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Interactive comment on "Supraglacial pond evolution in the Everest region, central Himalaya, 2015–2018" *by* Caroline J. Taylor and J. Rachel Carr

Anonymous Referee #2

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Review of 'Supraglacial pond evolution in the Everest region, central Himalaya, 2015-2018', by C Taylor and R Carr

The analyses by Taylor and Carr have used Sentinel-2 multispectral data for 2015 to 2018 to identify supraglacial ponds on 10 glaciers in the Everest region of Nepal. The study thus seeks to extend previous analyses of pond incidence by 4 years, and to evaluate the state of glacier lake development in the region.

The analysis has several major errors that need to be reconsidered before a review can be completed, but more unfortunately, the framework for the study does not look to provide any new insights into the prevalence of supraglacial ponding, nor for the devel-

opment of supraglacial lakes in the region. The current results are impossible (reported supraglacial ponded area several times larger than glacier areas) and over-interpreted in the discussion. I recommend that the manuscript be rejected and returned to the authors.

The following major points deserve consideration: 1. Lack of clear motivation and value for the study. The study aims to use Sentinel 2 data to extend the analysis of Watson et al (2016) to present, but provides very little justification for this aim or for the approach used. Why will 4 years of additional annual data be helpful in assessing glacier lake development? Glacial lakes usually take 10-30 years to develop (see the Imja lake development history, for example). Can you be sure that the changes over a 4-year period are not simply due to noise – past studies have identified high interannual and seasonal variability for these features, and a time series length of 4 gives low confidence. Altogether there seems to have been little critical evaluation of the research question and suitability of the analysis to achieve an answer.

2. Key references and understanding missing. The manuscript exhibits a low-level of understanding of past work on supraglacial ponds and ice cliffs, and their association and co-evolution. The use of terms like 'slope gradient' convey missing basic understanding, and in several instances the results of other studies are misinterpreted. The list of past efforts to map supraglacial ponds is incomplete, and only one alternative method to map ponds is even mentioned, with no consideration for its use. The classification scheme for glacial lake development is fundamentally misunderstood as well, leading to the development of the authors' own classification scheme. Fundamentally, the 'glacier' outlines used in the figure, and the text within the manuscript, indicate that the authors have a misunderstanding of what a glacier is – they have delineated only the debris-covered areas, but discuss accumulation areas within these bounds.

3. Incomplete methodological description. There are many key details missing with respect to the pond classification method (whether atmospheric corrections were performed, how many samples were used for each landcover class, which landcover

classes were used, even the physical basis of the scheme and how MLC works), for the slope analysis (why surface slope is a predictor for ponds, whether slope is the same as gradient, and any details for the slope calculation itself), etc. In addition, the velocity data analysed in the study were derived and provided by Dehecq et al (2015) but repeatedly presented as 'our' velocity data. At the same time, virtually no details are provided on its derivation (even what sensor it is derived from).

4. No consideration of pond seasonality or interannual variability. The 4 years of data are erroneously interpreted to directly imply a trend, despite the low sample size and low study duration. In addition, the analysed scenes correspond to an unusual period of the year for analysis (April) when cloud cover can be very problematic – no discussion is included for cloud identification and removal, which can be particularly problematic for pond delineation. The April scenes are also susceptible to the stochastic filling of small basins as winter and spring snow melts, which can lead to additional noise in the pond-cover time series. Worse, the study has also included a December scene, when ponds are often frozen-over and covered by snow, which also requires further consideration and discussion.

5. Erroneous/impossible results. The reported total pond areas are on the order of many km2, whereas the glaciers themselves are only a few km2 each. This may be a basic calculation mistake, but it is concerning that the proofreading did not pick up on this mistake. As someone familiar with the glaciers in question, the 2018 data look suspicious, indicating a higher pond coverage that I would have expected. This could be related to key point 4.

6. No advance in understanding based on the study. The addition of 4 years of pond coverage itself provides little value, as the authors have not considered seasonality (a task which modern optical sensors provide opportunity for). The authors have tried to reinterpret the stages of glacial lake development, but I have issues with their updated framework, which also does not seem to be based on the results within the manuscript. Most specifically, there is no evidence that cliffs or ponds increase in incidence up-

glacier with time or that ponds pre-date cliffs (suggested stage 2). Rather, the earliest observations of glacier surface morphology in the area (e.g. Watanabe et al, 1986) report debris, cliffs, and ponds with a similar distribution to that found today, with the change that the entire debris area has expanded slightly up-glacier. In addition, pond expansion and drainage is an ongoing, circular process that happens on seasonal and multi-year timescales (e.g. Benn et al, 2017; Miles et al 2017; Miles et al, 2017b). Fundamentally, perched ponds will eventually drain if they exist at all (Mertes et al, 2016) so it is unclear how this represents a distinct stage of lake development.

