Review of Dye et al., Warm proglacial lake temperatures and thermal undercutting drives rapid retreat of an Arctic glacier.

Reviewer 1 comments (all in bold)

This submission presents a collection of field- and satellite-derived datasets to characterise terminus fluctuations, surface elevation changes, and calving patterns/mechanisms at a proglacial Arctic lake. The primary conclusion is that frontal ablation is a key component of glacier mass loss, and that undercutting from warm lake temperatures is a key driver of rapid terminus retreat. These are logical interpretations to make, and are largely uncontroversial, but they are only weakly substantiated by the evidence that is presented, leaving the reader to take a large amount on trust. This is probably the most significant of a number of major issues with the manuscript in its current form. These are noted below, and given I believe there will have to be quite a bit of re-writing before this work could be published, I have stopped short of highlighting typographic and editorial errors at this stage.

Authors comments: Thank you for the very detailed and carefully constructed review that you have given. We appreciate the detailed comments about the manuscript that represents the first substantial study of Kaskasapakte, which presents numerous challenges in how to carefully synthesise 3 fieldwork campaigns at a remote and relatively complex glacier-lake system. Your comments are very much appreciated in addressing the challenges of producing a well rounded holistic article that captures as many elements of the system as possible, without becoming too complex and overbearing.

1. **There is, at present, no clear overriding research question that this study seems to focus on addressing. A well-designed study identifies a gap in knowledge, formulates a data collection strategy to shed light on that gap, collects those data, and then analyses the results to provide an advance in knowledge.**

   Authors comments; We agree in an ideal study where aspects of the system are previously known/proven then selected knowledge gaps can be identified and studied in an empirical fashion. The lack of previous study at the field site demands a holistic approach, in order to assess the inter-relations of the different components of the system where possible and identify knowledge gaps requiring further study to advance the science further.We will include the following aim statement on line 51;

   ‘This study aims to investigate the coupling between glacial lake warming and glacier retreat, (on a decadal and seasonal timescale) at a previously unstudied Arctic glacier through remote sensing and in situ measurements.’

2. **The data presented here seek to address four key objectives that are loosely connected but that each demand a major effort by themselves to reach some substantial and novel conclusions (i.e. they could and maybe should each be a study in their own right). As a result, the effort is spread too thinly, and each of the datasets are deficient in some way, undermining the interpretation;**

Authors comments; We appreciate the reviewer has identified the challenges of synthesising data from different components at a remote and highly complex glacial-lake system. This is necessitated by the lack of prior study at this field site, due to the remoteness rather than the lack of dynamic processes. We feel that presenting the data that we have available is highly valuable in progressing the science of glacier and lake interactions and will thoroughly review and amend the discussion of limitations. We feel that it is better to keep the objectives specified as they are by particular methodologies.

   a. **for example, if lake thermal characteristics are to be revealed then it really needs more than a single sensor to say something robust,**

Authors comments; We appreciate the reviewer has identified the challenges of combining complex limnology data in association with glaciology data. We also present lake temperatures of 3.5 ^{o}C from 20 m depth next to the glacier terminus from 8th August 2019 – whilst this is only a very limited snapshot it does prove the lake to be well mixed during the 2019. We also have further temperature data (3 to 4 ^{o}C) to support this from the central portion of the lake; this is included in the supplementary (we are happy to make this more explicit in the manuscript). We also have lake temperature from 2017 (3 ^{o}C at the ice front) and 2022 (3 ^{o}C at the ice front) – we are open to including this data but felt this may upset the balance between synthesis of information and an already complicated narrative (as the reviewer has kindly identified).

   b. **and similarly if calving rates are to be defined then 2D camera imagery acquired over six weeks of the melt season is not really sufficient.**

Authors comments; We appreciate the reviewer has identified timespan of the timelapse imagery data. We have made no claim or comments regarding defining calving rates at Kaskasapakte from this data in this manuscript. We have future plans for analysing this data in 3D.

   c. **This leaves the authors with a rather speculative discussion about what may or may not be driving mass loss at this site and making some quite significant leaps between observations and interpretations, which leaves the reader unconvinced that they are at all robust.**

