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The Cryosphere Discussions

Interactive comment on "A new 1 km digital elevation model of Antarctica derived from combined radar and laser data – Part 2: Validation and error estimates" *by* J. A. Griggs and J. L. Bamber

Anonymous Referee #2

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Reliable topographic data are needed for a better understanding of the recent evolution (mass balance) and reliable prediction (modeling) of future changes of polar ice masses. Contrary to the "temperate" regions on Earth [56S to 60N] that have been nearly completely mapped by the Shuttle Radar Topographic Mission in February 2000, polar scientists still lack a comprehensive DEM in their region of interest. Some recent projects or future satellite missions will help to fill this gap in our knowledge of the polar topography [SPIRIT based on Spot5-HRS data, ASAID based on Landsat mosaic & ICESat, Tandem-X from the DLR, G-DEM from the ASTER science team or, of course,

the Cryosat-2 mission]. Right now, the best mean to obtain a comprehensive topography of the Antarctic ice sheet is probably to combine data from two altimetric missions: the dense geodetic mission of ERS-1 and the less dense but more accurate profiles from the ICESat satellite. By combining these two datasets, the authors produce what is currently the best DEM of Antarctica with a 1 km resolution (part 1). They have made a great effort to gather an extensive and precise reference dataset (spanning all types of terrains existing in Antarctica) to validate their new 1-km DEM. This extensive and thorough accuracy assessment is the topic of this companion paper (part 2) and leads to a complete error map that is certainly going to be useful to the users of the 1 km-DEM.

Given the importance of this new DEM, its associated error map and the fact that the paper by Griggs and Bamber is well-written, I recommend publication in The Cyrosphere if the comments below are addressed by the authors.

Note: When reviewing part 2 of an article, it seems difficult to completely ignore its first part so that I have also a few comments about part 1 right at the end of the present review (I hope it is not too late, I assumed NO given that two companion papers should be accepted -or rejected- at the same time).

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GENERALS COMMENTS

Should the article be split in two parts? I guess the decision will be made by the editors themselves. As far as I am concerned, I read part 2 first to make sure that it could be read and understood independently of part 1. This is the case. The amount of work is also very significant and deserves two articles. However, there are some problems with this article being divided into two parts (particularly in Part 1). Most of the abstract / Discussion / Conclusion in Part 1 concerns part 2 !!!

Will the new dataset (including the error map) be available for free to any external user?

TCD

2, S520–S527, 2009

I hope so and if this is the case, it would be useful to indicate in the present article (or in part 1) what procedure should be followed to obtain the DEM.

Planimetric errors are never really discussed. Although I understand they are difficult to quantify for a moving ice-covered area, they should be discussed. You mentioned the pointing uncertainties for ICESat. What about those for ERS?

Error map. You should clearly indicate that what you model here is the RMS error and not the bias or the standard deviation (Am I right?). Why did you choose to model RMS error instead of standard deviation (the statistic that you mainly used in the article to describe the dispersion of your elevations)?

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SPECIFIC COMMENTS

In your introduction, I suggest to add some references that are some relevant examples of how the ice topography can be used to derive other important variables. It would reduce the percentage of auto-citation among the reference list which, I feel, is quite high in these two articles.... Also, Budd & Warner (1996) paper should be cited when balance velocities are involved...

Smith, B. E., C. F. Raymond, and T. Scambos. 2006. Anisotropic texture of ice sheet surfaces. Journal of Geophysical Research 111, F01019, doi:10.1029/2005JF000393

Remy, F.D., Shaeffer, P., & Legresy, B. (1999). Ice flow physical processes derived from the ERS-1 high-resolution map of the Antarctica and Greenland ice sheets. Geophysical Journal International, 139, 645-656.

Budd, W. F. and Warner R. C. (1996). A computer scheme for rapid calculations of balance-flux distributions. Annals of Glaciology.

P846. Line 10. Could you clearly indicate here the sign of the difference that you computed (1kmDEM-Airborne) or (Airborne-1kmDEM) so that we can easily interpret

TCD
2, S520–S527, 2009

the biases? It is never 100% clear throughout the manuscript.

P846. Also a short description of the statistics that you will use is welcome at this point in the paper. Define the modal bias (and why it is useful). Any points in giving both RMS difference and standard deviation? If yes explain why? A bias + a standard deviation contain more information than the RMS. FWHM...??? It is not really a standard statistics for glaciologists...

P847. 1st paragraph? The sign of the bias are not clear on the peninsula. +46 cm; +26.2 m and the overall -1.51 m. Also for the Peninsula, the lowering of the surface upstream of former Larsen B ice shelf [Scambos et al., 2004] in the order of 10s meters/yr may have a significant impact on your biases.

P847. Line ˜20. Showing some profiles could help to clarify/validate your hypothesis.

P848. Line 25. Here also some profiles would be welcome.

