

Earth Surf. Dynam. Discuss., referee comment RC1
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Comment on esurf-2021-21

Anonymous Referee #1

Referee comment on "Spatio-temporal variations in glacier surface velocity in the Himalayas" by Yu Zhou et al., Earth Surf. Dynam. Discuss.,
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In this article, Zhou and colleagues report changes in winter glacier velocity for seven regions of Asia between 2017-2018 and 1999-2000. Glacier surface velocities are derived from satellite image correlation. They use two sensors: Landsat-7 (L7) for the period 1999-2000 and Sentinel-2 (S2) for the period 2017-2018. Contrary to previous studies, they observe glacier slowdown in the Karakoram and acceleration in the eastern part of the Himalaya. They find that thinning glaciers have increasing winter velocities and stable/thickening glaciers have decreasing winter velocities. The latter result is very surprising and seems in contradiction with the theoretical framework of ice dynamics understandings.

While the text is well written and the figures are of high quality, the methods are not precise enough to allow a replication of the work. I also suspect major flaws in the data processing and analysis that leads to erroneous results and conclusions. In particular, the interpretation of changes in velocities in relationship with changes in ice thickness is not convincing at all (i.e. increased accumulation suggested in regions where glaciers are losing mass). Below I provide general comments about these major flaws and some technical remarks on the text.

1 - The velocity changes are not reliable and reproducible.

Analyzing glacier velocity changes from different sensors is very challenging and requires to check some basic requirements that are not full-filled here. Dehecq et al. (2019) showed that using different sensors can introduce large biases in velocity. Please find below a check-list of critical methodological points that need to be addressed:

- Calculate velocity changes on exactly the same pixels. From the text (L76-77), I understand that the authors calculate a mean velocity for each glacier for the first period, and then a mean velocity for the second period. The velocity change is calculated as the difference between these two terms. This framework is not suitable to calculate velocity changes, because the glacier velocity is highly variable in space, and consequently the mean glacier velocity for each period must be calculated exactly on the same pixels. This is even more critical when two different sensors are used (L7 and S2 here). These sensors have different capabilities and S2 likely produces reliable velocity fields for much larger fraction of the glacier surface than L7.
- Due to the non-gaussian distribution of residuals (i.e. the fact that the modulus of the velocity vector is always positive), you need to find a way to normalize the velocity changes. One good check of the efficiency of the chosen normalization is that the velocity change on stable terrain should be zero. Please provide the velocity changes on stable terrain to demonstrate the robustness of your processing. On this topic, I recommend to read thoroughly the supplementary of Dehecq et al. (2019), and in particular the figures S8 and S9.

2 - The interpretation of the observed changes is not convincing

This article is very short, which is in general good for scientific writing in my opinion. However, here I feel that I am missing important parts of the message, due to the text's lack of details. For instance, the authors should give more context/background about the section 4.1 ("Relationship between glacier surface velocity and geometry"). Why are these relationships investigated? Why is the change in velocity expected to be related to the slope, area or other variables?

I am also missing a more precise interpretation within the climate context. The authors study winter glacier velocities over a very large region with contrasted climate settings. In the central Himalaya and Nyainqentanglha glaciers accumulate during the monsoon/spring period, while in Karakoram they accumulate during winter (Maussion et al., 2014). As a consequence, winter does not mean accumulation period everywhere and any interpretation related to the climate context must be much finer than the proposed analysis.

The authors attribute the difference between their results and Dehecq's results by the fact they measure winter velocities, whereas Dehecq et al. measured summer velocities. The interpretation of the authors is not right here: Dehecq et al. measured annual velocities (fig. S2 of Dehecq et al.), with the average annual velocity field centered roughly in summer (fig. S3 of Dehecq et al.). Seasonal glacier velocity variability is poorly documented, especially for Asian glaciers (Armstrong et al., 2017; Usman & Furuya, 2018), but I doubt that the difference between the two studies originates from it. I suggest that the authors apply their workflow to summer image pairs to demonstrate that they can replicate Dehecq's results first.

L137-149 are very difficult to follow and do not make much sense to me. From my understanding, the negative relationship found in this study (fig. 6) lead to the conclusion that the parameter m in eq. 1 should be negative? This is in strong contradiction with basic physics of ice dynamics (e.g., Cuffey & Paterson, 2010). If the authors think that "ice mass loss promotes glacier motion in winter", they need to suggest a mechanism that could explain this. I don't follow the logics of the rest of the section (L145-149), and I think that most of these statements are not correct (and not backed up by any literature reference).

Technical remarks:

Title: "Himalayas" is not the name of the studied region, which encompass Karakoram, Hindu Kush, Himalaya and Nyainqêntanglha

Structure: it is clearer to write three separate sections for the methods, results and discussion. For instance, L90-100 read like a results sections and not a discussion.

L10-11: this is not shown in the paper

L55: this is incorrect, see my comments above

L70-71: a better way to evaluate the velocity changes accuracy would be to look at stable terrain changes

Armstrong, W. H., Anderson, R. S., & Fahnestock, M. A. (2017). Spatial Patterns of Summer Speedup on South Central Alaska Glaciers. *Geophysical Research Letters*, *44*(18), 9379–9388. <https://doi.org/10.1002/2017GL074370>

Cuffey, K. M., & Paterson, W. S. B. (2010). *The physics of glaciers*. Academic Press.

Dehecq, A., Gourmelen, N., Gardner, A. S., Brun, F., Goldberg, D., Nienow, P. W., et al. (2019). Twenty-first century glacier slowdown driven by mass loss in High Mountain Asia. *Nature Geoscience*, *12*(1), 22–27. <https://doi.org/10.1038/s41561-018-0271-9>

Maussion, F., Scherer, D., Mölg, T., Collier, E., Curio, J., & Finkelburg, R. (2014). Precipitation Seasonality and Variability over the Tibetan Plateau as Resolved by the High Asia Reanalysis. *Journal of Climate*, 27(5), 1910–1927. <https://doi.org/10.1175/JCLI-D-13-00282.1>

Usman, M., & Furuya, M. (2018). Interannual modulation of seasonal glacial velocity variations in the Eastern Karakoram detected by ALOS-1/2 data. *Journal of Glaciology*, 64(245), 465–476.