Reviewer 2

We thank this anonymous reviewer for their careful read and constructive comments and suggestions.

General Comments

Dear Editor,

Thank you for the opportunity to review this paper. The authors leverage a record of Landsat imagery to examine the evolution of ice-marginal lakes over the last 3 decades for the Gulf of Alaska. Changes to lake size are compared with available data about glacier dynamics, melt, glacier morphology, topographic conditions and climate data to better understand possible drivers of changes to glacier lake size.

The paper covers important topics that must be approached and I commend the authors for pursuing this original research. Clearly a large amount of novel data was ascertained and analyzed in the context of existing datasets. However, I found some substantial issues with the text which either require clarification or methods need to be adjusted.

In my opinion, the methods and conclusions are generally well-explained. However, I am skeptical of the conclusion that topography is the primary control on ice marginal lake change, given that the authors point to the interaction of topography on climate and glaciological parameters. This seems like a bit of circular argument. Successfully arguing this case will require evaluating the interaction between climate, topography and glaciology and the result deemed independent (or not). If, as stated in the discussion (Section 5.3), that topography is the control on climate and glaciology, then why were the two latter parameter types of data even leveraged?

If the controls on lake growth are retained, then, in the very least, only one of the factors that are dependent with another should be used in the analysis.

My statistical expertise is not comprehensive (in fact, it is quite limited), but I highly recommend exploring methods such as linear mixed models or PCA. I am highly impressed by the amount of work and data presented here. From my prospective, it would really be a pity if the authors did not thoroughly explore the methods needed to make their conclusions clear, concise and well-supported.

Thank you for the two suggestions, which are closely related and also echo one of the main comments from Reviewer 1. The revised manuscript includes two entirely new aspects to address these concerns.

(The following text is identical to that addressing the similar comment from Reviewer 1).

We now include both: 1) in-depth discussion of covariance between environmental variables, and 2) an entirely new analysis undertaking principal components (PC) analysis to reduce data

dimensionality and then running correlations against the PC scores. The discussion of environmental variable covariance highlights the difficulty in untangling some of these variables, as you note in the first paragraph. We believe these data, as well the new figure & existing table associated with them (that is now more centrally discussed), provide stronger support for our existing claims, but also better highlight uncertainty in determining causality between a single environmental variable and ice-marginal lake area change. The PCA results are consistent with our bivariate results, and provide stronger support for the dichotomous behavior of large, coastal, low elevation lakes and small, interior, high elevation lakes. The PCA results also help to disentangle the relationship between glacier attributes (e.g., glacier area, lake-adjacent ice thickness) and topographic attributes (e.g., distance from coast, elevation). Topographic variables load strongly onto PC2, with minimal influence from glaciologic variables, while the opposite is true for PC3 (Table S2 in the revised manuscript). The significant correlation between lake area change metrics and PC2 scores (which holistically reflect continentality), and very limited significant associations with PC3 scores (which reflects glacier size), supports the notion that topography is more closely associated with lake area change than glaciologic characteristics. We have updated the text to clearly state these observations. The PCA data are somewhat more abstract than the bivariate analyses, though, so we believe it important to present both bivariate and multivariate analyses to provide more compelling and physically-meaningful evidence of our claims than either analysis would provide alone.

We omit pasting the new sections, figures, and tables associated with these analyses, which can be found in our response to Reviewer 1.

Additionally, I may have missed it, but it is not entirely clear to me how the lakes we selected. Please explain be sure this is clearly included.

Thank you for noting that this was not clear. We choose a random sampling of ice-marginal lakes using zoomed-in Landsat 8 imagery. We strove to avoid spatial clustering of lakes by using a gridded map and including a near-uniform amount of lakes within each grid cell. We strove to sample from a range of lake sizes, and it is evident that we succeeded in this from analysis of our lake area histograms shown in Figure 4. We now present these study site selection methods early in Sec 3.1 to make these methods clearer. The first paragraph of Sec 3.1 now includes text stating:

"We utilize a gridded map and select a similar number of lakes in each grid cell to avoid biased site selection and clustering. A subset of lakes (n = 40) is sampled from a historical catalog of ice-marginal lakes in Alaska (Post & Mayo, 1971) to avoid undersampling lakes that disappeared and could not be observed in recent satellite imagery."

