> Journal: The Cryosphere (TC)
> Title: Assessment of permafrost distribution maps in the Hindu Kush Himalayan region using rock glaciers mapped in Google Earth
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> MS No.: tc-2014-128
> MS Type: Research Article

REVIEWER'S REMARKS

GENERAL COMMENT

I endorse the arguments for getting a better handle on the distribution of permafrost in High Asia, and much more attention to the largely neglected topic of rock glaciers there, for this and other purposes. Great concentrations do occur in certain mountain ranges, and offer a way to appreciate the occurrence and complexities of cryosphere conditions and a basis for tracking changes.

I also agree that this task is severely constrained both by the sheer extent, diversity and logistical difficulties of the terrain and environments of interest, and the near total lack of any concerted research in most of the high mountains, least of all into rock glaciers. As such there is an urgent need to exploit the high resolution satellite imagery that has become available, and I agree that it now shows good diagnostic detail for identifying RGs, their dimensions and diversity of forms, and sub-regional differences.

As a contribution to Cryosphere Discussion, important questions arise as to:

i) how far and how well prevailing notions of rock glaciers and permafrost, largely developed elsewhere, apply in poorly or un-researched areas of High Asia;
ii) how far rock glaciers relate to permafrost, are sensitive or effective indicators of its extent or boundaries; and
iii) the promise and reliability of emerging GIS methods in a vast, complex, and data-poor region.

Regrettably I find the paper as developed so far, hard to follow. The methodology and statement of results seem unconvincing. A much better appreciation of the nature of rock glaciers is required, their relations to permafrost, and implications of what is seen in the HKH.

MAJOR CONCERNS

The basic hypothesis or purposes of the study seems to be: use of rock glaciers (RGs) as indicative of permafrost, especially its lower elevation limits in the HKH region, and as a test and extension of two existing permafrost maps. In principle this seems fine, but:

1. First, the results cast doubt on the purpose and conclusions. It is stated that "Comparison of the two rock glacier mappings showed relatively small differences indicating that the proposed mapping procedure works consistently."(p.5306 l.7-8) However, apparently the "mappings" only identify or are reliable *in 4%* of the 4.5 million km^2 region of interest! They exhibit a larger area (26%) with uncertainty, and exclude over 70% of the region. Does this not suggest that either the hypothesis, or the method used to test it, are, at best, inefficient or marginal to the problem? For similar reasons the results say very little about the two region-wide permafrost maps. The "first order" (?) differences or agreements seem sketchy for IPA, and very local and marginal for PZI (?)

   The results in Fig. 6 and 8, give an impression of complex and fine-tuned findings, but it is not clear to this reviewer what they mean. How does Fig 6. reveal RGs "…in relation to

Permafrost Zonation Index summarized *over the entire HKH region*" -- if only established for 4% of it??? And in areas I know I cannot make sense of Fig. 8. It shows yellow squares in the core of the NW Himalaya/Karakoram/Hindu Raj area suggesting " there is only permafrost in favorable conditions". Surely, there is only ever permafrost under favorable conditions! However, in these sub-regions there are not only large areas of permafrost, but also hundreds if not thousands of RGs. Meanwhile:

2. A more critical assessment is needed of why the authors are convinced that RGs in these high mountain environments are, or can be, used delineate the limits of permafrost. My work suggests considerable caution on this. Even in the Alps and subpolar regions there has been a progressive retreat from the early view that permafrost is a prime factor in the origin of RGs, let alone definitive of them. Certainly, in the Hindu Kush, Hindu Raj, Karakoram, and NW Himalayan Ranges, with which I have some familiarity:
    i) a majority of RGs depend primarily on avalanched snow and rockfall or talus deposits, on glaciers up above or transitional to RGs, typically some combination of all these. They drive the development, scope, downslope reach and fluctuations in RGs, but relations to permafrost are unkown. At least, an explanation is needed for assuming that lowest or 'mean minimum lowest' reach would depend upon, or reflect the presence of, permafrost, rather than the scale and strength of avalanching, rockfall, glacier and wind driven processes.
    ii) Experience suggests that, in addition to RGs "… which do not reach the regional lowermost occurrence of permafrost.,,(p, 5307, l.12) there are many others that reach below it.
    iii) A key determinant of the lowest reach in any given valley and, presumably, mean minimum elevation of RGs (not permafrost), is *the elevation of valley floors*. This is determined by landscape and stream system evolution, in which permafrost is a dependent variable too. Thus, the RGs you reference in England and Owen (1998), descend as low as 4,300 m (there are many others in the same valleys terminating higher and up to 4,900 m). However, within 50 km to the west and north, many RGs descend below 3,900 m and some down to 3,400 m. There is no reason to think RG-generative conditions are much different, but valley floors are incised lower.

