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Interactive comment on "Heterogeneity in Glacier response from 1973 to 2011 in the Shyok valley, Karakoram, India" *by* R. Bhambri et al.

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A comprehensive inventory is provided for a hitherto poorly known set of glaciers; a valuable benchmark paper for the region and refinement of methods that could be applied more widely. It adds to the improved uses of higher resolution, more frequent imagery now available for High Asia. It goes well beyond the simple area statistics and visual inspection of so much past work, not least an undue reliance on terminus changes. Changes in glacierized area in the late 20th and 21st centuries were estimated for a sample of the glaciers. Broad episodes of decrease and increase were found, and quite irregular fluctuations for individual glaciers, but no net change. The results are of interest in relation to recent debates about Hindu Kush-Karakoram-Himalayan (HKH) glaciers and climate change (Raina 2009; Bolch et al 2012; Scherler

et al 2011). They further reinforce indications of distinct responses in the Karakoram and their 'heterogeneity', as well as differences from the rest of the HKH (Hewitt 2005; Gardelle et al 2012). Given the argument that distinctive responses in the Karakoram or whole HKH are due to 'debris-covered glaciers' it is of special interest that debris-covered ice seems far less significant here. A bonus is an examination of the Khumdan glaciers with a history of catastrophic glacier lake outburst floods (GLOFs), and their recent advances. This said, there is a need to clarify how the results are presented and interpreted, in particular:

1. The choice of drainage basins is puzzling, and why alternatives are not considered. It produces two very unequal sets of results, one four times larger than the other with a much greater proportion of the glaciers and all the larger ones. Hence, it is hard to see the point of comparisons between them. It is unclear why the Chang Chenmo is treated as a separate basin but not, say, the Galwan, draining the Aksai Chin, or the Chip Chap draining the Depsang Plateau and Mountains. They are Shyok tributaries of comparable size. Also, it is well-recognized in the literature that geology, geomorphology and sub-climatic regimes differ between the Karakoram Batholith terrane of the Sasir-Rimo Mustagh, and the eastern tributary basins between the Pangong Suture Zone and Karakoram Pass (Searle 1991). As far as I am aware, no surge-type glaciers have been identified east of the Shyok main stem. West of it, glaciers appear to be part of the higher, more rugged Mustagh Karakoram.

2. Size classes and elevation ranges. The paper is valuable in countering the almost complete neglect of smaller Karakoram glaciers in the past. However, treating all larger masses as one class ">10 km2" produces some curious results. It is recognized as "the largest area in the entire study" (3056.l1), but taken no further. When combined with the basin division chosen (Fig.2) the significance of the results is more confusing. The Rimo system alone comprises a greater ice cover than in the whole Chang Chenmo basin. If just four other glaciers are added, North and South Shukpa (="Kunchang") and the Chong and Kichik Khumdan, just 5 out of a total of about 2,200 comprise over

a third of the glacier cover. This raises a question about the significance of saying “only 8” glaciers span “over 2,000 m”. Is it because the range seems small compared to the main Karakoram further west, while is again an important variable? Rimo Glacier has a 2,605 m elevation span. Recent retreat of the North and South Shukpa (=“Kunchang”) Glaciers has reduced their elevation span to slightly less than 3,000 m. Once more, would it help to draw a distinction between the larger glaciers in the Sasir-Rimo Mustagh, west of the Shyok, and all basins east of it?

3. Climate change. While the data challenge notions of uniform and exaggerated HKH response to climate change, the impression that small net change means no real change should be treated very carefully. Most of the larger glaciers are at their furthest recorded retreats since the early 20th century, albeit complicated by recent activity of some surge-type glaciers. The Rimo has lost over 6 km2 in the terminal area alone since the 1930s, and become two separate glaciers (South and Central). It is not clear whether the data confirming no net change recently compensates such losses by growth high up, through combination with many small ice masses, or the claims about surge-type glaciers (?)

4. Longer-term changes. In general, some reference to the rest of the 20th century, at least, would put recent decades in context. Although there is almost nothing useful on the Chang Chenmo glaciers before satellite imagery, there are reports and good maps of the Rimo and Sasir Mustagh glaciers from early in the century (Dainelli and Marinelli 1928), (last Survey of India 1930s?/US 502 version). Perhaps this is more about article length, since the authors have done path-breaking work on the history of glacier mapping and inventories in the region (Bhambri and Bolch 2009 see article) but it does limit the impact.