Authors comments; We have reported a synthesis of what we have observed and recorded at Kaskasapakte glacier to lake system over three field seasons. Thank you for

identifying (comments above) that these observations are logical and largely uncontroversial. We acknowledge acquiring further data to fully understand the system would be fundamental to fully understand the interactions, so would like to amend the title as we accept, we cannot attribute a sole driver to the retreat of the glacier. However, we feel that the manuscript still presents an important advance in the study of glacier-lake interactions.
Dye et al. (2022) identified Kaskasapakte to have the highest retreat rate in the surrounding area. Our manuscript builds on this analysis by analysing retreat rates at higher temporal and spatial resolution, between 2008 and 2019. We have shown that lake temperatures have been warmer (above 4 C) than has been assumed and processes associated with thermal erosional undercutting (as proven with sonar) have been highly prevalent during fieldwork in 2017 and 2019. We are happy to include data from Landsat surface temperature retrieval (Ermida et al., 2020) that has no recordings of temperature above 4 C prior to 2000 BCE. Furthermore, frequent calving activity has been classified and mainly associated with thermo-erosional undercutting during field work periods. We have quantified geometric/volumetric changes in the lower portion of the glacier (we were unable to climb any further up loose moraine/mountainside to obtain further images for the SfM model).

We also wish to request for an extension to map occurrence and extent of iceberg cover in the proglacial lake in the past satellite record. As well as further investigating the velocity products/data that you have kindly highlighted.

Whilst we acknowledge that we have been unable to measure all components of this glacial system, we have provided a relatively thorough analysis of the overall system within the logistical limits of a small budget study at a remote field site location. The evidence that we present is not fully comprehensive of the entire system, but we feel that it is imperative to publish in order that better resourced studies can test the theory more conclusively. With this in mind we would like to amend the title to;

"Warm proglacial lake temperatures and thermal undercutting *enhances* rapid retreat of an Arctic glacier"

3. **Perhaps as a consequence of the above, there is no common thread that can be navigated through the different sections. Different datasets are introduced at different times, describing different aspects of analysis over different timescales. It is very difficult to keep track of what is going on at each step, and it requires a lot of checking backwards and forwards to remind oneself what has already been introduced and what its purpose was. In a similar way the discussion jumps from one aspect to another, with many elements benefitting from only a single paragraph comprising several sentences on the findings of other studies rather than making deep and insightful interpretations of the results that have been presented here. The manuscript would benefit from a re-design to tell a coherent story about the specific problem or question**

to be solved; at present it unfortunately fails to do this in any meaningful way.

Authors comments; We accept that the narrative is complex and challenging; a reflection of the complex and challenging system that it reports on. We will further review the manuscript and include greater organisation by timescale of each component (inter-annual, seasonal and event) as well as reviewing the intertwinement of the glacial and lake system.

Given the highly dynamic nature of such glacier-lake systems it is imperative that studies into them are published to further scientific understanding and also identify key knowledge gaps that enable these systems to be fully understood. We feel the manuscript provides a firm base for future studies into the glacier-lake system at Kaskasapakte (and other field sites) to be studied in order to facilitate greater understanding of how they have evolved and also predict future responses to climate. Given the speed of Arctic Amplification of climate change, we feel that it is imperative to publish such work in good time.

4. **The presentation of the methods is currently quite difficult to navigate.**

Authors comments; Thank you for your thoughts on this, we will review this section and try to develop a schematic or figure to locate more methods from the field campaign – Figure S1 in the supplementary goes some way to achieving this. We will investigate amending Figure 1 c to include more Methodological information , although a graphical abstract may prove to be a better visual tool to guide the reader.

a. I have read this section several times and still don't follow certain elements (in particular what metres of recession per melt year is – lines 87-92 are really confusing.

Authors comments; Thank you for your thoughts on this, we will review this section and revise this section (potentially with an equation to simplify the different terms). We will refer to metres of recession per year and include further equations in the Methods to explain how it is calculated.

b. and how subaqueous mass loss was calculated without a corresponding 2015 bathymetric dataset?).