The paper is generally well-written and organized. However, at many times I found that some terms imprecise and that some reorganization is needed to make the paper more concise. The number and category of ice-marginal/ice-dammed/proglacial lakes should be more clearly explained and simplified. Also, I found much material could fit better in other sections and quite a bit of streamlining will make the paper clearer.

We have reorganized the text, particularly in the results & discussion sections. In this revision, we sought to cut redundant language, instead focusing it in single sections as much as possible. We include a new summary table at your suggestion (described below) which further helps simplify presentation of these data.

Hopefully my comments assist the authors in presenting this data and the findings therein. With well-executed and justified analysis and clear presentation, I envision this being a highly valuable and important paper to the community. This paper could well be useful to all sorts of researchers from glaciologists to aquatic biologists examining changes to our Earth systems as climate warms.

I am quite excited to see (and maybe cite/use) the end result of this project and wish the authors the best in their finishing this work.

We are grateful for the encouragement and would like to thank the reviewer again for these thorough comments.

Specific Comments

Title: I would suggest a different title given the issues I mention the letter.

We have changed the manuscript's title to "Gulf of Alaska ice-marginal lake area change over the Landsat record and potential physical controls" to avoid over-selling our certainty in topographic controls.

Line 13 Abstract: Recent work...understood This sentence makes it sound like it is a one way process between glacier wastage and lake evolution. Might one argue it is a two-way processes with feedbacks?

This sentence now reads "Recent work suggests positive feedbacks between glacier wastage and ice-marginal lake evolution."

Line 16 Abstract: n=107 this seems in the wrong spot. Or maybe lake should be plural.

We moved n = 107 to after "lake perimeters" to avoid singular-plural confusion.

Introduction: I believe important information is discussed here and I found the background information adequate. However, I think some reorganization is needed. A definition of proglacial lakes is given in the 5th paragraph and the first paragraph, for instance. Specific terms of proglacial, ice-marginal, ice-dammed are discussed here but more clearly defined (in my opinion) in the Methods (Section 3). Unless I am mistaken, I also recommend that the term proglacial be explicitly defined as lake with glacier in contact with the water. For instance, does a lake with a proglacial area separating the lake from the glacier count? Also in the 5th paragraph lake is repeatedly discussed, without declaring the type.

We removed the redundant definition of proglacial lakes at the beginning of what was the 5th paragraph in the original submission. We also reorganized the introduction by moving this paragraph up (now appearing as the 4th paragraph) to keep glacier-related items together. The introduction now flows more logically, with the 3rd paragraph discussing impacts of lake change on glaciers and the 4th paragraph discussing impacts of glacier change on lakes. In the new transition sentence to the 4th paragraph, we now stress that glacier-lake interactions are a two-way process, as you mentioned in your comment about the abstract. We note that in the track changes document much of this text shows up a "new" rather than "moved" due to slight changes in wording in places.

Line 40 It is a matter of style, however, I find that such comments about the knowledge gap usually fit better at the end of the introduction, once the knowledge has been presented. Food for thought.

Thanks for this suggestion. We believe it is important to keep this knowledge gap statement in the first paragraph to motivate our work as quickly as possible, rather than leave the reader waiting to see what it is we do. At the end of the introduction, we then pose the specific research questions stemming from the broader statement in the first paragraph. We choose to retain this structure because we believe it engages the reader early, refocuses them later, and balances broadness and specificity.

Line 99 Shifting climate ...change. It is not immediately evident where this evidence comes from, also how does this comment reconcile with the comment in line 40 about the lack of knowledge.

We simplified this sentence, removing the "shifting climate conditions" part, and merged it with the following sentence. That sentence's function was simply to transition to Alaska-based studies, and the modified version does this more clearly.

Line 104–109 I found this paragraph a little bit strange. A model of physical controls is discussed, but none is referenced. From some prospectives, a model might be presented in this work. However, I think it is more compelling to present these as "findings", as opposed to a truly

generalizable model (i.e. could the code/technique/method/concept be slightly modified and applied somewhere else in the world). Also, I am concerned about the differences between physically modeling a process and statistically representing it.

