The Cryosphere Discuss., 2, S586–S591, 2009
www.the-cryosphere-discuss.net/2/S586/2009/

The Cryosphere
Discussions

TCD

2, S586–S591, 2009

Interactive
Comment

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

J. A. Griggs and J. L. Bamber

Received and published: 9 April 2009

We would like to thank referee 2 for their detailed and insightful review of our manuscript. We are in agreement with all points made by the referee and have altered the manuscript accordingly which we believe to have considerably improved it.

We respond to each point in turn.

General Comments

1) Splitting the article into two parts. We initially wrote the manuscript as one part but it quickly became unwieldy. The material presented in each part is very different

and distinct in nature, the first part being very methodological and glaciological and the second being more statistical and technical. We believe that the two articles each stand alone and we are pleased to read the referee's comment that on reading part 2 without initially reading part 1, they were able to follow the manuscript. The comments about the abstract etc of part 1 concerning information presented in part 2 are addressed at the end of this response.

2) Free availability of the dataset. Yes, both the DEM and the error map will be deposited with the NSIDC so that they are freely available to all. We have added a sentence to the conclusions (and to part 1) to confirm this.

3) Planimetric errors. We do not explicitly discuss planimetric errors. We quote the pointing error for GLAS but as ERS is a pulse limited instrument, planimetric accuracy is a bit meaningless. Geolocation error in the final DEM is implicit in our error analysis as any geolocation error will fold into the vertical accuracy assessment. It is clear, particularly from the SOAR analysis in figures 6 and 7 that geolocation error does not contribute a significant error to our DEM as this would be visible as constant biases over smooth surfaces.

4) Error map - RMS. We model RMS as we model the random component of the error in the DEM. The airborne validation shows that systematic error is small and not uniformly distributed with validation campaign. We have included a sentence explaining the choice of modelling random error rather than systematic. We use RMS as our measure of random error in the model as it has no implication of a Gaussian distribution in the errors. In the text we talk about RMS and standard deviation equally and we have added a section talking about the statistics used and their applicability as part of the introduction to the validation.

Specific Comments

1) We have added references to applications of DEMs as suggested

TCD

2, S586–S591, 2009

Interactive Comment

2) We have included a description of the types of statistics we use to analyse the validation data including a discussion of why we use a whole suite of differing metrics. As we discuss in the text, we display both standard deviation and RMS for compatibility with other studies, primarily Young et al (2008) in their comparison of the AGASEA data with older DEMs of Antarctica, and as, while standard deviation is commonly used in the glaciological community, random errors may not have a Gaussian distribution and so FWHM or RMS difference may be more appropriate metric (Bamber and Gomez-Dan, 2005).

3) The differences on the Peninsula, as in all areas, are changed slightly due to the new interpolation method applied (ordinary kriging as opposed to tension spline interpolation, see the new description in part 1). These bias are now +.48m for the shelf, -9.49m for the highly sloping region south of the shelf and +1.08m for the entire peninsula. The separation into different area is intended to show the range of possible bias on the Peninsula where input data in the DEM is very sparse and large negative and positive departures from the airborne data are seen. Our mask ensures that there are no data from Larsen B other than the region which still remains and can be seen in the MOA imagery

4) We have added a figure showing a profile across the Thwaites glacier mouth.

5) We have clarified the text to emphasis that we were making the point that we are less certain about the DEM over sharp features then the rest of the region due to their small scale and the lack of ERS data in break-in slope regions. However, the airborne data also doesn't capture the features fully and there is not enough data to produce a profile across any such region. The increased uncertainty is confirmed by the increased errors in the error model for these high slope features.

6) We have clarified in the text to be more explicit that our differences are always in the sense airborne-DEM. Therefore a negative bias in this region indicates that the Kamb ice stream has thickened as the airborne data has an earlier timestamp than the DEM.