Authors comments; Thank you for your thoughts on this, we assume a vertical subaqueous ice front in 2015; which is likely given the lack of debris cover to insulate an ice foot protruding into the lake (as has been reported from debris covered glaciers in New Zealand and the Himalaya). Given the lack of debris cover we assume no buried ice to be present in the lake bed – as it would have been subject to buoyancy and calved. So we assume the lake bathymetry from surveys in 2022 to be inherited from retreat since 2015. We will amend this section to clarify this assumption by changing line 115 to;

“The Volume Calculation tool in QGIS was used to calculate the volume changes between the 2019 and 2015 DEMs down to lake level *(with a vertical subaqueous ice front assumed in both years).*”

c. I also find several aspects to be missing – for example different lake temperature measurements at different depths pop up in the manuscript discussion and in the SI that are not mentioned at all within the methods section.

Authors comments; Thank you for identifying the oversight in not mentioning data presented in the Supplementary in the Methods section – we shall amend this by amending line 133 to;

‘Further temperature measurements (also HOBO UAA-002-08) were taken at 20m depth on 8th August 2019 for 3 hours and at the central position in the lake (see S1) at 5m deep from 5th August to 9th September; both results are presented in the supplementary section.’

5. **A major deficiency is the almost complete absence of any uncertainty estimates – uncertainty on the DEM calculations (associated with any offsets or biases between the two datasets, which come from very different sources/methods – were these co-registered in any way? And what sort of values were acquired in stable off-ice areas), uncertainty on the sonar depths, uncertainty on the measurement of the ice-cliff positions and the height of the ice face (that is then used in calculations). This need calculating, and adding, to every presented quantitative value.**

    - **Authors comments;** Thank you for highlighting this substantial oversight. We will review and include uncertainty calculations for both DEMs as well as investigating co-registration. Include this in descriptions of ice cliff heights
    - Include plumb line measurements that were taken in 2019 to validate the sonar depths (thankfully one of the co-authours remembered these)
    - Review and include uncertainty values for the ice front positions

6. **The calculation of mass loss is fundamentally flawed by the absence of any ice dynamics information. As the authors note themselves, terminus positions are a composite effect of the forward motion of the glacier and the removal of mass by melt and calving processes. And surface elevation changes are a composite effect of any vertical**

component of the ice velocity (emergence in this case), dynamic effects, and surface melt/sublimation.

Authors comments; Thank you for your thoughts on this, please note that we do not purport to calculate mass loss. We report elevation changes and acknowledge the limitations that you mention within the text. Unfortunately we were unable to conduct a surface mass balance survey due to the limited size (3 to 4 people) and restrained logistics (field camp) of our field campaigns. Whilst we have no ice density or compaction data, we will investigate whether data from neighbouring glaciers may help and consider whether this would represent a feasibly robust approach for calculating mass loss.

a. **To dismiss ice velocity as being negligible because other glaciers in the area are slow flowing is not acceptable. Velocity data are now widely available, for example the velocities from Kaskasapakte Glacier are readily available from Millan et al., 2022, and a quick look suggests these are not negligible, as stated in the manuscript (Ref: https://doi.org/10.1038/s41561-021-00885-z).**

Authors comments; Thank you for your thoughts on this and identifying this potential dataset. We will remove the assumption that velocity rates are low based on surrounding glaciers. Given the lack of debris cover on the glacier body (there is problematic debris at the margins), potential for geolocation problems at 68 °N (projection and image registration) and relative course spatial resolution of the data used in the suggested velocity product; we would like to request an extension to investigate the accuracy of it on surrounding glaciers where velocity fields are known. It may be possible to use selected data inputs to the product, correct geolocation errors and reduce noise within the dataset to provide a useful velocity record.

b. **It would not be a big step to incorporate these measurements into the calculations and make them a lot more robust. An additional observation here though is that the % contributions of each form of mass loss are highly dependent on the areal extent of the surface elevation change analysis – this seems to be an arbitrary distance from the terminus at present, whereas to be able to talk about mass loss from the system this needs to be integrated across the entire glacier. Otherwise, the information collected is simply surface elevation change, not mass loss.**

Authors comments; Thank you for your thoughts on this, as mentioned above we have not claimed to report mass loss (although I appreciate the original title infers this). Unfortunately we could not travel any further during the SfM survey due to loose unconsolidated moraine and steep mountainsides; so the limit of the SfM model is limited by the extent of relatively safe ground travel. We have reported surface elevation change and suggested what needs to be achieved at this glacier to improve future understanding and how mass loss will respond. We accept that the original title did incorporate this claim into it though and have since proposed a modified title (which removes the single driver assumption). We are investigating other data that may be used for deriving elevation data.

