Dou et al. present a new dataset of lake area proximal (<10 km) to glaciers across High Mountain Asia which the authors suggest represents lake extents over three broad study periods (1990-1999, 2000-2012, 2013-2019). The study employs an image aggregation technique in Google Earth Engine to compile scenes from the Landsat 5 and Landsat 8 archives to increase image coverage over parts of the high mountain region which may otherwise have been obscured by clouds or shadows in individual scenes examined by previous inventory studies. The authors are able to map a greater number of lakes as a result.

Whilst the scope and approach of the paper seem appropriate, I don't believe the description of the methods employed by the authors are currently detailed enough to provide the reader with reassuring information about their efficacy. The aggregation of imagery from the three broad study periods is only addressed by the authors at the beginning of the discussion section, which is a significant oversight as the effectiveness of the technique is key to the rest of the paper in my opinion. The authors need to consider more carefully how this technique biases the effective date of composite imagery. The reader is currently given no information about when lake area observations from the three broad periods may have been from, which limits the conclusions which can be drawn about area change rates.

I also agree with previous reviewer of the paper that the analyses and resulting discussion of the new inventory is not far-reaching enough. The consistency of the area estimates have been compared with two existing inventories, but the authors make no attempt to examine the relationship between their inventories and other factors, such as glacier mass balance or various meteorological parameters, all of which are readily available. As a result, I don't think substantial new findings are apparent unfortunately. I would encourage the authors to expand the analyses of their inventory in this regard.

There is certainly potential for the study to yield interesting new findings, but substantial further work is still required at this stage, in my opinion.

I found a variety of more minor inconsistencies within the manuscript, which I list below:

L13- This statement gives the impression that an inventory is lacking, but this is not the case.

L40- Would be good to cite some early work on this topic here.

L43- Delete 'of computers' here.

L44- Landsat imagery generally has moderate resolution (at best 15 m). It can't be considered high resolution alongside WorldView, for example.

L61-73- This discussion of minimum lake threshold is rather repetitive and could be shortened considerably. To me the important point here is the later statement about the general lake area not changing much with different minimum thresholds (L77).

L81-82. This first sentence is contradictory. One says that most studies are sub-regional, the next mentions region-wide examples.

L85- I agree with the other reviewers here that, aside from the aim to produce a lake inventory, there are no clear research questions as such. A new dataset is helpful for sure, but there is much more that could be done with the new data.

Study area section- The setting of the geographical context isn't really required in its current fashion. It'd be more useful to describe the extent/coverage of the new inventory.

L114- ‘mighty’ not often used, suggest deleting this.

L116-117- How much of the work was manual in the process of generating the inventory?

L132- It’s not clear what this approach actually does to remove cloud/shadows from imagery. A clearer explanation of this method is needed here, rather than just stating which tools were used in GEE.

L135- The use of a composite DEM is a strange choice. This DEM represents a surface which could be from any point between 2006-2011. It’s not really clear how important this is to the authors method- can they offer more information on it’s use? The following sentence about ‘manual correction’ is not clear.

L145-152- This could be considerably shortened to state that you used GAMDAM because it offers a contemporary record of glacier extent. The excessive citations in the lines above aren’t needed.

L155-159- The authors need to provide some clear information about the results of this test case. Without a quantitative evaluation of these results it neither conclusive nor reassuring.

L163- For all images?! This sounds incredibly time consuming and subjective.

L165- Please provide the reader with a description of what these ENVI functions do.

L174- *All* proglacial lakes should be connected to a glacier, by definition (and described as such by Carrivick and Tweed, 2013).

L183- I’m not sure how a lake can be attributed as ‘glacial’ if it is not fed by a glacier? Fig.3 panel D shows this point- these lakes could be anywhere on the Tibetan Plateau, a long way from glaciers?

L206- But the image resolution is the same (30 m) without pan-sharpening. How is Landsat 8 just better?

L209- Repeated from earlier, would remove.

L221- What is ‘compated’? Compared?

L225- This sounds very contradictory to the idea that glaciers commonly retreat (apart from those with substantial debris-cover) from their lowest elevation. Please rework this sentence.

Figure 6- I can only see the relation of one of these lakes to and nearby glacier (b) so the context of their expansion/contraction is missing. The reader isn’t provided with any information about lake type here, so the comparison is a little meaningless.

Figure 7- What is plotted here? Lake area Vs altitude, lake number?

L245- The Kunlun Shan is part of the so called ‘Karakoram anomaly’, where glacier mass balance has recently been ~0, so I would revisit the idea that these glaciers are retreating and forming lots of new meltwater lakes. Similarly, debris-covered glaciers don’t ‘retreat’ as quickly as clean-ice glaciers if their debris mantle is thick.

L257- Numerous studies based on higher resolution data have shown the prevalence and expansion of supraglacial ponds across debris-covered, Himalayan glaciers recently. The observation about there being few SGLs in the central Himalaya is related to data quality here and should be omitted.

~L260- There’s a lot of interpretation mixed in with the reporting of results here, which should be saved for the discussion (which I note other reviewers have encouraged too).

Figures 8 and 9- The different approach to these two Figures really highlights an unfortunate contrast. Figure 8 suggests the strongest expansion in lake area in the Tibetan Interior Mountains, which to an inexperienced reader would suggest a large lake population here. Figure 9 contests this idea though, and actually (correctly) shows that there are very few lakes here. I think Figure 8 should be removed as a result.

L276-281- Much more on the aggregation technique needs to be included in the methods section and assessed in the results. The validity of the temporal analyses depends on this method and its description and it is currently not addressed.

L305- I doubt a different in glacier inventory extent of a few pixels (which would be the extent of the buffer contrasts) would result in the difference of >1000 lakes between inventories.

L310- The ALOS DEM has its limitations too- poor surface contrast, shadows and clouds all hinder the success of DEM extraction which do not effect the SRTM DEM. The authors are splitting hairs here which I don't think have had a big impact on results.