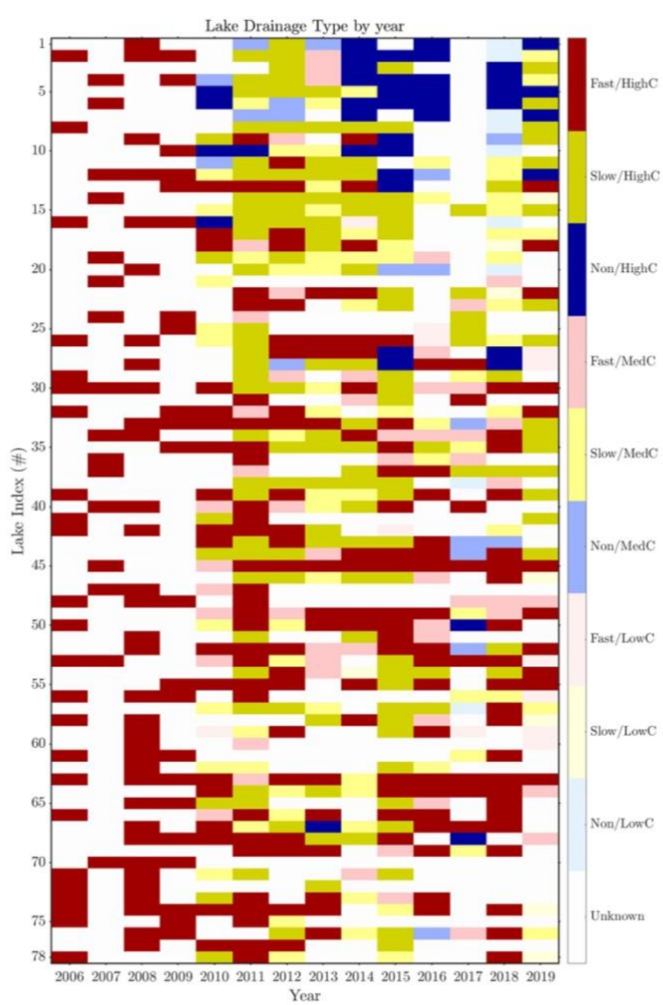
I have some queries regarding Section 4.1.2, in particular the statement 'fast-draining…and bottom-draining are not synonyms' (L514). Probably originating from the binary described by Tedesco et al. (2013), I have always considered 'fast-draining' and 'bottom-draining' to be synonymous (i.e. to indicate 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. over-topping). 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.

We agree with these insightful comments. The Chudley et al. (2019) study blew my (K.P.) mind as well and similarly made me reframe my conception of "fast-draining" and "bottom-draining" lakes, with the new dimension of "complete" versus "partial" drainage. I agree with your assessment that earlier studies (Banwell et al., 2012 through Williamson et al., 2018, including perennially influential ones such as Tedesco et al., 2013) used drainage speed as a proxy for drainage mechanism. With the new wide *availability of WorldView and frequent Sentinel-2 imagery, I think we can soon move beyond this, at least for smaller studies such as this one (N=78 lakes) or perhaps with future AI approaches!*

I'd like to address your point that "40% of the lakes we classify as fast-draining are not bottom-draining" (L494) could amount to classification error. I agree with your suggestion that an overtopping lake feeding a non-lake-bottom hydrofracture and draining in something like ~4 days (or possibly even a matter of hours) would satisfy this classification scenario. I think the point of disparity is "non-lake-bottom" versus "non-local" hydrofracture. Field observations show that hydrofractures some ~500 m (Chudley et al., 2019) or ~1500 m (Stevens et al., 2015) from the lake bottom can facilitate fast lake drainages. In both studies, these hydrofractures sat within the lake basin, but closer to the downstream edge. In both cases, our simple analysis would categorize these as "non-bottom-draining" events because the hydrofractures are both >390 meters from the lake center. Thus, our classification has two problems: (1) using the mean lake radius of 390 meters as a threshold, and (2) approximating the lake bottom as the lake center. I propose to redefine "non-bottom-draining" as feeding a moulin more than mean + 2sigma lake radius (700 meters) from the lake center, in an effort to account for lake-to-lake variability and uncertainty in the precise location of the lake bottom. This changes the sentence in question to "some 10–20% of the lakes we classify as fast-draining are not bottom-draining". We've updated the text in Section 4.1.2 accordingly (lines ~545 in the differenced document).