Thanks for pointing out this lack of clarity. The other reviewer also keyed into this statement. In the revised text, we clarify here that we mean a conceptual model rather than a specific numerical model. We very much view our work as the first steps in a larger effort that could eventually culminate in a physics-based numerical model, but these observations and analyses are just a starting point.

Lines 110–115 A personal issue, which the authors may disagree with and wish to ignore. I have problems when questions begin with "how". In my opinion, it is imprecise, abstract and overly academic. Instead, I find testable questions much more interesting? "Are proglacial lakes increasing or decreasing in size? and What processes may cause variations in lake growth?" "we hope our findings will yield insights into the interactions between glaciers and downstream fluvial systems as climate warms?"

We do choose to leave the "how" question as is, but I understand the reviewer's perspective. We wish to cast these questions broadly, but we do also state more specific/testable questions following the broad ones in that paragraph.

Line 122 One thing, which I may have missed, is how were the 107 lake selected? Here, the study area is discussed, so the number of lakes can be omitted, in my opinion. However, I found this vague in other parts of the paper.

We revised this statement to say we "study a sampling of ice-marginal lakes" across northwestern North America and now just include the sample size as a parenthetical. The reason that we wish to leave the sample size here is to be clear that we did not digitize every lake in the region. To avoid spatial clustering in study lakes, we used a gridded map and selected a constant number of lakes from each grid cell. The number of 107 lakes is somewhat arbitrary, but was found to balance the needs for a relatively large sample size for robust statistical analyses with the time-intensive work of digitizing lake perimeters and extracting the relevant datasets. We describe the site selection process in Section 3.1, and have now moved the text to appear earlier in that section. This revised text appears in our response to your main comment about this issue.

Line 142–144 Control variables, environmental parameters and predictor variables. The way this reads, it seems like these are three terms for the same thing. Also, is any "prediction" done in the paper? I am not sure this a proper term to use here.

Yes, you are correct that we used these terms more or less interchangeable. In the revised manuscript (and in response to one of your main comments), we avoid saying "controls" due to the complicated dependencies between environmental variables and the fact that our statistical analyses show correlation but not control/causation. We mean "predictor" in the statistical sense, in which it is essentially synonymous with "independent variable". If there is a statistical association between two quantities, the predictor (independent variable) can be used to predict the value of the dependent variable. We choose to retain the use of "predictor variable" just to avoid redundancy in saying "environmental variable" over-and-over again throughout the text.

Table 1 and Sect. 2.3 It seems like parameter and variable are used somewhat interchangeably here. A parameter is a static quantity in a model, while a variable is an evolving one. I am not sure exactly how these definitions fit in to your usage later, however, please fix this and make the terms consistent. In other parts of the paper, I noticed the term factor. This relates to the comment above as well.

Thank you for pointing out that we were not being precise. In the revised manuscript, we just call all of these quantities "variables" to avoid confusion. All of these quantities evolve in time over sufficiently long timescales.

Line 180 Glaciologic parameters Same comment as above. I found some of the information here a bit beyond what is necessary for the purposes herein. It gives me confidence in your work and rigor that these things are discussed, at the same time the paper would be somewhat more concise if certain bits were omitted. For instance, do uncertainties in the GloGEM data affect your results? If so, is it best to discuss the uncertainty here, or later in the discussion when a reader may understand the interaction between your results and the GloGEM data. I would recommend stream-lining.

Thank you for seeing this language as adding rigor to the study and increasing your confidence in it. In our perspective, it is important to include these brief synopses of the datasets and their associated uncertainties because most of these products heavily rely on model data in an area with sparse ground observations. Later, the fact that we do/do not find a statistical relationship between two quantities (e.g., summer temperature change and lake area change) could be because, in fact, no relationship exists, or because our estimates of temperature change are in error. We discuss this idea in Sections 4.4 and 5.4 and believe retaining these statements here provides the reader with more information to make their own decision about the reliability of these results and datasets.