7. **A final more general point is that to be able to convince the reader that one thing is driving another, it is necessary to show that the effect you have observed has happened because of some behavioural aspect of the control.**

   Authors comments; Thank you for the very pertinent comment. We feel that changing the title from 'drives' to 'enhances' retreat deals with the limitation of the study that you have rightly identified. We would also be happy to include lake surface temperature data from Landsat (Ermida et al., 2020) that shows no recordings of 4 C prior to 2000 BCE.

   a. **Here, the lake has been in existence for multiple decades, maybe even a century, and will have warmed up every summer and cooled down every winter, albeit with some warming over the long-term. So what is it that has changed recently to cause the rapid terminus recession?**

      Authors comments; We note that the lake has existed since the end of the Little Ice Age in the study area. Further retreat details (since 1959) are in Dye et al., (2022), which we refer to but will interweave into the Introduction more. Unfortunately the only data on elevation for the upper part of the glacier are for 2015, which has limited elevation change analysis to the lower part of the glacier where the SfM survey was focused. We accept that whilst this is a substantial limitation – surface lowering of the lower section of the glacier reaches a maximum of 7 to 8 m between 2015 and 2019 – roughly 2m per year. This is incorporated into the volume calculations and attributed as such in the manuscript. The revised title is more in acknowledgement of the limitation that surface lowering in the accumulation area has not been accounted for.

   b. **There is a hint in the discussion that calving may have increased, but without evidence.**

      Author comments; We believe that this is a reasonable postulation given the data we present. We also have images of the glacier from pre 2008, that show a lack of an active calving front (the ice front smoothly grades down to the waterline), which we are happy to include in the supplementary as further evidence. We would also like to request an extension to investigate mapping iceberg appearance/persistence in the proglacial lake from the satellite imagery record. We appreciate that these are limited snapshots in time though, so we are prepared to remove this assertion.

c. **There is also some suggestion that recent heatwaves may have contributed, but again there is no long-term weather station data presented to show this. Is it not much more likely that the glacier is responding to a negative climate forcing, and the ice flux has reduced, and that has caused the rapid recession?**

   Author comments; We refer to Dye et al. (2021) for detailed analysis of weather station data and also Dye et al. (2022) for comparison to retreat rates of other land terminating glaciers. Kasakasapakte has retreated substantially faster than other land terminating glaciers in the region (see Dye et al., 2022). We shall also include some further statistics from this on line 70;

   "Recently the area has experienced pronounced heatwaves (month long), with August 2014 and July 2018 being 5.4 oC and 5.6 oC above the long term average (Dye et al., 2022)."

d. **Unless some change in the forcing can be shown, and/or all other possibilities can be discounted, the conclusion of a single or key driver being responsible for the changes is highly suspect, especially when this is the same driver that has been around for many years or decades beforehand.**

   Authors comments; We acknowledge this limitation, which is also acknowledged in the text and are only able to comment on the relative balance of processes within the system. The title will be amended to reflect this. We would also like to request further time to investigate and present other data sources to quantify different components of the system.

I do think there are some valuable observations within the data that are presented here, and with some careful thinking about (and reformulation of) the manuscript structure they will be worthy of publication. Unfortunately I do not support the publication of this submission in its current form.

Authors comments; We would like to thank the reviewer for the very carefully considered and philosophically deep review of the paper. The comments will be very useful in further amending the manuscript and we feel that publication of which will provide a very useful contribution into current understanding of glacier-lake interactions, as well as providing further impetus and directing further studies and publications in this field. Further to this and the proposed changes suggested above, we would like to request an extension to investigate including other sources of data to provide a more comprehensive analysis of the system.