Detailed comments through results; I feel that there are enough errors that need to be amended that feedback beyond results is not yet constructive.

L8. Proglacial lakes can represent a major hazard to downstream communities, but do not always. In this instance I think you mean moraine-dammed lakes, or ice-marginal glacial lakes. See Carrivick and Tweed, 2013.

L8. Why only S2A, and not also S2B? Not that the 10m spatial resolution only applies to some bands.

L27. Dehecq et al 2018 (should be 2019) is an odd choice of reference here, as that study did not look at impacts on communities.

L31-35. See Harrison et al, 2018, which discusses the historical incidence and future development of glacial lakes.

L40. -0.22+/-0.12 m w.e./a is not 'strongly negative'; this is similar to the global historical average (e.g. Haeberli et al 1999).

L46. I would suggest qualifying this statement: 'are often characterised by. . .' as there is considerable variability across the region, and not all of the glaciers follow this description.

L63-78. I think there may be some referencing confusion here. Miles et al, 2016 (in Annals of Glaciology) did discuss these points, but you have referenced one of several

Miles et al 2017 publications (in Journal of Glaciology).

L73-75. This is backwards to me; according to the Brun, Buri, and Watson studies, the ponds enhance the ice cliff melt (not cliffs enhancing the pond melt) and certainly are the stimulus for calving.

L80. Watson et al, 2016 should be in this list.

L81. This point should be attributed to Salerno et al (2012) and Watson et al (2016), which bot examined these same glaciers with high-resolution imagery to quantify the areas missed by coarser sensors.

L86-91. Why is the addition of 3 years of coverage a scientific priority? There is little clear motivation for this analysis, especially as the study has not integrated the results from Watson et al (2016) to provide a continuous perspective from 2000 to present. Of the stated objectives, (1), (2) and (3) have already been carried out by previous investigators, so it is important to clarify how your analysis will differ from those of Gardelle, Salerno, and Watson, respectively.

L96. Presumably this should be 'high-elevation accumulation areas'

L101. Why the reference for Immerzeel et al (2010) here? That study did not focus on the Everest region, nor did it examine GLOFs at all.

L103-108. Thank you for formalising the pond/lake name convention.

L110. Not all S2 bands are at 10m resolution, but also, none are <10m.

L114-116. The choice of April images is strange to me, since you could not carry this out for each season, but also because of the high cloud coverage in your tables. Most studies of this type have focused on the post-monsoon given the lower cloud frequency. I appreciate the addition of observations from other times of year, but seasonality also confuses your interannual comparison due to the inclusion of December data for 2015. Furthermore, in April there is often snow on parts of the debris-covered area – how

did you mitigate this, which could cause confusion for your algorithms (and for the comparability between years)?

L125. Please provide the full set of training classes, as well as the number of training sites used for each class, and a comparison of the spectral characteristics derived from each class (as in Gardelle et al, 2011).

L127. For consistency, please specify the wavelengths of these bands.

L128-133. This is a very cursory overview of glacial pond/lake mapping techniques, with no discussion of spectral methods based on the normalised difference water index (Watson et al 2018) or other indices (Gardelle et al, 2011) or even the physical basis for delineating water using wavelengths beyond visible light.

L136. More details are needed with respect to your derivation of a slope map. What algorithm did you use? Did you simply calculate slope at each pixel based on its neighbours, or did you perform a more sophisticated analysis (Quincey et al 2007; Miles et al 2017 JGlac; King et al 2018)? Also, the terms 'slope' and 'gradient' mean different things, and 'slope gradient' is literally the spatial rate of change of slope.

L141. More discussion is needed with regards to the ice cliff mapping. Did you simply mark cliff edges (i.e. Thompson et al 2016), or map cliff outlines (Brun et al 2018), and how did you assess your error in this regard? Most studies that map ice cliffs use even higher resolution imagery as cliffs can have narrow planimetric areas, so how did this work practically? How did you associate cliffs and ponds to one another?

L145-157. None of these ponded area values are plausible. All are greater than the area of the individual glaciers by at least an order of magnitude.