5. Debris-covered ice is shown to be far less significant in the sub-region than the rest of the Karakoram, but does vary substantially within it. There is much more on the Sasir Mustagh glaciers compared to the Rimo Mustagh to the north, as well as eastern upper Shyok, and some conspicuous areas of debris-covered, stagnant ice around termini,

or rock glaciers. If not here, in future it might be a useful experiment to see whether this spectrum of absent, low or medium debris covers has any relevance for the area changes identified.

6. “Surging activity”(p.3058. l.19). What exactly is the evidence to suggest “advances in glacier area can mainly be attributed to known surging activity”? It would be a critical observation if true. Surge-type glaciers surely help explain why terminus variations of Karakoram glaciers are not just out of phase with the rest of High Asia, but with each other – as indicated by the Khumdan observations here. However, the overall picture of glacier area change is a quite different matter. Surge rhythms are always more or less out-of-phase with, and cannot readily be used to reflect – or counter – climate driven mass balance changes. They are unlikely to reveal any systematic shift in glacier areas except over time frames of some centuries at least. In any period of less than a century, the great majority of surge-type glaciers will be retreating in the ablation zone and storing mass high up. This raises the issue of how well thickness changes can be identified or are at all reliably related to measureable area change in rugged terrain? Just how many surges the authors have identified in the study area is not clear. As far as I am aware there is only a small number in the period of record and none are known in the Chang Chenmo basin. If there have been more (or fewer) surge events in recent decades, it is most likely because of poor or absent coverage before. This reviewer (Hewitt 2007) did suggest an unusual concentration of tributary glacier surges in the period but in the high Central Karakoram, not the study area. And in that case, improved observation or coincidence cannot be ruled out as major factors.

2. Specific comments

i) (p.3052) Most maps trace the Shyok River to a junction with the Indus in Baltistan. To avoid confusion the sub-basin chosen could, perhaps, be called the ‘Upper Shyok’ (?). It might also help readers to point out that some other, more extensive and heavily glacierized areas of the Karakoram drain to the Shyok; via the Nubra (incl. Siachen), the Saltoro and Hushe valleys. All of the area is disputed politically by India and

Pakistan but is the geographical nomenclature? ii) (p.3052, 20-25) It is good to see Cameron Wake's (1987; 1989) important work cited. What is said needs qualification. It was carried out 150-300 km west of the upper Shyok; in basins with different hypsometry, elevation ranges and ice cover. For instance, reported snowline and ELA elevations are 400 to 1,500 m lower than those for, respectively, the Rimo Muztagh and Chang Chenmo Mountains (von Wissmann. 1959; Yafeng et al 2010). iii) (3052. 21) Summer precipitation in Wake's (1987, p.96) snow pit profiles showed a minimum of 1/3 and maximum of 3/4ths. The average was almost $\frac{1}{2}$ for all years and sites, and he suspected it might be more. Would conditions in the upper Shyok not be transitional from the high Karakoram, and towards an even greater summer contribution? iv) (p.3056, l.2). The significance of singling out that "glacier termini are 100 km lower in the Shyok basin" etc, is hard to understand. In both basins actual termini range over more than 1500 m. v) (3059.5) The notion that the Chong Khumdan actually surged from 2002 to 2011 is surprising, if not unique. My evidence suggests a surge – a sudden, short-lived acceleration of a few months of the north-east branch – followed by a more gradual adjustment of the main lobe (?). vi) (3059,28) "The number of surges almost doubled after 1990. . ." Please confirm evidence for this and whether it could reflect improved observations vii) (3060, 2). Can surge-type glaciers have an "all-year accumulation regime" and not other glaciers in the same region? Note also, as Wake (op cit) showed, Biafo has an all-year accumulation regime, is predominantly direct snowfall-, not avalanche fed, and is not known to surge, although two of its avalanche-fed tributaries have. viii) (3060, 13-14) "mystical" may not be an appropriate word. Also, the phrasing suggests that responses are only "heterogeneous" because of a lack of "long term programs. . . etc". In other words, the data presented create a misleading impression. Is that what is meant?

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