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?

See the figure here, which summarizes the drainage type by lake and by year of our entire dataset. There is substantial year-to-year variability in drainage type at many of the 78 lakes. In general, higher-elevation lakes have lower indices (~#1–20, toward the top of the diagram), and lower-elevation lakes have higher indices (~#60–78, toward the bottom), but the indices are not carefully ordered (Morris et al., 2013). You can see that higher-elevation lakes may undergo fast, slow, or non-drainage from year to year, while lower-elevation lakes are less variable. Some lakes could be called "usually fast" (e.g., Lake #49) or "usually slow" (e.g., Lake #14). Considering the already-large size of this paper and the limited insight



gleaned from this variability analysis, we're opting to limit its inclusion to only this response document.

Minor Comments

L30 – Cite also Doyle et al. 2013 here.

Added (line 32 in the differenced document).

Paragraph beginning L35 – Mention also Hoffman et al. 2018 here.

Added (line 37 in the differenced document).

L65-66 – Cite also Sugiyama et al. 2008 here.

Added (line 67 in the differenced document).

L146 – "These definitions follow Harper and Humphery (Harper et al. 1998)". Surely just "...follow Harper et al. (1998)" or "...Harper and Humphrey ([year])"?

Fixed (line 150 in the differenced document).

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.

Explanation added (lines ~247 in the differenced document).

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.

Indeed it is not significant. We added the words "but insignificant" to specify this (line 478 in the differenced document).

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.

See our long response to the earlier "specific comment" on lake-bottom moulins.

L591, and elsewhere - Some errors with bibtex or equivalent citation software are occurring here.

Fixed (line 650 in the differenced document).

L636 – 0.3 km^2 seems a bit small for an entire lakebed study?

Indeed. We read this incorrectly from the Methods of that study, which describes a planned image footprint of 400 x 660 meters = 0.29 km^2. From Figure 4 of Chudley et al. (2019), however, the UAV-mapped area looks more like ~40 km x 1.5 km = 60 km^2. We've accordingly replaced 0.3 km^2 with 60 km^2 (line 698 in the differenced document).

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.

We appreciate these UAV references. After reviewing them, we agree that a spatial coverage estimate of some tens of kilometers is more appropriate, which is in fact what we have in Table 1 ("10–30 km"), but we've now increased it to 10 km in Figure 13. We've also incorporated context from both studies into Section 4.2.3.2, Photogrammetry Observations, added these citations, and adjusted our assessment of the potential of airborne photogrammetry accordingly (lines ~701 in the differenced document).

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.

We've added a discussion of these technologies to Section 4.2.3.C, "Dense regional GPS networks" (line ~725 in the differenced document), with the following text:

Our ability to accurately measure GPS receiver position and velocity on ice sheets has improved with the advent of carrier-phase technology, now used widely in glaciology (e.g., Ryser et al., 2014; Andrews et al., 2018; Jouvet et al., 2019; Riverman et al., 2019), and the 2013 implementation of the L2C band, which comes at the cost of power requirements to monitor both L1 and L2 bands (e.g., Van de Wal et al., 2015). Use of single-phase receivers can reduce instrument costs, power requirements, and instrument attrition, allowing deployment of more extensive or denser arrays (e.g., Van de Wal et al., 2015; Sutherland et al., 2015). However, these benefits must be balanced with reduced accuracy, which becomes critical for observing ice motion at hourly timescales, and increased maintenance needs. Design of any GPS network will require careful consideration of the trade-offs in spatial resolution, spatial coverage, and the cost and feasibility to install and maintain stations in the challenging conditions of ice-sheet ablation zones.

Paragraph beginning L674 – This review of surface routing models misses that of Koziol et al. (2017).

We meant this to be a summary of the input methods used in subglacial models, rather than a review of surface routing models. We see that the Clason et al. references blur that line, as that work is really an englacial, not subglacial, hydrology model. We now specify that the Clason et al. studies are englacial hydrology models (line 760 in the differenced document). We also added the Koziol et al. reference alongside Banwell et al. (2016), who uses essentially the same surface routing approach (line 755 in the differenced document).

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.

We have added year labels to each moulin in every panel. We retained the white dots for "off-year" moulins to emphasize the "current-year" moulin, whose dot is colored. Overall, we think the change addresses the stated goal of making it easier to identify recurring moulins from panel to panel. We have also added a yellow ice-flow arrow to each panel.