

Review of Thota et al.: "Digital Elevation Models and Orthomosaics of 1989 Aerial Imagery of the Western Antarctic Peninsula and Surrounding Islands between 66-68°S"

Summary

The authors present a novel data set of Digital Elevation Models and Orthomosaics generated from historical aerial imagery collected over the Antarctic Peninsula using a workflow adapted from the Historical Structure from Motion Package (Knuth et al. 2023). The DEMs are calibrated and independently validated against modern high-resolution satellite optical stereo (REMA) and laser altimetry (ICESat-2) observations over non-glacierized areas. The historical dataset will be very valuable in extending the historical mass balance observation record over this data scarce region. I have some comments which the authors can consider to clarify some issues in the manuscript, and I am looking forward to published manuscript post revisions.

Major Comments

The manuscript organization can be streamlined with clear division between the methods and results and discussion. There are some places where new experiments are introduced in the results section. I have detailed those at specific occurrences in the general comments below. Reorganizing the sections properly will go a long way in improving the readability of the manuscript.

General Comments:

- In the first figure, would it be possible to enlarge the Antarctica-wide subfigure, and add star markers to locations for which previous studies have provided historical mass balance, as described in the introduction literature review? It will provide context to a wider set of audience, and clearly establish the knowledge gap being filled by the new data set and manuscript.
- In auxiliary data section, confirm if the REMA strips are first co-registered and then mosaicking is, or the mosaic tiles are co-registered to CryoSat/ICESat. Currently it is mentioned that the mosaic strips are co-registered to altimetry observations.
- Page 6, Line 140: I am not sure if we can compute base to height ratio/convergence angles from pixel reprojection errors. If the camera parameters are estimated well, the reprojection error can be very well 0. Maybe this was a typo in the sentence, please check and consider modifying.
- First sentence in the paragraph on line 145 is unclear: we should define better what is meant by poorly localized reprojection error, and why were the sizes of these tie points larger than the others which caused this issue. Please clarify and revise.
- The description of the uncertainty estimation between 185 to 190 is a bit repetitive. Consider describing the Seehaus et al. 2019 approach first, which is unchanged for both cases. Then just mention you did a 50 m outlier filter on elevation residual additionally on the altimetry

measurements before applying the general Seahaus et al. approach to get rid of outliers due to clouds.

- The image quality section in the first result section describing the Shanon index is a bit out of place. It should be mentioned first in the methods. And the results & discussion sections should only describe the results and interpretation, not a new method.
- Why is Section 4.3.1 stated as “relative accuracy”? In general relative accuracy would refer to residual values between maybe the overlapping regions of the historical DEM strips. In comparing against external reference like REMA, should this not be absolute accuracy?
- The description of the variogram analysis could be improved. Like the previous comment, the experiment setup should be mentioned in the method section. In the description section, some interpretation of the the three ranges and sills should be provided. Some speculative points on why was a range upto a particular value on the three different scales, what do we understand from the different sill measurements would be useful.
Right now there is only a comparison with a double nested spherical model and not much weight is given to the main 3-variogram result presented in the manuscript.
- Reviewer 1 raises a good point about the degraded IS-2 accuracy in areas with high slopes and surface roughness. It would be good to acknowledge this in the discussion section. Also refer to papers by Schenk et al., 2022; Csatho et al. 2024 and other members of IS-2 science team etc on this topic.

Note: I did not download the data for evaluation. This review is for the science manuscript only. As a note, I did not read the already available reviewer comments before I completed the first draft of my review.

Looking forward to the revised manuscript,
Shashank Bhushan

References

<https://ieeexplore.ieee.org/stamp/stamp.jsp?arnumber=9698042>
<https://www.sciencedirect.com/science/article/pii/S0034425722004850>
<https://tc.copernicus.org/articles/13/2537/2019/>
<https://isprs-archives.copernicus.org/articles/XLVIII-3-2024/83/2024/>