

## General comments

The GIMP data set is a fantastic resource for anyone studying the Greenland ice sheet and clearly a lot of work went into producing it. This paper is as far as I am aware the only one that describes the development and accuracy of the data and is therefore a useful contribution. However, what was missing was any real interpretation of the data sets and this is reflected in the conclusions section which is not a conclusion at all but a description of how to access the data and the next phase of the project. I understand that this is a paper describing the generation of a data set but it would not have been a lot more work to have included some analysis of the data set that indicates the value of the improved resolution and accuracy. Has the area of the ice sheet changed from previous estimates (will come back to this below), for example? Do drainage basin boundaries change with the new data or is it easier to identify them (compared to say Rignot and Kagnaratham 2006)? What about a difference plot between this and say the PEB DEM to show how big some of the differences are between the two etc.? With a little more effort, the value of the data set would be much more apparent and would sell itself much more than it does now. There is little that is controversial in this paper and the methods are well described. I just feel more effort could have been made in presenting and interpreting the products described.

## Specific comments

P3, l8. Excessive use of jargon/acronyms. What is ESPG. Why not just spell it out. You only use it twice. Ditto P4 l5: LTK?

There are other cases where the acronym is not needed and would make it easier to follow if it was not used. Section 5.6 is particularly acronym heavy.

Section 4: masks. Nowhere do you state what you define as glacier ice and whether you have separate classifications as per Rastner et al for example. need to make this clear.

Section 5.2 PEB DEM. The PEB DEM is derived from elevation data collected around 1995. ICESat data is from 03-09 so there is a >10 year gap between the two. Some, possibly significant, component of the RMS **difference** (not error) will be due to the elevation change between the dates of acquisition. Is it not possible to exclude areas of large elevation change from the analysis (e.g. anywhere where the velocity is >100 m/yr or where ICESat measures rates greater than some value say). This would be a more useful and informative measure of the RMS difference.

One can only talk about RMSE if what is being compared to has no error or is considered to be a stationary reference. This is not the case here, but you could possibly make this case if you removed all the areas with large elevation rates.

P9 l20-25. There is a contradiction here. l20 refers to the ice sheet RMS but later you discuss ice-free terrain as inflating the error. If it is the ice sheet RMS then surely the ice free terrain has been excluded from the estimate?

P14, l2 which less -> which is less

P14, l25 cause in DEM -> cause DEM

Section 6. This is not a conclusion. It is a description of how to get the data and what you plan to do next. This does not sell this wonderful product well and does all the hard work that went into producing it something of a disservice.

P26, Fig 7. Need to annotate figure with arrows showing artefacts. They are not obvious to a non expert.