P849. Last line. Your bias of -4.8 m suggests a thinning. But Kamb Ice Stream is thickening... Why this discrepancy (probably just a matter of convention)?

P852. Line1-5. Cannot you use ICESat repeat profiles (that cover now about 6 years) to test your hypothesis of moving surface features/undulations?

P854. Line 13. What metric did you use to account for the deviation from the quasi-regular grid? I guess the distance between the center of regular and quasi-regular pixels. Define it clearly.

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TECHNICAL COMMENTS

I suggest to add "radar and laser altimetric data" in the title

P845. Line1. "high spatial resolution". The 5 km footprint of ERS is not exactly what I would describe as a high spatial resolution. Do you mean dense coverage?

TCD

2, S520–S527, 2009

P845. Line25. Could you give here the time stamp of the 1 km DEM? Even if this is discussed in the first part it needs to appear here.

P846. Line 1. Cite Csatho et al. 2005 (thickening Kamb Ice Stream)

P846. Line 23. Figure 2 shows the difference between the CECS/NASA data for the Antarctic Peninsula. –> Figure 2 shows the difference between the CECS/NASA data and our DEM for the Antarctic Peninsula.

P848. Line 15. Can you provide also the standard deviation for AGASEA at cross-overs?

P851. "in area south of 81.5°"; –> "in area North of 81.5°S";

P852. Line1. Difficult to locate the blue track in Figure 5...

P854 Line 18. "The further any DEM gridbox"; –> "The further any interpolated DEM gridbox";

P855 Line 14. which each contains (add s)

P856 Line 4. Remove "of" exist.

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TABLES

Table 2 and 3. The caption could probably be improved.

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FIGURES

Histograms range between -100 m and 100 m. Is it simply a cosmetic choice or did you really consider those points where the absolute differences were greater than 100 m as outliers?

It is difficult to identify the color code on the map (it is OK on the computer screen but

TCD
2, S520–S527, 2009

impossible on a hard-copy). Can you improve that (e.g. make the profile thicker)? The histogram should be larger. In some cases (see my specific comments) it may be wise to also show some profiles of elevation difference (for instance in the same plot you could show the absolute altitude from airborne data and the elevation difference on a secondary axis).

Figure 2. Why do you have some suspicious spikes in the histogram for the Peninsula?

Figure 6. Nice demonstration!

Figure 8. Could you add on a different vertical axis the absolute altitude from SOAR?

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PART 1: Only a few GENERALS COMMENTS

As stated in the introduction of my review, there are numerous initiative/future satellite missions to improve the knowledge of the topography of the polar ice masses. You could mention these initiatives (intro or in the outlook of your conclusion). I guess that ASTER (Stearns & Hamilton, 2007), SPOT5 (Korona et al., in press) or CRYOSAT2 will be especially relevant close to the coast where your errors (and the spacing of the satellite track) are largest.

ABSTRACT.

"Near the ice sheet margins and in other areas of steep relief the SRA data tend to have relatively poor coverage and accuracy". Did your work really improve the DEM in the margins given the spacing between ICESat tracks (20 km at 70S compared to 2 km for SRA)? It is not really clear in the article that there was an improvement compared to SRA DEM close to the coast.

"The accuracy of the .... 86°S"; These two sentences refer to part II and not part I.

INTRODUCTION

TCD
2, S520–S527, 2009

You could also add that a good DEM is needed during the processing of satellite (SAR or optical) images. See InSAR for instance.

DISCUSSION

I wonder whether this discussion is needed. Right now it is rather poor. The first part of the discussion concerns the error (so mainly discuss part II of the article...). Then there is a long paragraph about the accuracy at the grounding line and how a new tide model is going to be taken into account, however these are more some perspectives and we do not learn much from this paragraph. I do not really see what you really mean.

CONCLUSION

Once more, half of the conclusion of part I is about the error assessment which is the topic of the companion (part 2) paper. So it raises the question whether these two companion papers should be split like that?

Good luck with the revision of your work,

REFERENCES for my review

Korona J., Berthier E., Bernard M., Rémy F. & Thouvenot E. SPIRIT. SPOT 5 stereoscopic survey of Polar Ice: Reference Images and Topographies during the fourth International Polar Year (2007-2009). ISPRS J Photogramm, in press, doi:10.1016/j.rse.2008.09.015, 2009

Scambos, T. A., J. Bohlander, C. Shuman, and P. Skvarca. 2004. Glacier acceleration and thinning after ice shelf collapse in the Larsen B embayment, Antarctica. Geophysical Research Letters 31, doi:10.1029/2004GL020670

Stearns, L.A., and G.S. Hamilton, Rapid volume loss from two East Greenland outlet glaciers quantified using repeat stereo satellite imagery, Geophysical Research Letters, 34 (5), L05503, 2007

TCD

2, S520–S527, 2009

Interactive comment on The Cryosphere Discuss., 2, 843, 2008.

TCD

2, S520–S527, 2009

Interactive Comment