3. I have trouble with various aspects of the statistical procedures and results.

    Is 'random sampling' as used here, an appropriate method? It is one thing to select at random to prevent bias in sampling for characteristics distributed within a known population. But thousands of random spatial samples in order to find some particular item in this vast region seems like searching for needles in haystacks? Moreover, it must provide randomized outcomes based not on your concerns, but on probability distributions of regional terrain. It seems unlikely to be good at discriminating the comparatively rare RGs.

    Incidentally, we *know* there are tens of thousands of individual RGs clustered across the whole region! In this sense I am surprised that all your results involve only 'one, two, or occasionally 'more than three" RGs. In hundreds of valleys in the NW Himalayan ranges and, no doubt, other parts, there are concentrations of dozens of RGs within radii of 10-30 km.

    "Mean minimum elevations per sample" (Fig 5 etc)? Not sure what this implies. You seem to have a lot of cases with only one or two RGs per sample, making a mean minimum value seem meaningless? (eg. in Fig. 5). Conversely, how is it valid to compare such with others having three or more. Again, this disregards readily available evidence that, in valleys with numbers of RG's, termini elevations typically vary and may range over 100's, if not a thousand metres, when permafrost does not?

    I am surprised just two operators are seen as sufficient to establish or preclude operator error

in such a complex task and visual procedures -- even assuming you could get started without some common set of instructions and discussion with them, which is bound to affect selection procedures and make it entirely possible both would be wrong while producing identical results (?) With respect to operator error, the lowest elevation lines at RG snouts appear the critical ones and from what you show they seem to differ little. However, this begs two questions;

i) does one *or either* trace show the actual lower limit of the active RG. You appear to assume it does, but I am not at all sure. The images in my copy are not of the best resolution, but Figs 2.and 4 are good enough to raise doubts about how much of what you show *inside* each operator's trace, can be confidently treated as active RG. They look suspiciously like examples I know that combine active, inactive and 'fossil' areas, while margins in this steep terrain may involve debris derived from RG activity, but not part of the active body.

ii) If RG termini are spread over a range of elevations, it is unclear to me how taking a mean value for two or three, or even ten or fifty, gets any closer to the lowest elevation of permafrost, being at most, a very crude value of where permafrost may occur

iii) In such an exercise, the complete lack of any ground control is problematic, or any indication of attempts at field checks or experience with RGs anywhere. Nearly all our knowledge of rock glaciers and related permafrost issues is based on field studies, and translating from them to remotely sensed data needs to be spelled out.

MINOR MATTERS

p. 5298 line 23-4? "Many of the investigated rock glaciers have developed out of Little Ice Age moraines…" Isn't this based on assumption? Of the tiny number of RGs investigated in the HKH, are there *any* actual age determinations or established histories, let alone "many'? Also, views of the LIA, its duration, intensities and uniformity or otherwise across High Asia, are all being contested; also whether Eurocentric views haven't misled us as to what has happened there.

p.5298 l.22-3 Hewitt (2014) is cited but evidently not consulted. Nowhere does he state or imply there are "lowermost elevations… around 4,000 m". The tables and surveyed examples in his Chapter 11 include RA termini at 3,500 m and some down to 3,350m (this in the W. Karakoram, which might have led to a comment on the "lowest elevation" you cite, in Northern Afghanistan of "3,554 m"). He also reports a nearly 1750 m difference between lowermost termini across the Greater Karakoram region surveyed (his p.275).

p.5303, l.10 "…transversal and longitudinal flow structures, providing a subjectively acceptable, but here not objectively testable, level of confidence in interpreting landforms as intact." Does "intact" mean 'Active'? If so, this is not reliable, 'subjectively' or otherwise. In HKH 'ridge-and furrow' "flow structures" can be highly developed and may persist indefinitely in inactive features, even in relict RGs.

Also, I suggest a further caution concerning;

"…Vegetation coverage, an indicator of inactive or relict rock glaciers…"

Apart from the roles of lithology, elevation and local climate, there is extensive, intensively practiced mountain pastoralism almost throughout HKH areas where your RGs occur. Active RGs are avoided, but inactive and relict RGs can be heavily used, and modified by grazing, firewood collection and temporary summer residences. Also, vegetation cover is not everywhere a reliable indicator of 'inactive' RGs. In some areas I have observed *active* ones with a ground cover.

p.5304 l.10 "If variations within close proximity occur, they follow regional patterns."

In such a vast region and complex task, you need to specify just what the ‘variations’, and ‘close proximity’ mean here, and which “regional patterns” are followed?

p.5306 “A clear increase in the minimum elevation reached by rock glaciers can be observed between the south and the north side of *the mountain range*.” The HKH region as shown in Fig.1 has many huge mountain ranges. Are you saying that in all of these you expect RGs to descend lower on northerly than southerly? Can the very limited and scattered identifications really support this conclusion? In my experience other factors reverse this relation in some areas, as they do for glaciers and snowlines.