**The Cryosphere
Discussions**

Interactive comment on "Refined broad-scale sub-glacial morphology of Aurora Subglacial Basin, East Antarctica derived by an ice-dynamics-based interpolation scheme" by J. L. Roberts et al.

Anonymous Referee #2

In this paper, the authors do a nice job of pursuing the practical aspects of the technique introduced by Warner and Budd in 2000 for interpolating ice thickness values from surface-elevation data grids. By presenting schemes to find local optimum values of c, and by evaluating the results of their schemes, they bring the Warner and Budd much closer to being a useful technique for gridding ice thickness measurements. There were a few areas where I found the article less than clear, and improving these would make the paper much more readable and useful. The first main problem comes in the choice of the measure of skill for the techniques. As I read the description, the techniques

are tested by deleting each test point's nearest upstream and downstream neighbors, if they are closer than a specified distance from the point, then recalculating the ice thickness based on the deleted data set. If this is the case, increasing the deletion window should not cause rapidly increasing errors in the inverse-distance (ID) scheme, as the ID scheme uses points from all directions and deleting points in the flowline direction only remove data two of the eight octants. I'm sure this is a misapprehension on my part about how the evaluation scheme works, but if so, the authors should spend some time clarifying this section so that others do not make the same mistake. Second, I could not make out what the authors meant by robustness. In common statistical jargon, a robust technique is one that is insensitive to outliers from an assumed, usually Gaussian, error distribution. That doesn't seem to be what the authors mean here. A definition, or better yet a metric, of robustness would help. Third, I could not find a good explanation of where and why the flowline interpolation fails, and where and why the local-thickness-factor interpolation fails. This seems like an important part of the technique, and it deserves a paragraph or two. There are a few words in the appendix about how this works, but not nearly enough to let me recreate the technique. Fourth, I had no idea whether the recovered values of c_eff were physically reasonable. C_eff is a proxy for the depth-averaged flow parameter; Warner and Budd use a uniform value that is not unreasonable, but I couldn't tell here what values the interpolation scheme was giving, and whether they might correspond to actual ice flow. Fifth, in various parts of the paper, the authors discuss streamlines converging upstream of a data point. By definition, streamlines should not cross or converge. If the numerical schemes used in calculating the streamlines do allow this, then there should be some note of it in the text, and a description of how streamlines are merged. Last, I really liked seeing the discussion of the basal elevation structure that the technique actually recovered, but was sorry to see it as an afterthought within the conclusions secion. This deserves a bit more attention, as there are some interesting findings here. My other concerns were with the organization and presentation of the paper.

Organization: Conventionally, paper sections are, in order: Abstract, introduction, data,

methods, results, discussion, and conclusions. While this presentation is not necessary to a good paper, it can help the reader understand the purpose of each section of the text. It would be helpful to make a more straightforward distinction between the data and methods; The authors present important details of the method before the data are described (P4, Lines 17-end) , and present their scheme for evaluating the skill of the interpolation at more-or-less the same time that they present the results of this evaluation. Presentation. Hyphens should be used to build compound adjectives where needed to improve clarity, not, generally, otherwise. Thus "y-intercept" is incorrect, but "inverse-distance-cubed interpolation" is correct, as is "shallow-ice approximation." The name of the inverse-distance scheme should be "inverse-distance-cubed" not "inverse distance cube." I would suggest an abbreviation: call it ID interpolation for short, or IDW (inverse-distance-weighted). Specific points: I give page numbers and line numbers separated by commas, and I give my preferred wording in brackets. 3,14 [The ICECAP instrument suite is based...] 3, 21 [which we use] 4,16-5,5 Move to the techniques section; give a brief description of the schemes to be evaluated here, but no details. 6,3-12 It is premature to describe the relative skills of these methods; it's probably best to add a subsection after the methods have been evaluated that describes how they are combined to give the final ice-thickness map.

8,16 this is unclear- is the data point eliminated (i.e. not considered in the evaluation) or are the upstream and downstream points eliminated?

9, 15 This paragraph needs more development, or might be deleted as it doesn't clearly add much to the paper. It is not clear why the variogram shape described here implies an isotropic distribution, or how much scatter would be required to prove an anisotropic distribution. The authors state that the topography looks strange before filtering, and that they filter it to make it look better. Does the variogram prove that this was not the right thing do to? 9, 21 Is this filtering done prior to the evaluation of the interpolation methods, or only on the final data product? If the former, it should be mentioned earlier in the paper, if the latter, its effects on the interpolation evaluations should come up

here.

Figures 4 and 5- These are complicated figures with lots of information in them, but they aren't discussed much in the text. What's the significance of the different statistics described by the bars? What about these graphs shows the superiority of one technique over another?

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