Authors response to editors review of manuscript tc-2011-5, submitted on 27 Jan 2011, titled “Refined broad-scale sub-glacial morphology of Aurora Subglacial Basin, East Antarctica derived by an ice-dynamics-based interpolation scheme” by Roberts et al. Items for each review have been listed sequentially with text from the reviews shown in *italics*. Locations in the text are referenced by page and line, e.g. page 660 lines 19-20 is given as P660 L19-20.

1 Review tcd-5-C531-2011 (Prof Waddington)

Using ice physics is good, and the simplest historical way to introduce ice physics has been to assume that the ice is perfectly plastic with a yield stress τ_0*. In that case, by assuming that the yield stress is reached everywhere at the base, i.e.* $\tau_0 = \rho g D \nabla S$ *the depth D(x,y) can be estimated as* $D = \frac{\tau_0}{\rho g}[\nabla S]^{-1}$ *(1)*

Only the surface slope is needed, and τ_0 *(in the coefficient* $\frac{\tau_0}{\rho g}$*) is the adjustable parameter that plays the role of* c_{eff} *in the current manuscript.*

Has anyone attempted to infer bedrock using (1)? If so, does a simple plastic scheme perform better than an inverse-cubic law, i.e. are the major gains obtained by introducing at least some ice physics, or by then refining the physics? By assuming the SIA, this current manuscript (and Warner and Budd, 2000) offer a more-sophisticated physics-based interpolation scheme. I would also be curious to see how much improvement the SIA approach makes in comparison to a plastic scheme. I suspect that that this improvement in sophistication also makes a big difference.

The suggested thickness approximation based on the perfectly plastic approximation, i.e. $D(x,y) = \frac{\tau_0}{\rho g}[|\nabla s(x,y)|]^{-1}$ can also be treated in a similar manner to the TELVIS scheme. In particular a dimensionless, order unity, locally varying scaling factor can be introduced to assimilate observational data. However as shown in Fig. 1 the Warner and Budd (2000) formulation gives a ‘tighter’ correlation with observed ice thickness than the plastic flow formulation. Therefore, the current manuscript focuses on the Warner and Budd (2000) formulation, but the authors would be interested in a future collaboration to assess alternative interpolation schemes. Figure 1 also exemplifies the improvement in predicting ice thicknesses obtained using the shallow-ice approximation compared to the plastic flow assumption, as Prof Waddington expects. It is not unexpected that the shallow-ice approximation, with $n = 3$, yields a better approximation than the plastic flow model, as the latter is the asymptotic limit of the former for large n, and we know that $n = 3$ is a better approximation than 'large n' for much of the ice-sheet. Furthermore, although the influence of the ice fluxes in the Warner and Budd (2000) scheme is attenuated by the $1/5$ power, it still produces a factor comparable to κ (the non-dimensional replacement for c_{eff} introduced below). For example, the ratio of the 75^{th} to 25^{th} quartile of the ice fluxes is 4.9 and attenuating this by the $1/5$ power yields in a factor of 1.4 in the Warner and Budd (2000) ice thicknesses.

My only concern about the paper is the non-intuitive nature of the units for c_{eff} and $t(x,y)$. I have a bit of trouble gaining much insight from an "effective flow parameter" coefficient c_{eff} that has units of $\text{m}^{3/5}\text{yr}^{1/5}$, and a "thickness factor" that has units of $\text{m}^{2/5}/,\text{yr}^{-1/5}$.

I think the work could be made more accessible to readers by non-dimensionalizing the flux equation...

In terms of the non-intuitive nature of the units for "c_{eff}" and "t" the authors agree completely with Prof Waddington. Rather than the suggested non-dimensionalisation, we have opted to decompose c_{eff} into a dimensionless factor of order unity (κ) and the (Warner and Budd, 2000) factor (c_0) and to fold the latter factor into the definition of t. This results in t being the approximate ice thickness (in m) and κ being a simple (dimensionless, order unity) locally varying scaling factor. The authors believe these changes address the underlying issue and result in more intuitive parameters than both the original manuscript and the proposed non-dimensional parameters. Specific changes to the manuscript addressing this point are

- Replaced "c_{eff}" everywhere (including the axis title of Fig 3b) with "κ"
- P659 L9 Changed Equation 1 to include "c_0" thereby redefining "t" to have mean-