Line 202 To me, glacier response time is analysis that you conducted. Thus, if probably fits better in the Methods (Sect. 3).

The calculation of glacier response time very closely stems from Huss and Hock (2015), cited in that sentence. You are correct in that it is technically a new analysis because those results were not published in that paper, but we feel that including this short statement in the Methods would be confusing and distract from the main drive of that section. This drive of this paper is ice-marginal lakes, not glacier modeling, and we feel a separate section devoted to this relatively short statement would seem out of place.

Section 2.5 I think much of this section describes work conducted by the authors. Thus, I recommend it be transferred to the Methods (Sect. 3).

We view those analyses using topographic data as pre-processing for later statistical analyses. To reflect the fact that Section 2 largely describes datasets, but also includes some minor processing steps, we now title it "Study area, datasets, and data pre-processing". This may also partly address your comment above concerning glacier response time.

Line 223 This would be a result, the way it is phrased.

We did not mean this statement as definitive, but rather just meant to motivate why we extract ice thickness in the region immediately abutting the lake. The statement now reads "However, it is plausible that glacier attributes in the region immediately bordering an ice-marginal lake may be more important for the lake's evolution." We believe there is less likelihood that this will be interpreted as a result with the "it is plausible" statement.

Line 233-235 This is also a method. I personally find it hard when authors discuss alternatives to their approaches, as it can make the methods hard to follow. I consider methods to be a description of what was done, and not so much a justification compared to alternatives. If you believe that your results could change substantially because of these metrics, it might be worth discussing in the context of the results in the discussion. Also, the need for an alternative method could be discussed in the introduction.

We deleted the statement about alternative strategies, and agree it likely just reduces clarity/concision. Again, we hope the reframing of this section to include pre-processing steps for statistical analyses makes inclusion of these steps in this section more agreeable.

Line 250 Definitions of lake types are given. I think this is needed early in the paper. Also, it seems like two types of lakes exist with three definitions. Ice-dammed and proglacial... Then all lakes together. Are the processes so closely related that is it worth while examining the two type together (Ice-marginal)?

We include somewhat-broader parenthetical definitions of proglacial and ice-dammed lakes in the first sentences of the introduction, and here simply provide a little more detail. We do believe it is justifiable to split *a priori* proglacial and ice-dammed lakes for later analyses. The relationship between these kinds of lakes and their associated glaciers is fundamentally different, and one would reasonably expect them to respond differently to climate & glacier change. Our results show the two groups have distinctly different distributions of area change (Fig 5 in the original manuscript). Further, our statistical analyses later show several correlations where the sign of a statistically-significant correlation differs between proglacial and ice-dammed lakes (e.g., initial lake area; Table 2 in the original manuscript). Combining the two lake types would add noise that could obscure physically-meaningful trends.

Line 270–272 Due to... behavior. This question starts to hint at how the lakes were selected. It seems like if not all lakes could be sampled that some pretty inherent biases could be in place. At one point I got the impression that these 107 lakes were all of the ice marginal lakes in the region, but this sentence suggests otherwise.

It is correct that we did not digitize all lakes in the region, and have added text to clarify that we investigate a sampling of the region's ice-marginal lakes. We discuss how we selected these sites in response to the comment about Line 122. We also state how we sought to avoid study site selection bias in Lines 272-273 of the original manuscript.

Section 3.2 I found that much of this section could fit into the results section. The different characteristics of lake evolution, seem like an interesting result.

Yes, it was a little difficult to decide where to put these statements. We have to give the reader enough context to know what we're doing and why, but ultimately, these statements are meant to motivate the methods we then describe (e.g., curve fitting). We removed the statements with specific numbers (e.g., 9 proglacial lakes formed) because, like you mention, these are results and they also appear in Section 4.1 (Lines 477-483 of the original manuscript).

Section 3.3 Something of a matter of personal discretion, however, I do not think that this much information about the choice in non-parametric tests is needed. Also, for instance, I think that simply reporting the alpha value in the text will due, no need to mention here.