L158-169. I find it quite difficult to accept the perceived increase in supraglacial ponded area over a 4-year period, given the potentially-large interannual variability of supraglacial ponds (Miles et al 2017, JGlac).

L187-189. You have given 2 examples of coalescence at large lakes resulting in areal increase, but this statement needs a specific analysis – there are >6000 ponds in your study, so it is difficult to consider that this is 'predominantly' the mechanism of expansion. How many lakes drained over the study period, and how many new ponds formed over the same years?

L196-197. A comparison between glaciers based on number of ponds and total ponded area is nonsensical, as these glaciers differ in size and characteristics. Percentage area is a better basis for comparison.

L198-200. Repetition.

L204. These values indicate that you have calculated the average surface slope, which differs considerably from the average surface gradient for debris-covered glaciers (e.g. Quincey et al 2007), which is the actual control on pond incidence (Miles et al 2017 JGlac).

L205. Some confusion of the > and < through here.

L208-213. Your glacier outlines seem to correspond roughly to the glacier's debris-covered area for each glacier (ie they do not include the accumulation areas, which are substantial for Khumbu and Ngozumpa, and non-negligible for the others). Thus your 10% area segmentation is also just for the debris-covered area of each glacier. Please clarify this in the text.

L217. There is no high accumulation zone within your study area.

L214-255. This is really very detailed, but is missing the big picture – a 1% change in ponded area or cliff area most likely does not represent a real change, but noise in the system. This is exemplified by, e.g. Ama Dablam Glacier, which shows considerable interannual variability in ponds and cliffs. If anything, the lack of change on an annual timescale for most of your glaciers is noteworthy.

L254-255. This misses the link between these two features in their development and evolution, which has been identified and examined by several authors (Benn et al

2001;2012; Miles et al 2016 AGlac; Brun et al 2016; Buri et al 2016; Watson et al 2016;2017; etc). Cliffs and ponds form and evolve together; in some cases an isolated cliff or pond is left, but they often share developmental history due to melt feedbacks and water supply.

L260. There is some confusion with regards to this evolution scheme. 20,000 m2 is not a very large pond. Also the stage 2 of development refers to the development of a feature similar to Spillway Lake, which could then expand up-glacier and develop into an Imja-style proglacial lake.

Figures and Tables

F1. These outlines roughly correspond to each glacier's debris covered area only. Also it is possibly worth noting that the Imja Lake and terminal moraine have been included in Imja Glacier's outline, so these aresomehow debris-covered glacier systems?

T1. Possibly worth including Watanabe et al (1986) and even Fritz Muller's work (1962) both of which reported these features.

T2. Again, these characteristics are only for the debris-covered areas of the glaciers.

F2. This excel plot is not very aesthetic, and a poor use of space. Instead of 3 panels, one could combine axes and plot types (e.g. lines or markers) to a single figure.

F3. What do i and ii indicate? Not clear from caption.

F4. Unusual to continue the subpanel numbering from the previous plot.

F5. This is an odd way of visualizing a glacier, as the reference point is actually the middle of the glacier! Better to start from the terminus (or terminal moraine) and work upwards, as this at least is a fixed reference.

F6. Same comment as for F5. These two figures should be combined.

F7 and F8. These should also be combined with F5 and F6 – there is no new analysis for the data, just the visual comparison with the velocity data (which could be added to the F5 plots).

F9. I suggest adding a column for the total %ponded area. Also, some consideration needs to be made for your ability to detect cliffs with this methodology, which was not entirely clear.

F10. Many authors would argue that Ngozumpa's Spillway Lake is still too small to be considered stage 3. Imja is certainly stage 3 in 2015 though! For the 2015 data, some consideration needs to be made for the difference in season for comparison purposes. Also, 4 years is a very short time to interpret changes in stage, as there haven't been any profound changes.

F11. This two additional stages are meaningless as they operate continuously on glaciers that exhibit cliffs or ponds, and the authors have provided no evidence that glaciers currently without cliffs and ponds will someday develop them. Rather, early observations on these glaciers did note the presence of cliffs and ponds more or less where we find them today (though likely with lesser frequency).

Suppmat misc

Choice of scene dates is unusual

Figure S1. Units incorrect – these values are considerably larger than the respective glaciers!

Figure S3. These charts should be combined. How were ponds smaller than 100m2 derived? That should not be possible with Sentinel-2 data.

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