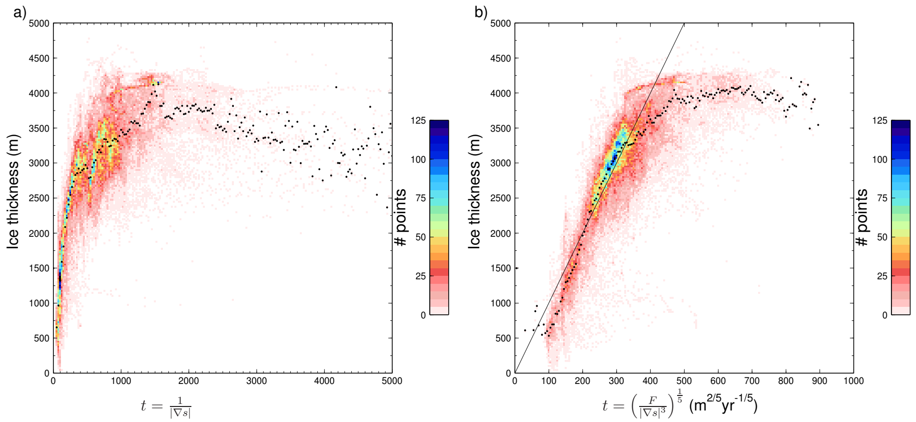

Fig. 1. Basin wide characteristics from the ICECAP data and the ice dynamics based interpolation. Binned distribution of measured ice thickness and pseudo thickness (t). Also shown are medians of ice thickness (D) of the binned distribution (dots). **(a)** plastic flow model and **(b)** Warner and Budd (2000), also shown is the line of slope 1 corresponding to $c_0 = 10^5$ m^{3}yr.

ingful magnitude and units.

– P659 L10-11 Changed "where c_{eff} can be regarded as an effective flow parameter, and $t(x,y)$ as a local thickness factor (in m$^{2/5}$yr$^{-1/5}$)." to "where Warner and Budd (2000) suggest a value of $c_0 = 10^5$ m^{3}yr, corresponding to an ice flow rate constant in the Glen cubic ($n = 3$) flow relation with the exponents 3 and $\frac{1}{5}$ in Eq. (1) resulting from n and $\frac{1}{n+2}$, respectively), yielding $t(x,y)$ as a pseudo ice thickness (in m), with magnitude approximately that of the local ice thickness. We have introduced the dimensionless factor κ, with magnitude of order unity as a locally tunable parameter to compensate for local deviations from the modelling of Warner and Budd (2000)."

- P659 L12 Changed "Warner and Budd (2000) suggested using a constant $c_{\mathit{eff}} = 10.0$ m$^{3/5}$yr$^{1/5}$." to "The interpolation scheme of Warner and Budd (2000) is obtained by setting $\kappa = 1$ everywhere."
- P659 L17 Changed "allow c_{eff} to vary" to "introduce κ and allow it to vary"
- P661 L10 Changed "local thickness factor" to "pseudo thickness"
- P661 L17 Changed "local thickness factor" to "pseudo thickness"
- P661 L24 Changed "local thickness factor" to "pseudo thickness"
- P662 L6 Changed "local thickness factor field" to "pseudo thickness field, putting an emphasis on similarity of that quantity, rather then on proximity"
- P662 L7-8 Changed "local thickness factor" to "pseudo thickness"
- P662 L10 Changed "local thickness factor" to "pseudo thickness"
- P662 L13 Changed "local thickness factor" to "pseudo thickness"
- P664 L19 Changed "local thickness factor" to "pseudo thickness"
- P664 L20 Changed "600 m$^{2/5}$yr$^{-1/5}$" to "6000 m"
- P671 Table 1 Changed "local thickness factor" to "pseudo thickness"
- P674 Caption Changed "local thickness factor" to "pseudo thickness"
- P674 Table 4 Changed "local thickness factor" to "pseudo thickness"
- P676 Caption Changed "Unconstrained inferred ice sheet basal elevation based on basin wide average c_{eff} of 10.0 m$^{2/5}$yr$^{-1/5}$" to "Unconstrained inferred ice sheet basal elevation based on a constant $\kappa = 1$"
- P677 Caption Changed "local thickness factor" to "pseudo thickness"

