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The Cryosphere Discussions

Interactive comment on "Multi decadal glacier area fluctuations in Pan-Arctic" *by* S. H. Mernild and J. K. Malmros

Anonymous Referee #1

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General Comment

I found this paper to be poorly written, scientifically problematic, and displaying a remarkable lack of awareness of prior literature that addresses the same issues as it does but in a much more comprehensive manner. This literature is comprehensively summarized in chapter 7 of the 2011 SWIPA (Snow, Water, Ice and Permafrost in the Arctic) report published by AMAP and available for free download from the AMAP website. For a number of regions discussed in this paper (Yukon, Canada's Queen Elizabeth Islands, Axel Heiberg Island, Bylot Island, parts of the Russian Arctic for instance) there are published analyses of recent rates of glacier area change that are based on analysis of every glacier in the regional population (not a small sub-sample as is the case here). Although there are some differences in the time periods covered, there are

a number of cases where the regional rates of area change derived from analysis of the full ice cover differ radically from those reported here. I think this is pretty clear evidence that the subsample of glaciers used in this study is not regionally representative, despite the authors' claim to the contrary. This likely reflects fundamental flaws in the argument that they use to justify the representativeness of their sample.

There are a number of other areas where I have serious concerns about the work presented here, and I will discuss these under "specific comments" below.

Specific Comments

1. The whole analysis presented here is predicated upon the assumption that air temperature is the only climatic parameter that affects glacier surface mass balance and area change (which is manifestly untrue for a number of the areas considered (Kamchatka, western North America, southern Alaska, southern Greenland for instance, for which available mass balance measurements clearly show a sensitivity of mass balance to variations in winter precipitation) – or on an implicit, but unstated, assumption that, if precipitation is important, it is directly correlated with mean annual air temperature and does not therefore need discussion (an assumption that, at the very least needs to be demonstrated to be true).

2. Since the authors only ever discuss changes in mean annual air temperature, they need to demonstrate that this parameter is well correlated with mean summer air temperature, which is what most directly affects the summer and annual mass balance across the drier parts of the Arctic (northern Canadian and eastern Russian Arctic, for instance).

3. Many of the sub-regions considered are extremely large, and there are noticeable regional variations in the degree and even timing of recent warming across these regions. Where complete analyses of glacier area change have been conducted, these variations are apparent in the record of glacier area change. Unless the selection of the sub-sample of glaciers chosen to be "representative" of glacier area changes in a given

region respects these patterns, it will inevitably be biased relative to the population as a whole.

4. In a number of these areas, a not insignificant number of glaciers have actually disappeared completely within the past few decades. Only one such glacier is incorporated in the analysis presented here – so this is another uncorrected bias.

5. The authors state that the number/size distribution of glaciers included in their study for the whole pan-Arctic matches that for glaciers in the same overall geographic area in the Randolph Glacier Inventory. In fact, Figure 4 clearly shows that the largest ice masses in the region are totally omitted from the analysis and that the size distributions of the population and the sample deviate quite significantly. Regardless, the more important point is that the number/size distributions of glaciers in the different sub-regions may differ significantly from that in the population as a whole, and the authors have failed to demonstrate that their sub-regional samples have number/size distributions that are concordant with those of the sub-regional populations. Given the enormous climatic and mass balance regime differences between some of the sub-regions, this issue simply cannot be ignored.

6. Whilst glacier surface mass balance responds to climate change on an annual time scale, the same is not true of glacier area change, which responds to climate trends integrated over significantly longer periods of time. There is no obvious reason to assume that recent rates of area change are directly (or solely) linked to climate trends over the period for which area changes were measured.

7. Factors like glacier elevation, hypsometry, slope, and shape will also influence how glacier area responds to climate forcing. The authors fail to show that their sample glaciers are representative of their regional populations with respect to any of these parameters.

8. The discussion of why small glaciers lose a greater proportion of their area in a given time period than large ones misses the obvious point that small glaciers likely

formed in locations that were climatically marginal for glacier growth, while large ones formed in more favorable locations. If so, it is hardly surprising that small ones shrink more rapidly if climate cause the mass balance to become more negative.

9. On page 10, the majority of the regions that have a relatively high proportion of glaciers that grew over the measurement period are found in regions of high precipitation. To me this strongly suggests that factors other than air temperature are driving glacier area change in these regions.

10. On page 12, the authors refer to the pentadal regional mass balance estimates of Cogley (2012). Bear in mind that since these estimates are based on interpolation of measured mass balances to all glaciers in a region using an inverse distance weighted method, they can be extremely unreliable in regions where there are no local measurements. This would be true of the Russian Arctic, for instance, where the regional balance signal derived by Cogley carries the signal of mean summer air temperature change in northern Scandinavia and Svalbard, even though this signal is not apparent in air temperature records from the Russian Arctic. This issue is also discussed in the SWIPA report.

Technical Corrections

In my view, the degree of revision of this manuscript (and more importantly the work on which it is based) that will be required to address the scientific concerns outlined above is such that it is not worth devoting time to highlight the widespread deficiencies in the English in which it is written. Suffice to say that if the authors do submit a revised version they should be sure to have it reviewed and edited by someone who is fluent in scientific English before resubmission.

I will, however, note some second order scientific issues that need to be addressed. These are listed below, referenced by line number.

57: Why not refer to the geodetically-based regional mass loss estimates of Jacob et

al. (2012)? The Radic and Hock paper referred to provides a modeled estimate, not a measured one.

59 and elsewhere: the name is Bolch, not Bloch.

78: are grows or increases, it does not advance!

158: How do you know the errors were underestimated?

164-166: sentence makes no sense as written.

231: Not convinced Figure 8 serves any useful purpose.

244-245: whilst it seems only logical that glacier advance is a response to positive mass balance and (cooling/wetting?) climate, it isn't clear to me that you can actually prove this for any of these cases.

Interactive comment on The Cryosphere Discuss., 6, 4417, 2012.