

## ***Interactive comment on “A new bed elevation dataset for Greenland” by J. A. Griggs et al.***

**Anonymous Referee #3**

Received and published: 15 January 2013

This work proposes an updated digital elevation model of the bedrock in Greenland. Previous version was computed about 10 years ago. Since that time, very large efforts have been deployed to survey the bedrock underneath the ice-sheet, and for some specific outlet glaciers a very dense coverage is now available. Publication and distribution of a new version of a gridded dataset is of primary importance, as most of the coastal regions were so far described with not enough accuracy, particularly the large coastal trenches that strongly constrains the ice flow. This work is of significant importance and must be published hopefully in the shortest delay.

I however have essentially one remark and three suggestions regarding the paper. (i) bedrock elevation underneath the ice-sheet is deduced from the measurement of ice-thickness and subtracted to the elevation surface. As clearly mentioned by the authors, surface elevation is not always acquired during thickness measurements and a specific DEM of the surface elevation have been produced and used. This DEM is

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static in time, whereas the thickness measurement have been done over more than 2 decades. During that time, some coastal glaciers have considerably thin (>100 m) and may significantly impact the estimated error (about 20 m in some of these regions). This issue is not addressed at all and I believe that a paragraph explaining how the none concomitant data have been handle to produce the final product would be of important added value. (ii) The authors also build a surface DEM and a thickness raster. These 3 datasets are consistent and some users will have to used them all together (almost a necessity for ice-sheet modeling). Thickness and elevation are not presented in the paper. I really would have appreciate to see the 3 rasters gather together and shown in the figures. (iii) section 4.1. I do not really see the added value of presenting the hydraulic potential. This is to me a bit out of the scope of the paper, and this work does not need a succinct application to demonstrate its great significance. I would suggest to remove that section and corresponding comments in the other part of the manuscript. (iv) After computing this excellent dataset, the authors are probably the persons who have the best overview of where new data should be acquired in priority. One sentence somehow mentioned that (p. 4843, l. 12) on one specific region. I think it would be very pertinent to highlight more regions that deserve survey. I agree that priority may be somehow subjective (e.g., and ice core project needs a dense survey locally whereas the study of a particular outlet glacier will prioritize somewhere else). Anyway, I think that it would make sense to look at that in a general terms of better inferring the ice dynamics and discharge in the future. I also believe that using surface velocity and the estimation of the error, they could build an objective index of where information are lacking. I think it would be of great added value to the paper without so much supplementary efforts.

Some minor comments and suggestions : - Abstract line 4. “majority of this having been collected”, is it 50 or 90 %? giving a percentage would make sense. - section 2.1, airborne datasets. As I understand radar used by CReSIS since 2010 is part of OIB, and PARIS is also in OIB. So I do not clearly understand the motivation behind the classification, is it the operator or the tool... It is apparently a mix in between. I would

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suggest to sort by type of radar and of course mentioning the different contributors. Figure 1 should correspond to the proposed classification. - section 3.2. A threshold of 2000 m of the surface is used and two different treatments are then used. To me choosing a threshold on the surface properties when dealing with the bedrock looks a bit awkward, something like the density of measurement would have sounds more natural. Could this arbitral choice be a bit more discussed (sensitivity)? - section 3.2.1 and 3.2.3. I did not clearly understand how the bathymetry is handled below the ice-shelves. I think that more details could be given, -section 4, p. 4842, l. 10. Which ellipsoid? I guess wgs84, would be could to detail. - section 4.2, p. 4845, l. 22. Here again a threshold of 2000 m is used. I basically have the same comment than for section 3.2: how sensitive are the result to this arbitrary choice? Probably mpore important, how the estimation on Petermann glacier can be extended to all other coastal regions? Is it really robust? - section 4.2, p 4845, l. 28. As I understand the sentence from p. 4837 l. 15, thickness is set to zero. This is not that clear here, so finally I am not sure to understand what has been exactly done. - section 4.2, p 4846, l. 1. To my understanding the Bahr et al. relation makes sense when dealing with a lot of glaciers, I am not convinced that it could be used to improve a DEM. - conclusion, p. 4846, l. 23. 1.5% of ice sheet below sea level. This must be a typo, this is clearly in contradiction with visual inspection of figure 3.

I am extremely thankful to the authors who accepted to provide the data before publication. This allowed me to check the quality of the data and more particularly how easy it was to handle them. I would then have few small remarks and suggestions on the dataset. - Data are provided in a netcdf format. This file is nicely processed with the projection informations and x-y axes included. This format is generally very pertinent for the modeling community. However I would suggest to also provide the rasters in Geotiff format which would facilitate the implementation in any GIS. I believe that it is all the more pertinent because the chosen projection does not have, to my knowledge, an EPSG code. - NumberAirbornePoint. Apparently a default value of 1 has been attributed over the none-modified bathymetric data. My feeling is that it should be zero

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outside the landmask. - bedrockElevation\_Unprocessed. Apparently there is an artifact in western Island. - LandMask. I would have expect that it would consistent with the surface DEM. Part of Island, canadian arctic archipelago and Svalbard are present. I would suggest to remove them, or to have them on the same extend than the bedrock DEM.

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Interactive comment on The Cryosphere Discuss., 6, 4829, 2012.

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