- P677 Caption Deleted "c_{eff} is the slope of the data."
- P677 Caption Changed "and the constant slope line $c_{\mathit{eff}} = 10.0$ m$^{3/5}$yr$^{1/5}$" to "and the line of slope 1 corresponding to $c_0 = 10^5$ m^{3}yr"
- P677 Caption Changed "high local thickness factor" to "high pseudo thickness"
- P678 Caption Changed "$c_{\mathit{eff}} = 10.0$ m$^{3/5}$yr$^{1/5}$" to "$\kappa = 1.0$"

Modifications to address the list specific editorial comments are detailed below

- P659 L1 *"...data...*were *first mapped..."* Changed "was" to "were"
- P659 L26 *The inverse cubic relation is introduced here without explanation or citation. Perhaps cite Lythe et al. (2001) here* Changed "inverse distance cube method" to "inverse-distance-cube (IDC) interpolation method (see for example Lythe et al., 2001)"
- P660 L1-9 *Both high values of t and low values of t that fail to follow the straight line occur near ice ridges and domes. The fact that both extremes occur in the same generic terrains may deserve some additional comment beyond just attributing the failure to low slope in the first case, and low flux in the second case. Flux and slope are both low near all ridges and domes. Perhaps, more fundamentally, as Equation (8) (above) suggests, the failure arises because the SIA does not apply there? Or is it simply because the method can become unstable in slow flow in the presence of data errors, in spite of the 1/5 power?* These two paragraphs have been replace by the new paragraph "Inspection of the relationship between measured ice thickness D and the pseudo thickness t (Fig. 3a) shows a clustering of values along a line of constant slope (corresponding to c_0) over much of the range of t. The distribution diverges strongly at both high and low t due to low slope and fluxes, respectively. This corresponds, in general, to regions near domes, ridges and saddles where the shallow-ice approximation is not appropriate, and a linear

flow relation ($n = 1$) in Eq. (1) (Pettit and Waddington, 2003) would be more appropriate. These regions are excluded from the dynamical interpolation by enforcing the limits $t(x,y) \leq 6000$ m and $F(x,y) > 600$ m^{2}yr^{-1}."

In addition the following text has been introduced earlier (P659 L16) "and the inapplicability of the simple shallow-ice approximation around summits, which would result in a transition from the cubic Glen flow relation ($n = 3$) in Eq. (1) to a linear relation ($n = 1$) (Pettit and Waddington, 2003) and a different c_0."

- P660 L10 *misplaced "only". Text should read "...these criteria* exclude only*...".* Changed "only exclude" to "exclude only"

- P662 L3-4 *What happens if $t_i = t_p$ in the denominator?* Modified eqn (2) to include a small constant 'ϵ' and changed "where the summations" to "where ϵ is a small constant to avoid division by zero. The summations". Also modified Eq. (3) (P662 L16) to include 'ϵ'

- P662 L4-9 *"..summations range over all streamlines involving p and using..." Presumably p is on only one flowline. How do other flowlines get involved? It would be helpful to explain if some lateral averaging scheme is being introduced here.* To clarify the relationship between Lagrangian streamlines and the Eulerian grid, the following text has been added to P660 L20 "The streamline integration is preformed in the Lagrangian frame with sub-metre precision and mapped back to a 1 km $\times 1$ km Eulerian grid, therefore each cell in the Eulerian grid can contain multiple, distinct streamlines."

  In addition, the following changes were introduced to Section 2.2.1;

  - P662 L2 Changed "position" to "cell"
  - P662 L5 Changed "observational points" to "observational cells for each streamline"
  - P662 L8 Changed "upstream points and one downstream point" to "upstream cells and one downstream cell"

  - P662 L9 Changed “resulting in multiple streamlines” to “resulting in multiple streamlines passing through a Eulerian grid cell”

  And the following changes to Section 2.2.2;

  - P662 L15 Changed “position” to “cell”
  - P662 L20-21 Changed “differ, with the former being over all streamlines passing thought the grid point, while the later is over 16 points” to “differ, in the former being over all streamlines passing through the grid cell, while in the latter involving 16 cells”
  - P663 L4 Changed “points” to “cells”
  - P663 L5 Changed “points” to “cells”

- P662 L16 *misplaced “only”. Text should read “...method* varies only*...”*. Changed “only varies” to “varies only”

- P663 L11 *“...data around the test point* are *excluded...”*. Changed “is” to “are”

- P663 L22 *“...data from the flight lines* were *mapped...”*. Changed “was” to “were”

- P664 L5 *Needs hyphen “...simple* inverse-distance *cube method...”*. This paragraph has been rewritten for clarity, and the abbreviation IDC introduced earlier. See first item under review tcd-5-C527-2011 for details.

