1 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.

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.

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.

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.

Specific Comments

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

- **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?

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

- **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.

- **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.

- **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.

- **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.

- **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?"

- **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.

- **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.

- **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.

- **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.

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

- **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).

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

- **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.

- **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*)?

- **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.

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

- **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.

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

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

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

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

- **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?

- **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.

- **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.

- **Lines 544** *geometric parameters... factors*? Is this topographic parameters? is this "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?

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

- **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.

- **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.

- **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.

- **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.

- **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.

- **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.

- **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.

- **Lines 663** Doesn't this sentence run counter to many of the arguments presented in 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.

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.

- **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.

- **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...

- **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?

- **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.

- **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.

- **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.

- **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.