We believe discussing the non-parametric statistics is important because they will be less familiar to many readers (than Pearson linear correlation) and the choice of test makes a big difference for final results. For example, when the tests we describe here are run through Pearson correlation, many more statistically-significant relationships appear, but that is just because outliers and non-normal distributions exist in many of these datasets, and Pearson is more sensitive to these things than Kendall.

Section 4.1 I think a lot of this data could be presented nicely in a table.

This is a great suggestion. We added such a table (Table 2 in the revised manuscript) and now include references to this table throughout the text, which allows us to cut bulky, number-laden language elsewhere. This new table is copied below.

Table 2. Summary statistics for proglacial and ice-dammed study lake area change. Steady lakes are defined as having changed by less than ± 0.1 km^2. Summary statistics are shown for the change of individual lakes, as well as the cumulative area of all study lakes. For descriptors of individual lakes, we use the robust statistics of the median and 10th and 90th percentile lake area change because the existence of extreme values makes the minimum, mean, and maximum area change less meaningful. Relative area change is scaled by a lake's initial area, so a 100% increase indicates a lake that doubled in area, while -100% indicates a lake that completely disappeared.

	Proglacial			*Ice-dammed*		
Number of lakes	**Growing**	**Steady**	**Shrinking**	**Growing**	**Steady**	**Shrinking**
(-, %)	72 (83%)	11 (13%)	4 (5%)	3 (15%)	8 (40%)	9 (45%)
Absolute area change	**10th %**	**Median**	**90th %**	**10th %**	**Median**	**90th %**
(individual, km^2)	0.01	1.28	6.76	-3.7	-0.04	0.36
Relative area change	**10th %**	**Median**	**90th %**	**10th %**	**Median**	**90th %**
(individual, %)	8%	125%	>1000%	-82%	-15%	212%
Cumulative area	**1984**	**2018**	**Change**	**1984**	**2018**	**Change**
(km^2)	336	606	270 (81%)	96	80	-17 (-17%)

Line 346 This again refers to my uncertainty of how the lakes were selected for study.

It is our intention with this statement is exactly to point out that we do not digitize every lake in the region. We are not exactly sure, however, what/how the reviewer would like us to change this statement.

Line 358 In term of lake number . . . number of lakes?

Changed.

Line 399 Isn't τ already to describe glacier response time?

We changed the symbol for glacier response time to "T" and retain τ for the correlation strength.

Section 4.2.1 and Table 2 I would recommend describing what summer temperature and winter precip. represent. It seems like also, water input to lakes might be an important parameter. Why is not total annual precipitation discussed? and why only summer temperature? Also, I mention this in the cover letter, but a lot of these parameters are correlated. While this is interesting, I am concerned that concluding about processes or drivers from this information is difficult. Elevation and temperature are surely correlated. I recommend some substantially different methods to evaluate these relationships. Also, maybe it is mentioned, but what is the relationship between relative lake area change and absolute lake area change?

We have added clarifications in Sec 4.2.1 and Sec 5.3 that we run correlations with winter precipitation and summer temperature (and their change) because these are the climatic variables most closely associated with glacier mass balance, which presumed would be the most relevant for predicting ice-marginal lake area change, though our results show that to not be the case. In Sec 5.3, we now make the following statement.

We note that we do not run correlations between lake area change and mean annual precipitation because variations in winter precipitation and summer temperature show strong relationships with Alaska glacier mass balance, particularly for coastal glaciers (e.g,. McGrath et al., 2017), though changes in precipitation throughout the whole year could be more important for glacier mass balance elsewhere. Probing relationships with environmental variables beyond those presented here provide productive avenues for future research.

As mentioned in our response to the first major comment, the revised manuscript includes more discussion of covariance between environmental variables, clearly states where some variables are very difficult to untangle, and provides new multivariate statistical analyses to support our argument that topographic factors appear more important than glaciological or climatological factors in predicting lake area change.

Relative area change is the absolute area change divided by the lake's initial area. We describe this in the first paragraph of Section 3.2 ("Lake area change analysis"), copied below.

We determine absolute lake area change (ΔA) as the simple difference in area between our last and first lake delineations, where a positive area change indicates a growing lake … We determine relative lake area change as $\Delta A / A_0$, where A_0 is the lake's first observed area and ΔA represents the absolute change in lake area over the study period.