- P664 L26 *Misplaced “only” and mis-spelled “increasing”. Text should read “...with the biases* increasing only *slowly”*. Changed “only increase” to “increasing only”

- P664 L27 *Should have hyphen in “...an* inverse-distance *method...”*. Changed “inverse distance” to “inverse-distance”

- Tables 2,3 and 4 *It would be helpful if the captions explained or defined* r *and* r^2 *respectively*. Added “r is the Pearson’s correlation coefficient” to Table 2 caption and “r^2 is the square of Pearson’s correlation coefficient” to the captions of Tables 3 and 4.

- Figure 3a *The figure seems to call for 3 straight lines, perhaps with nearly flat lines for small and large t?*. While it would be possible to fit linear least squares line segments to this data for small and large t, these are the very regions that we want to exclude from the flux based interpolation, due to the inapplicability of Warner and Budd (2000) for these regions (see item P660 L1-9 above). Fitting such line segments would, therefore, confuse the readers.

2 Review tcd-5-C274-2011 (Dr Ritz)

The method to calculate ice fluxes from the stream lines seems interesting but must be better explained. In particular;

- *How exactly is the flux computed, is it on the Eulerian cells or in the Lagrangian frame ?* This is calculated in the Eulerian frame. The text of P660 L19-20 changed from “Individual streamlines are mapped back to an Eulerian grid (for further processing) by tracking the streamline integration as grid coordinates in the Eulerian frame.” to “Individual streamlines are mapped back to an Eulerian grid where the flux is calculated taking into account the Eulerian cell width normal to the local flow direction.”

- *How convergence and divergence of the flow lines are taken into account ?* This has been addressed by adding text to Section 2.1 and rewording sections 2.2.1 and 2.2.2. See review tcd-5-C531-2011 item P662 L4-9 for details.

- *What is the difference with for instance Testut et al. (2003) method ?* The current method advects point masses, while Testut et al. (2003) considered the surface between two flow-lines. The following text has been added to P660 L17 to clarify this point. “The local accumulation surrounding the origin of each streamline is advected downstream as a point mass, in contrast to the flow line method of Testut et al. (2003) which considered the surface between two distinct flow lines."

- *How are subsampled streamlines in order to have 16 upstream seeds ?* Changed P660 L20-22 to clarify the oversampling, from "Additional accuracy is obtained by oversampling in the Lagrangian frame, with each Eulerian cell containing 16 upstream seeds for subsequent downstream streamline tracing." to "Additional accuracy is obtained by oversampling the Lagrangian streamlines, in particular each Eulerian cell is the origin for 16 streamlines, arranged in a regular 4×4 spatial grid and each advecting one-sixteenth of the accumulation from the Eulerian cell."

there should be deeper discussion about the selection of regions where the dynamical method can be applied, the role of basal sliding, and the difficulty to detect deep gradients, also in relation to the SIA hypothesis. The manuscript has been modified to include a discussion on the inapplicability of the Warner and Budd (2000) formulation in low devatoric stress regions. In particular see the discussion for review tcd-C531-2001 item L660 L1-9.

In figure 3a, the breakaway from constant slope at high local thickness factor appears around $t = 400 - 500\text{m}^{2/5}\text{yr}^{-1/5}$ *but to exclude regions from dynamical interpolation the authors use a limit at* $t = 600\text{m}^{2/5}\text{yr}^{-1/5}$*. Could this high threshold be an explanation of the relatively bad performance (see comment on figure 5) at high thickness.* The modification of t in Eq.(1) to contain c_0 (see review tcd-5-C531-2011) makes the origin of these limits clearer. In particular, Dr Ritz suggests limits of the modified $t = 4000 - 5000$m while the manuscript uses a limit of $t = 6000$m. We have deliberately selected this relatively high limit for two reasons. Firstly, to not artificially limit the upper bound of the ice thickness to the range $4000 - 5000$m. Second, the TELVIS scheme has the ability to compensate for local deviations from the underlying shallow-ice approximation, and therefore should cope quite well with moderately-too-large pseudo ice thicknesses. In actually, the "relatively bad performance (see comment on figure 5) at high thickness" is partly an artifact of the relatively tight bounds of TELVIS, and partly due to the ability of TELVIS to produce thicknesses outside the range of the local thicknesses, unlike inverse-distance cube type schemes which average local thicknesses. See review tcd-5-C527-2011 item P679-P680 for additions to Section 3 to better elucidate this.

