Response to referee 3 comments.

Below we detail how we have responded to the referees' comments. In addition we provide, as a supplement a highlighted copy of the revised text to show where changes have been made. We are very grateful to the referees for their thorough, careful and constructive comments and suggestions.

(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 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.
This is a good point and we have added some text to the m/s to explain precisely what we did to correct for this, which is as follows. For most of the data volume (74%) dH/dt is not relevant because a LIDAR was flown simultaneously with the IPR providing reliable surface elevation estimates. This was the case for CRESIS and HICARS. As described in the paper, for every IPR point, all LIDAR footprints in a 2 km area on the same day are collected and the mean is calculated. If none are available the time span is extended to two days. This succeeded for 84% of all CRESIS data and 66% of HICARS data, making up 74% of all available data.
If no concidental LIDAR footprint is available, the GIMP dem is used where the dH/dt issue comes into play. The exact time stamp of GIMP is unclear as it is a mosaic of images. However, Jakobshavn, Helheim, and Kangerdlugssuaq glaciers are based on imagery from 2007, and Petermann glacier from 2003 (personal comm; I. Howat). As the largest dH/dt s are expected over the big outlet glaciers, a dH/dt correction was used for these areas and for all other areas GIMP was used as is. dH/dt values were taken from an ICESat based dH/dt estimate for Greenland, interpolated to 1km resolution (Hurkmans et al., submitted). Annual dH/dt values were added for years between the IPR data point and the time stamp for the area of interest. Assuming GIMP is representative for broadly the middle of 2003 and 2007, respectively, and we know the month of the IPR measurement, the appropriate fractions of the dH/dt values for the GIMP year (2003 or 2007) and the IPR year are taken into account. Of the 26% of data points for which GIMP was needed, 22% are located over one of the four big glaciers and is dH/dt corrected. Part of the remainder (eg nearly all AWI data) is in the interior where dH/dt values are very small (cm/yr).

(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.
The surface DEM was touched upon by referee 2. See our response there. We do not discuss this product and a detailed publication is in preparation on the GIMP DEM. Our focus here is on the bed. We agree that the thickness data set could be presented and have added it as a new panel in Fig 3, alongside some additional text.

(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.
Agreed. Done.

(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.
Agreed. Have added additional text on this in discussion.

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
Agreed. Added.

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 suggest to sort by type of radar and of course mentioning the different contributors. Figure 1 should correspond to the proposed classification.
We agree. This is now done and all CRESIS data are grouped together.

- 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 (sensitivty)?
We agree. Please see response to referee 1 on this point. Error analysis and interpolation are now handled with respect to bed properties not surface.

- 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,
Again see reply to referee 1. We have added additional 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 im- portant, 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.
All these points are now redundant as we no longer use 2000 m contour. See reply to referee 1.

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.
Clarified.

- 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.
Perhaps. The scaling law is applied to a variety of different glacier sizes and shapes but it does not preclude a similar sort of approach (i.e. volume/area scaling in some form) for other ice masses such as ice caps.

1.5% of ice sheet below sea level. This must be a typo, this is clearly in contradiction

with visual inspection of figure 3.
Corrected error also spotted by another reader.

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.
This is a good suggestion and we have also produced a geotiff of the bed elevation data set. We are, however, a little reluctant to distribute this as is because geotiff does not allow the provision of all the ancillary data and metadata that we provide in the netcdf file. We believe it is important that the use has all these data, including the mask, sampling density, error map and so on. It would be a challenge and a lot of extra work to provide all of these as geotiffs and ensure that the user had the necessary metadata for all of them. A lot of time and effort went into ensuring all the information needed by a user is available if they need it and netcdf was the most effective means of doing this.

- NumberAirbornePoint. Apparently a default value of 1 has been at- tributed over the none-modified bathymetric data. My feeling is that it should be zero outside the landmask.
We will check this and see if this is a mistake.

- bedrockElevation_Unprocessed. Apparently there is an arti- fact in western Island. –
Ditto.

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.
Will check and ensure consistency.