Line 498– 499 I understand the correlation here. Maybe you will get to this. However, it seems like there might be aspects of maritime topography and morphology that lend to large lake formation compared to interior areas. I hope that this will be discussed latter in the paper.

Yes, we share the same thoughts and discuss them in Section 5.2. We are trying to keep this Section 4.3 (the section you refer to above) as close to pure results as possible, and so do not elaborate on that finding at that point.

Section 5.1 I believe that this section would be strengthen if potential regional drivers/differences change cause variations between regions. For instance, the comparison with Wolfe makes sense because of a trend of warming light of your work, given that work goes until 2000. Also what are the differences, physically, that may cause variations between your findings and the Himalayas and Andes.

We added discussion into how our results may be similar or differ to other areas depending on the relative stage of proglacial lake development, debris cover on glaciers, and methodological differences.

Lines 544 geometric parameters... factors? Is this topographic parameters? is this ”glaciological processes?”

We changed “geometric parameters” to “topographic variables”. The other reference is to glaciologic variables and not glaciological processes.

Lines 547– 563 This makes sense. However, does other work validate these findings? For instance, I assume there are papers about lake area vs. catchment area/morphology. Also, does greater glacier width increase the surface area over with frontal ablation can occur, thus creating a glacial lake faster?

We only mean to hypothesize potential explanations for the correlation between initial lake area and lake area change, not to state that this is a fact. We have modified the text to highlight that we are just taking an educated guess here, now stating “We hypothesize two mechanisms that may explain [this association]...”. We added reference to existing work that discusses the idea of stages of proglacial lake evolution and a caveat that the first hypothesis we present requires that Alaska’s lakes are in an early stage of development with ample room to grow into overdeepened basins. However, we do not know of any study that systematically assesses the relationship between ice-marginal lake area and potential subglacial catchment area to be able to provide context/verification for this hypothesis. In response to your second question, we would expect the contact area between glacier ice and lake water to scale with glacier width, likely nonlinearly with an exponent > 1, because wider glaciers tend to be thicker glaciers. The idea you are getting at is what we meant to convey in our second hypothesis, which we have clarified. It now reads “Alternatively, 2) larger lakes likely have greater surface area at the glacier-lake interface, which may lead to higher rates of frontal ablation”.

Lines 565 Is there other work on this? Also what is the greater implication of this finding? Are estuary ecosystems changing?

We are not aware of any other working reporting statistical analyses of topographic factors linked to ice-marginal lake area change.

Line 570–580 Something of a description of the landscape evolution is given, yet no papers have been cited no data or analysis provided to this end. As a result, this text must be omitted and cannot be used to support findings.

We added references to Péwé (1975) "Quaternary geology of Alaska" and Kaufman & Manley (2004) "Pleistocene Maximum and Late Wisconsinan glacier extents across Alaska, U.S.A." to support these "zoomed-out" statements about Alaska's glacial history and the general structure of the landscape encountered across a transect from coast to interior.

Line 585–589 I think this is an important topic, and I am really glad the authors are bringing it up. I hope they discuss the implications more, given paper such as Farinotti 2020, which discuss the growth of hydropower reservoirs following glacier retreat. This also has implications for GlOFs in other parts of the world.

We agree this is an important and interesting observation and now note the link to Farinotti et al. (2019) "Large hydropower and water-storage potential in future glacier-free basins".

Section 5.3 This section seems a bit problematic to me. Only a limited number of climatic variables were examined and the relationship between climate and glaciology is very non-linear (degree day model in the most basic sense). Does the winter precip account for more winter precip falling as rain? This is discussed in the later part of the section, but leads me to wonder why the issue was brought up in the first part of section. I suppose one motivation may be to discuss the role of topography, as opposed to climatology or glacier dynamics. However, lumping these three categories together presents something of a "chicken or the egg" problem. I recommend reconsidering this section. I discuss these issues in the cover letter.