Additionally, based on this figure, could it be possible to add a third case of exclusion by looking a posteriori at the calculated thickness (if the calculated thickness is higher than 4500 m, use inverse distance cube interpolation) ? It would be possible to recalculate the ice thickness (using an inverse-distance-cube type scheme) for very thick (say > 4500m) points, but in reality these only represent a very small number of points, and these points are subsequently removed by the Gaussian smoothing. In addition, as mentioned above, such a limit might artificially restrict the calculated thicknesses, with ice thicknesses in excess of 4500m being measured.

The authors obviously made an effort to quantify bias and robustness of the method and this is a point I appreciate in this paper. I still have a few questions: Is it possible to use the fact that SIA is not valid at horizontal small scale to quantify the limits of the methods in term of detection of deep gradients ? Is it possible to assess the relative role of data points sparsity and SIA hypothesis ? This would require an estimation of sparsity (consistent with equations 2-4). The TELVIS algorithm has been designed to assimilate observational data and to accommodate mild deviations from the shallow-ice approximation with a cubic Glen flow law relationship. In particular, TELVIS is designed to maximise adherence to the shallow ice approximation and it is therefore not the appropriate tool to detect deviations from the shallow-ice approximation. Furthermore, the deviations that do occur are due, in general, to the inapplicability of the cubic Glen flow relationship, and the underlying cause of that inapplicability, be it low devatoric stresses or large 'deep gradients' are not quantifiable with the current technique.

Detailed comments

– P659 L9 *please give the horizontal scale upon which is calculated the slope. Is it the same as the one used for Lagrangian flow lines?* The slope is calculated from central differencing of the smoothed 1 km $\times 1$ km surface elevations. Changed "and surface elevation, $s(x,y)$, at all spatial locations" to "and gradient of the surface elevation, $s(x,y)$, (with the gradient calculated via central differencing of the smoothed (see Section 2.1) 1 km $\times 1$ km surface elevations) at all spatial locations".

- P675 P676 *It would be better to join figure 1 and 2 to make the comparison easier.* While we agree with Dr Ritz, that this would ease comparison of the two figures, it would needlessly shrink the figures to a size where it would be difficult to make out any features, especially the location of historical data in Fig. 1.

- P662 L5 *(In streamline interpolation). As mentioned in general comments, a figure could help to understand how are obtained all the streamlines involving p.* This is a consistent item across all the reviews, and has been addressed in the response to review tcd-5-C531-2011 item P662 L4-9

- P666 L21-26 *in comparison of regions below sea level both in bedmap and TELVIS. Please give extent in surface rather than in %.* Changed P666 L24 from "more extensive submarine base (9.9% more area below sea-level)," to "more extensive submarine base (1.33×10^6 km^2 compared to 1.21×10^6 km^2 below sea-level)." Also changed P666 L24-L28 from "with the areas of the Aurora Subglacial Basin and deep Aurora Subglacial Basin being greater than shown by BEDMAP by factors of 3.97 and 431 respectively (alternatively 21% of the Aurora Subglacial Basin is below -1000 mbsl in the new reconstruction compared to a mere 0.2% in BEDMAP)." to "Additionally, the TELVIS reconstruction has a more extensive Aurora Subglacial Basin (3.56×10^5 km^2 compared to 8.97×10^4 km^2) and deep Aurora Subglacial Basin (7.53×10^4 km^2 compared to 1.75×10^2 km^2). Alternatively 21% of the Aurora Subglacial Basin is below -1000 mbsl in the new reconstruction compared to a mere 0.2% in BEDMAP."

- P672 Caption *Table 2, it would be easier to understand if the definition of "mega-scale ice thickness range" was also given in the caption.* Added "r is the Pearson's correlation coefficient and mega-scale ice thickness 'roughness' is the difference between the maximum and minimum smoothed ice thickness within a 50 km radius." to this caption.

