

Comments on ‘High-resolution mapping of glacial lake expansion in Kyrgyzstan (2016–2024) using Sentinel-2 imagery’ by Piroton et al.

The manuscript by Piroton et al. provides new lake growth evolution data from mid-to-high altitude environment in entire Kyrgyzstan. The study is based on lake contour delineation using Sentinel-2 imagery, conducted either by hand or by NDWI thresholding, in order to create a reference inventory from which machine learning is applied to reconstruct historical lake distribution. The reference and reconstructed datasets are from 2022-2024 and 2016-2017 respectively, corresponding to a 6-8 years evolution time span for this study. The trends described in this manuscript go along with other trends detected across similar high-mountain environments.

The manuscript is well written and provides interesting data on lake evolution in relation to hydro-cryosphere environment. Specific regional aspects are highlighted, following a thorough processing framework.

Overall, the manuscript is technically sound, but several important issues need to be addressed before publication.

Major comments

- The title suggests high-resolution mapping of ‘glacier lake expansion’. On Lines 118-119, the authors write ‘The geographic extent of this work corresponds to areas across the territory of Kyrgyzstan at elevations $\geq 3\ 000$ m to focus on high-mountain glacial lake environments’. On Lines 242-243, they write ‘Polygons were further filtered by proximity to glaciers (within 30 km, based on the RGI v7.0) and by elevation ($>3\ 000$ m)’. First, this suggests that the authors consider that all lakes above 3 000 m are glacial lakes. Many lakes above 3 000 m in Kyrgyzstan (and Central Asia) are indeed of glacial origin, but not all high-altitude lakes are “glacier lakes” in the strict sense. There are many lakes of tectonic origin, or which resulted from landslide- or GLOF-damming, especially in this seismically active region of Central Asia. Second, a lake being of glacial origin does not necessarily mean that its formation is related to current deglaciation mechanisms. It is also known that, as a result of climate change, large share of precipitation is currently falling as rain instead of snow, not only at low and mid elevations, but also more and more at higher elevations. This is due to warmer winters and snow season shortening. Some mapped lakes can therefore simply result from the accumulation of higher amounts

of liquid precipitation than before. Third, both the temporal and spatial relationships between lake expansion and glacier retreat are not touched upon in this paper. The only argument that the reader gets to know, although from general statistics, is whether the lakes were detached from, attached to, or located on glaciers. How can the authors then claim, for instance, that (see Lines 17-18) ‘These results highlight the ongoing influence of glacier retreat on lake formation and expansion in Kyrgyzstan’? Given the strongly oriented scope of the *Cryosphere* journal, one would expect an in-depth analysis of such ‘glacier-hydrology interactions’, as argued in the Discussion and Conclusion.

- Why did the authors conduct a high-resolution mapping of high-environment lakes only between 2016-2017 and 2022-2024? In general, authors generally work specifically by decade(s), allowing for clearly distinguished time periods and for reaching a reasonable minimum number of study years for statistical representativity. Here a maximum of 6 to 8 years of study is proposed. Why did the authors not start in 2015 (when Sentinel-2 first appeared), and why did they stop in 2024, whereas the article was submitted in 2026? As a result, there is already 25-30% of the study period that has become obsolete since the last year of study. I believe the thorough 2024 Randow Forest model could be applied to late 2025 or early 2026 to complement the lake inventory and make it more robust in terms of identified trends.
- In last years, various studies involving efficient water surface level monitoring in lakes (e.g. <https://iopscience.iop.org/article/10.1088/2515-7620/ad7701>; <https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2021GL095950>; <https://www.nature.com/articles/s41598-025-20434-7.pdf>) using InSAR (or multi-source data) and machine learning has emerged. The present study does not provide perspective or comparison between their method and the latest advancements (as would be expected in the *Cryosphere* journal).
- The reader often finds itself lost through the various inventories and comparison exercises that were conducted. The manuscript would strongly benefit from a clearer unfolding and sequential description of these processing steps, e.g. in the form of a clear descriptive structure (and why not a summary table) between 2024_NDWI, 2024_manual, 2024_corrected, 2016_manual, 2016_corrected, 2024_RFC datasets.

Minor comments

L. 33: Remove ‘Indeed’, as the previous line is more general.

L. 45-46: GLOFCA (<https://glofca.org/en/>) , a new lake monitoring initiative in Central Asia would deserve mentioning, given their strong focus on disaster risk monitoring.

Figure 1: describe the inset.

L. 87-88: ‘Mean July temperatures vary from 17–40 °C in valleys to around 4 °C at high altitudes’. This 17-40 range in valleys is rather wide – why not provide an average temperature, like is done at ‘high altitudes’?

L. 95-96: Provide references.

L. 101-103: Idem.

L. 118-119: ‘The geographic extent of this work corresponds to areas across the territory of Kyrgyzstan at elevations $\geq 3\,000$ m to focus on high-mountain glacial lake environments’. Does this mean that the presented inventory is not a complete inventory of what they call ‘glacial lakes’? How many of these lakes are located below 3 000m?

L. 126-128: ‘Two distinct temporal windows were considered: [...] (2) July to October of 2022–2024, representing recent conditions for our glacial lake inventory’. Why extend the reference period across two years? This is not discussed. See Major Comments on time period limitations for trend robustness.