We changed the title of this section to "lack of evidence for … climatic control" rather than "lack of climatic control" as it read before. It is correct that one could run correlations with other climatic variables, and we acknowledge the potential role of unexplored variables in Sec 5.4 . However, we do run a fairly large number of correlations and find minimal evidence for direct climatic control on lake area change, and believe the new framing is better supported by our data.

It is correct that we present this section partly to oppose the stronger findings for topographic controls. But we believe it is still valuable to present a result of no finding, such as we summarize

in this section, because links to climate are an obvious hypothesis to explain observed lake behavior. Therefore, this finding of minimal evidence for a link between climate and lake behavior is interesting in its own right. Not publishing negative results has been singled out as a major factor in the scientific "replication crisis" (e.g., https://www.nature.com/news/1-500-scientists-lift-the-lid-on-reproducibility-1.19970), and we view it to be especially important to publish a result of "no finding" in this context.

Additionally, we added a clarification in Sec 2.3 that the precipitation dataset does not distinguish between rain and snow. Therefore, this dataset would not be able to resolve a transition from dominantly snow to dominantly rain, which would likely negatively impact glacier mass balance, if the total winter precipitation were unchanged.

Line 605 backward climatic correlations... inverse? also the possibility for these relationships are discussed, but no confidence interval/or correlation statistic is given. This is problematic.

We changed "backwards" to "unintuitive". We just mean correlations that lack a clear physical mechanism underlying the association. The revised manuscript includes much more quantitative discussion of covariance between environmental variables, and this line now also points to figures and tables for support.

Line 615–626 Given the non-linear reaction of glacier dynamics to climate and the justification here, I am curious why climatic parameters were explored. It seems rather post-hoc to explain why climate matters little given the correlation is small. To me, this should have been accounted for when designing the experiment.

Climate and climate change are obvious first hypotheses for mechanisms to explain the observed lake area behavior. Admittedly, we were probably overly simplistic in our thinking of the link between climate, glaciers, and ice-marginal lakes when we originally set out on this study. However, it is very plausible that there are many researchers in the community who would expect that lakes have grown fastest in the places that have warmed most rapidly, which has caused the most dramatic rates of glacier mass loss. This study can show that this is not the case and that the story is more complicated. We view that as being a statement worth making, to remind others that there are a lot of moving pieces and nonlinear dynamics in these systems.

Lines 643 Be careful about GLOFs. These can also occur on moraine dammed lakes and while it is beyond my expertise, these dynamics could well evolve with changing proglacial lakes.

We revised this statement to include discussion of both ice-dammed and proglacial lakes as potential GLOF generators.

Section 5.5 I think these section may need restructuring. I believe much of its content is in some way discussed above or deals with the inherent limitations or advantages of physical vs statistical modeling.

Thank you for highlighting this issue. This section has been substantially revised to reduce redundancy with other sections (e.g., elaborating on "unintuitive correlations") and now instead devotes more discussion to the inseparability of associations with lake elevation, continentality, and initial area. The revised section more clearly conveys our difficulty in establishing causality between these potential controls on ice-marginal lake area change, as you have encouraged us to do in other comments.

Lines 663 Doesn't this sentence run counter to many of the arguments presented in lines 615–626?

We do not see these sections as being contradictory. Glaciers are certainly sensitive to climate (Line 663 in the original submission), though this relationship can be complicated, and the exact sensitivity/response can vary from glacier to glacier (Lines 615-626).

Section 6 I often consider "Conclusions" the best opportunity to position the research in the existing knowledge and state the knowledge gaps that have been filled. As a result, I am skeptical of the fact that no citations or references exist in this section.

This is a matter of opinion and writing style, and we choose to keep the conclusion as is.

Figures
Figure 1 I noticed this on lots of the figures, here especially. The lake area change is the close to the color of the glaciers. Can different colors be selected? also it seems a bit curious to me why lake area change, a result, is being presented here. I understand the desire to save space, but would another metric (lake area?) be better? "Detached lake" ... this seems like another term that should be defined together with the rest.

We changed figure colors to promote readability. Additionally, we now present two map-view figures so the revised Figure 1 just shows the location of proglacial and ice-dammed lakes, as opposed to showing the result of area change (now the separate Fig 3). Detached lakes are defined in Sec 3.1.