- P679 *Figure 4. where are lines R19 and R21 ? Why were these flight lines selected ?* These are the two western most pairs of flight lines shown in Figure 8a. The following text has been added to the caption of Figure 4 to clarify this point "R19 and R21 are the second most westerly and most westerly pair, respectively, of flight lines shown on Fig. 8a." These flight lines were selected as they had the least nearby historical data.

- P680 Fig 5 *could the authors explain the important number of "outliers points" with calculated ice thickness substantially higher than observation (in the range 3500-4500 of observed thickness). It would be interesting to know where they are located for in-stance (I guess they are in red in figure 8a), and if the sparsity of data is higher for those points. Because it occurs in thick regions, could it be related to a different type of base (cold-basal melting) allowing or not sliding (for instance).* Firstly, it should be noted that there are relatively few outlier and extreme points (114 and 652, respectively, out of 38505 points or 0.3% and 1.7%), and that these outliers are eliminated by the Gaussian smoothing. Also over much of the range, the substantially reduced inter-quartile distances for TELVIS, results in a much more restrictive definition of an outlier than for the inverse-distance-cube scheme. The relative peak in outliers and extreme points between a measured depth of $3500 - 4500$m is due to two factors. Firstly, the inverse-distance-cube scheme tends to cluster points near the average depth (as shown by the large data spread over $1500 - 3500$m range and therefore be deficient in large calculated thickness outliers. Second, TELVIS has the ability to produce depths in excess of the nearby measured depths (with suitable flux and surface gradient conditions). This ability not to be bound by nearby measurements is a strength of TELVIS, although it obviously comes with some cost in terms of producing outliers. It would be possible to posteriori replace flux based thickness estimates in excess of some threshold (would suggest around $5000\mathrm{m}$ to not artificially restrict the data range) with an inverse distance based estimate. However, since these outliers and extreme points are sufficiently isolated that the Gaussian smoothing eliminates them, this step was not deemed necessary. Additional text has been added to clarify this (see review tcd-5-C527-2011 item P679-P680).

3 Review tcd-5-C527-2011 (Anonymous)

Note that this review appears to be of the original submitted manuscript, which differs in both typesetting (I have modified the page and line references to correspond to the published discussion manuscript) and slightly on content due to suggestions from the editor. Despite this, all comments have been addressed as detailed below.

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. In fact, for both methods, all points within a specified radius are deleted. To clarify this we have changed P663 L23 - P664 L7 from "For the streamline interpolation method, each gridded data point was considered in turn, and if that data point had either the nearest upstream or downstream data points within a variable cut-off distance they were excluded from the data-set used to evaluate the skill. The ice thickness at the test point was then evaluated using only the remaining upstream and downstream data points and compared to the observed ice thickness. The same data points were used for the simple inverse distance cube method, with all data-points within the cut-off distance excluded from the calculation." to "For both the streamline and IDC methods, each gridded data point (test point) was considered in turn. All data points within a particular (variable) cut-off distance from the test point where temporarily discarded (reinstated for the next test point) from the data-set. The ice thickness at the test point was then estimated from this sub-sampled data-set using each method and the resulting thicknesses compared to the observed ice thickness."

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. We did not mean to imply a statistical usage to robust, merely to convey a lack of sensitivity. We have changed P658 L10-11 from " which is more robust to the absence or exclusion of data than traditional inverse distance type schemes." to "which is significantly less sensitive to the absence or exclusion of data than traditional inverse distance type schemes (i.e 'robust' to data sparsity)" to reflect this usage.

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. This failure is due to the inapplicability of the Warner and Budd (2000) formulation at both high and low t. In particular, the cubic power in the denominator of Eq. (1) in inappropriate in these circumstances. This has been fully explained in the revised manuscript and is detailed in the response to review tcd-5-C531-2011 item P660 L1-9

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. The definition for c_{eff} was very obtuse and made it difficult to gain any insight from the magnitude or variation in this parameter. This is the same issue as identified in the tcd-5-C531-2011 review item on the "non-intuitive nature of the units for c_{eff} and t", and this has been resolved by replacing c_{eff} with the Warner and Budd (2000) c_0 and a dimensionless, order unity, spatially varying scaling factor κ. Folding c_0 into the definition of t changes t into a "pseudo ice thickness" with units of thickness (m) and magnitude of approximately the ice thickness. See the response to the relevant item in the tcd-5-C531-2011 for specific modifications to the manuscript.