TCD

2, S586–S591, 2009

7) More exploration of small scale features and thought about this issue leads us to think that the large ground footprint of the ERS data (about 4 km) and the smoothing by the interpolation, which will include some influence of surrounding data, are far more likely causes of the differences seen. We would expect small scale surface features to have moved in the time periods over which our data were recorded, but we do not think that we are able to fully resolve these features and so will not capture their movement. This is clear from the fact that all DEMs miss the same features (figure 9) even though the other DEMs use the two instruments separately and are representative of different time periods.

8) We have clarified the metric used to describe the deviation of the final interpolated DEM from the quasi regular grid in the text. It is simply the absolute difference between the final DEM grid box and the quasi regular gridbox centre in gridboxes where satellite data exists.

Technical Comments

1) We have changed the text to reflect all grammatical and technical comments made.

Figures and Tables

1) We have altered the table captions.

2) We do consider 100m absolute difference to be outliers as they are uncommon. We display the histograms out to +/-100m as well as it enables the reader to see that the comparison in the Peninsula, in particular, has a wide spread and long tailed distribution.

3) The spikes in the histogram were caused by some ocean viewing airborne data entering out comparisons. These data have been removed from the analysis.

4) Thanks! We felt it important to emphasis why the particularly rigorous approach to the creation of the DEM is a necessity in attempting to produce a DEM with minimal errors.

TCD

2, S586–S591, 2009

Interactive Comment

5) We have included the absolute elevation for the airborne data.

Comments on Part I of the paper

We appreciate the additional time and effort this reviewer put into reading and commenting on paper I. Their comments were incorporated in the revised version of paper I as discussed below.

Discussion of other relevant missions and activities. The introduction has been updated to include other research in this area, planned and/or underway.

Abstract: Did our work improve accuracy near the margins. The answer to this question is yes. Although the ICESat data have relatively coarse spacing, they have much lower (up to about factor 20) errors compared with the SRA data. Our interpolation procedure uses values weighted by their error. Thus the ICESat data have a higher weighting and reduce biases toward the margin, even away from the data points/tracks, as a consequence of the interpolation methodology used.

..The accuracy.. refers to part II and not part I. This is true but we include, in paper I, a discussion of the improvement in accuracy of the DEM compared with earlier versions, referencing paper II where appropriate. Although this statement is not proven in paper I it is discussed and, therefore, we considered it suitable for inclusion in the abstract.

Is the discussion needed? The referee believes the discussion is 'rather poor' with a 'long paragraph about the accuracy at the grounding line'. We have revised the discussion a little, removing the components on future work. The aim of the discussion is to illustrate how the new DEM can improve calculations/investigations of a number of factors such as mass balance (where grounding line accuracy is critical), ice divide locations (for regional mass balance), for calculating balance velocities and for delineating surface features related to basal topography/processes. These are all studies that have utilised this, or earlier, DEMs and we believe the discussion shows, quantitatively and graphically, the improvements achievable with the new DEM. We believe this

TCD

2, S586–S591, 2009

is important to demonstrate.

Conclusion: half the conclusion is about error assessment. This is true but, as mentioned earlier, we discuss in paper I, earlier DEMs and their accuracy, which is why we have included these sentences in the conclusion. We could, indeed, have transferred this discussion to paper II and there are arguments for and against both splits. We believe, however, both papers can be read stand-alone but also benefit from each other, as would be expected of a part I and part II approach.

References discussed in the response to the reviewers comment.

Bamber, J., and Gomez-Dans, J. L.: The accuracy of digital elevation models of the Antarctic continent, Earth and Planetary Science Letters, 237, 516-523, 2005.

Young, D. A., Kempf, S. D., Blankenship, D. D., Holt, J. W., and Morse, D.: New airborne laser altimetry over the Thwaites Glacier catchment, West Antarctica, Geochemistry Geophysics Geosystems, 9, Q06006, doi:06010.01029/02007GC001935, 2008

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