L. 133-134: ‘In areas where a single tile did not meet these conditions, multiple adjacent Sentinel-2 tiles were merged to obtain optimal coverage, minimizing both cloud contamination and residual snow’. How were these tiles merged?

Figure 3 caption and L. 186: why were Sentinel 2-A images only used? This is not discussed.

L. 156-157: ‘The DEM was generated from optical stereo images acquired by the Panchromatic Remote-sensing Instrument for Stereo Mapping (PRISM) between 2006 and 2011’. Since the DEM used was created in 2006-2011, could the time difference with the study period affect the precision of altitudes provided in this work given the rather intense and rapid glacier environment evolution that is discussed?

L. 166-167: ‘This dataset offers global coverage of linear hydrographic features that are too narrow to be represented as polygons’. Please add ‘, including those’ after ‘features’.

L. 179-180: ‘to accurately map glacial lakes in Kyrgyzstan and analyze their spatio-temporal evolution’. This is very redundant across the manuscript and most likely not needed.

L. 186-189: ‘Spectral index and topographic constraints were used to generate preliminary lake outlines, which were then carefully refined through extensive manual editing to ensure accurate delineation. This manually corrected inventory provided a consistent set of

reference polygons, serving as training samples for supervised classification’. Idem – reduce redundancy across the manuscript.

L. 193: ‘our comparison between the two inventories’. Add ‘already existing’ or so after ‘two’. See Major Comments of clarity between the various processing steps.

L. 257-258: ‘whereas the Random Forest-based inventory was compared to the manual 2016 inventory’. It is rather unclear to figure out how this 2016 inventory was created and compared to. See Major Comments on processing steps clarity.

L. 285-289: Several statistical aspects are vaguely described and should deserve more attention and detail. I also propose to create a special table to highlight the main findings in this regard.

L. 370: ‘Inventoried lakes were classified as supraglacial, proglacial, or glacier-detached (Fig. 8a)’. These classifications were already illustrated in Fig. 6C. Should Fig. 8a come before the latter for figure order coherence?

L. 371-372: ‘whereas glacier-detached lakes are no longer directly affected by glacier ice or meltwater’. This is not entirely true from a glacio-hydrological perspective. It is not because a glacier becomes detached from its original lake that they get disconnected flow wise. Caution should be taken in the discussion on this kind of hydro-topographical considerations.

L. 375: ‘consistent with its high glacier fraction (9.34 %)’. Add a table, map and/or graph for highlighting such relationships with glaciers (distance, density, glacier coverage etc). The RGI contains all the necessary information for this. See Major Comments on this.

L. 376-377: ‘Despite their different regional sizes, this result suggests that glacier size rather than regional area primarily limits lake formation’. Is this relevant to the discussion? If yes, this should be clarified.

Figure 9 caption: do the ‘a)’ and ‘b)’ correspond to the ‘A)’ and ‘B)’ of the graph? Also, why not add letters to the left part of the graph, as the figure design is not particularly clear on how to read this part.

L. 422 vs L. 431: ‘Over half of all lakes (54 %) remained stable over the study period’ vs ‘Stable lakes accounted for about 20.7 %’. Please explain why these ‘stable lakes’ differences appear here. Please also be more specific when describing such trends.

Figure 10: It is not clear from these maps where most changes occur, given the large number of points, diameters and classes. How about producing maps with continuous changes, where colours directly define gradients?

L. 485-486: why referencing 'low FP'? Shouldn't the false positives only be described (as e.g. simply 'FP') before being discussed?

L. 486: 'recall'. It has already been mentioned earlier on several occasions (e.g. L. 230, 250 etc). It should be defined earlier on these first instances (and not here).

L. 526-527: 'This reflects the progressive development of new depressions exposed by glacier retreat'. See Major Comments: these glacier-lake relationships have not been properly argued through this discussion.

L. 526: 'progressive development of new depressions'. The depressions were already present before the glacier retreat. 'development' should be replaced e.g. by 'exhumation'.

L. 529: 'highlighting the role of glacier retreat in creating new depressions'. Idem, please replace 'creating' by e.g. 'liberating'.

L. 535-536: 'Supraglacial lakes showed slight increases in median area and an upward shift in elevation, consistent with the retreat of ice surfaces'. Can this be clearly related to the respective 2016 and 2024 glacier status (e.g. using the RGI)? If not, this is speculative.

L. 537-538: 'gains from supraglacial-to-proglacial transitions were broadly balanced by losses as some proglacial lakes became glacier-detached'. Idem – this is to be demonstrated. See Major Comments on this, as well as comment on L. 371-372 as rivers could flow out of the lake, keeping the water level balanced.

L. 540: please add 'lake' before 'expansion'.

L. 544-545: 'topography becomes the key limitation, as observed for example in the Tropical Andes where glaciers have lost more than 50 % of their LIA area and the topographic potential for new lake formation is now fairly limited (Emmer et al., 2020)'. I do not see why this reference is used in this Central Asian context, and how this related to this part of the discussion. Should another reference be used? Is this sentence necessary?

L. 573-574: 'This is reflected by the high precision and recall values'. How can this be reflected if some lakes were not detected in the other datasets? Or is this related to the efficiency of the Random Forest classification? Please clarify.

L. 660: 'The original manuscript was refined using ChatGPT'. For transparency, please explain in which ways.

Given the comments above, my overall impression is that this manuscript is not yet mature for publication and should be resubmitted after major revision.