The Cryosphere Discussions

The Cryosphere Discuss., author comment AC2
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Reply on RC2

Jakub Małecki

Author comment on "Recent contrasting behaviour of mountain glaciers across the European High Arctic revealed by ArcticDEM data" by Jakub Małecki, The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-165-AC2, 2021

Dear Reviewer,

Thank you for your time and work on my manuscript and all suggestions you made to improve its quality. There is a number of issues raised in your review, please may I respond to the most important ones below.

Comment #1 – the presented manuscript discusses only selected mountain glacier sites, while the recent study by Hugonnet et al. presents elevation changes of almost every glacier in the world.

It is hard to argue that the study by Hugonnet et al. (2021)(or "H21") is a game changer and a landmark dataset in glaciology. No study with incomplete data coverage can compete with H21 and, obviously, this has not been the aim of my manuscript. From the very beginning it was meant to make use of test-sites, rather than trying to achieve full data coverage. Simply speaking, there was not enough ArcticDEM data that was useful for my analyses, because in many subregions of SV, NZ and FJ there is only a handful of DEM strips to browse from and even less from summer seasons.

However, even though only part of mountain glaciers in SV, NZ and FJ are studied in the manuscript, I do consider the sites as representative of the wider population of mountain glaciers in the Barents Sea sector. One argument is that the study sites cover the regions rather uniformly and represent different glacier settings. The stronger argument is, in my opinion, that glacier elevation changes are relatively homogenous between sites within larger subregions (see e.g. Figure 4). This suggests that a denser array of study sites would not necessarily bring much new information, however, smaller anomalous sites might still remain undetected with the presented data.

After careful reading of the two reviews I now see the need for a more extensive discussion and comparison between my dataset and the one by H21. The latter might serve as a benchmark, e.g. to show whether the study sites selected for analysis are representative for the general population of mountain glaciers in SV, NZ and FJ. Such an attempt will be presented in the revised version of the manuscript.

Comment #2 – glacier surges, common in SV and NZ, are not discussed in the manuscript

Obviously, this will be corrected. However, as far as I know, no glacier studied in the manuscript has surged within the past decades, so this dynamic instability likely does not play a significant role in geometry changes of the study glaciers.

Comment #3 – outcomes of mass balance models, e.g. van Pelt et al. (2019) and Noel et al. (2020) would be useful to give an insight into the processes behind the observed glacier changes

I agree, this will be elaborated in the revised version of the manuscript. The excellent work by van Pelt et al. (2019)(or "vP19") was omitted by a mistake and this will be corrected. However, interpretation of the mountain glacier changes observed in my analyses in the background of vP19 outcomes would require caution. Note that vP19 calculates generally positive long-term balances in many areas dominated by mountain glaciers in SV, e.g. central Nordenskiold Land, Bunsow Land and Nathorst Land, being in contrast with their rapid retreat over the past decades. This is quite common for regional mass balance models and that is why I personally consider regional simulations the best for giving an overall picture, rather than for providing adequate reproduction of details, such as the mass balance of small mountain glaciers.

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In the end, your work, including linguistic corrections, is greatly appreciated.

Kind regards,

Jakub Małecki