Figure 2 Please consider the colors again. Also, I understand the appeal of including this information. However, I am not entirely sure that I took away important findings from the figures and trends were hard to visualize given the layout. The authors could omit the figure if they desire.

We moved this figure to the supplements, as we agree it is not entirely necessary for the main text.

Figure 3 I like this figure. It demonstrates the important things. Would it be worth making a couple more panels (or a cartoon) with each type of lake? Proglacial, ice-dammed, detached...

Thank you. We did not add a schematic to this figure to avoid more complication to an already busy figure, but now include examples of both proglacial and ice-dammed lakes in Figs 1-2, which captures the essence of what you're suggesting.

Figure 4 Given the choice of having the three categories of lakes above (Section 3.1, I think). Would it make sense to add a third regression line with all lakes? May the divergent behavior of the two types of lakes here suggest that the "Ice-marginal" type of lake be omitted from analysis?

This is a good suggestion, but for the reasons listed above in the comment about Line 250, we believe it makes sense to not include an "all ice-marginal lakes" fit line. In addition, this plot is already somewhat busy as is, and the addition of more lines may make it confusing to many readers.

Figure 7 I would recommend presenting this information in 2 plots. One with percip/area change and one with temp./area change. To me this plot describes more the change in climate as opposed to the effect on lakes, which is hard to see amongst the different colors and shapes.

We moved this figure to the supplements, because it was somewhat distracting from the main points of this paper. As mentioned in the next comment, this paper is more about the lakes than how climate is changing, and so we have tried to minimize this kind of content in the main text.

Figure 8 Again, this is a somewhat difficult figure to read, and in my opinion somewhat deviates from the point of the paper, which is about lakes, not necessarily climate. Discerning a trend from the color bars is quite difficult for me, and the other information is quite intuitive and presented Figure 2. If the authors decide that this figure must stay, I recommend changing the c-axis and y-axis for the panels.

While we moved the other climate-related figures to the supplements (Figs. S1 & S5 in the revised manuscript), we do believe it is important to leave this figure in the main text. In the revised text, the figure appears later (now Fig. 9), after more text has been devoted to analyzing lake area change, emphasizing that these are our primary results. However, given how much we discuss "unintuitive links" between climate variables and lake area change, we believe this figure provides a more intuitive visual for putting forth physical mechanisms to explain these associations than just a table or test statistic value could provide. In the revised manuscript, we discuss this covariance between environmental variables in greater detail and believe this figure is now more relevant and better supported. While the new correlation matrix (Fig. 10) shows these values in a

more abstract way, we believe it may help get our point across to show the data in this more physical manner. We choose those variables for the y-axes because they are the ones most strongly linked to distance from the coast (the x-axis). The variables in the color axis generally do not monotonically vary with distance from the coast (aside from lake area change) and so the plot ends up not being as meaningful.

Figure 9 Proglacial new this seems like new term, possibly mentioned before. I think I understand what it is, but I recommend creating some kind of glossary early in the paper to make these things clear. Also the exclusion of "Ice-marginal" makes me think that there are two categories of lake, not 3.

We have added definitions of this term (and reminders of its meaning) in Sec 3.2 ("lake area change analysis"), Sec 4.1 ("summary of regional lake area change") and the caption of Fig 3 (where the term first appears in a figure). These "new lakes" are lakes we observed to form during our study period. While we show them differently on plots, which allows the reader to see the environmental variables associated with new lake formation, we do not separate them for any statistical analyses.

Figure 9–11 Isn't this information a visual representation of the findings in Table 2? I think for the point of brevity and such that one or the other should be included. The other could fit well in a supplement.

It is correct that the essence of these figures is contained in Table 2. Table 2 includes all correlation results, while these figures just show a few selections (3/15 environmental variables). We believe that it is useful to show a few examples of the scatter plots because these can be more physically meaningful and/or intuitive than simply reporting a correlation coefficient. A reader might not be able to mentally picture what a $\tau = 0.3$ looks like, and showing these plots give a concrete example of how much predictive power exists in this relationship. We therefore choose to keep all these figures.