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. Inexorably, the reviewer is correct and streamlines do not coalesce or cross (although they can converge, ie vary their distance between each other). The streamlines are integrated in a Lagrangian frame with sub-metre precision and then these positions mapped into a 1 km $\times 1$ km Eulerian grid. Therefore each Eulerian cell can have multiple, distinct Lagrangian streamlines passing through it. Modifications to the manuscript to reflect this are detailed in review tcd-5-C531-2011 item P662 L4-9.

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 section. This deserves a bit more attention, as there are some interesting findings here. The purpose of this manuscript was not delve too deeply into the interesting science, but to set the framework for the TELVIS interpolation technique. The discussion of the basal elevation structure is deliberately broad scale and fairly generic in nature. Other manuscripts recently published, under review and in planning address specific aspects of the topography in detail, and we do not want to cut across this effort. We have replaced the following text on P667 L12-13 "Finer details of the Aurora Subglacial Basin will be revealed when the ICECAP project completes field operations and subsequent post-processing." with "Specific aspects of the under ice topography are addressed in separate publications, for example the glacial evolution is reported in Young et al. (2011), while the basal hydrology of the region is discussed in Wright et al. (submitted)." to reflect this focus.

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). The following changes have been made to address this issue

- P659 L4 Changed "shallow ice approximation" to "shallow-ice approximation"
- P659 L20 Changed "shallow ice approximation" to "shallow-ice approximation"
- P659 L26 Changed "inverse distance cube method" to "inverse-distance-cube (IDC) interpolation method (see for example Lythe et al., 2001)"
- P660 L11-12 Changed "inverse distance cube" to "IDC"
- P659 L24 Changed "shallow ice approximation" to "shallow-ice approximation"
- P661 L8-9 Changed "elevation, with a third inverse distance cube method employed" to "elevation, while a third (IDC) method is employed"
- P661 L18 Changed "inverse distance cube" to "IDC"
- P663 L2 Changed "inverse distance cube" to "IDC"
- P663 L20 Changed "inverse distance cube" to "IDC"
- P664 L2-3 Changed "inverse distance cube" to "IDC"
- P664 L9 Changed "a y-intercept" to "an intercept"
- P664 L10 Changed "inverse distance cube" to "IDC"
- P664 L13 Changed "inverse distance cube" to "IDC"
- P664 L19 Changed "y-intercept" to "intercept"

- P664 L23 Changed “inverse distance cube” to “IDC”
- P664 L25 Changed “inverse distance cube” to “IDC”
- P671 Caption Changed “y-intercept” to “intercept”
- P673 Caption Changed “y-intercept” to “intercept”
- P674 Caption Changed “y-intercept” to “intercept”
- P679 Caption Changed “inverse distance cubed scheme” to “inverse-distance-cubed scheme”
- P681 Caption Changed “inverse distance cubed method” to “inverse-distance-cubed method”

Specific points:

- P658 L18 *Would prefer “The ICECAP instrument suite is based...”* Changed “The ICECAP long range aircraft is based on the SOAR instrument suite” to “The ICECAP instrument suite is based on SOAR equipment”
- P658 L25 *Would prefer “which we use”* Changed “which can be used” to “which we use”
- P659 L23-P660 L13 *Move to the techniques section; give a brief description of the schemes to be evaluated here, but no details.* We see no reason to leave even a brief description at this point in the manuscript. Therefore we have moved this text to the start of Section 2.2 (Interpolation methods).
- P661 L11-22 *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.* We had tried such a structure in a earlier version on the manuscript, and unfortunately it

did not aid in helping the reader to understand the TELVIS algorithm. It was found to be easier to understand if the reader had the motivation as to why various schemes were being introduced and where they might be applicable, rather than having the reader try to memorise the various schemes, then merge them into a single scheme, and finally find out why it has been done this way. Therefore, in order to aid clarity in understanding the entire TELVIS scheme, we have opted to leave the this section of the manuscript structured as is.

- P663 L24-P664 L7 *this is unclear- is the data point eliminated (i.e. not considered in the evaluation) or are the upstream and downstream points eliminated?* It is upstream and downstream points that are removed. The text has been rewritten to clarify this and this rewrite is detailed in the first item for this (tcd-5-C527-2011) review.

- P665 L4-10 *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?* While we believe that the very high correlation (Persons' correlation coefficient $r^2 = 0.97$) with the reference isotropic distribution, is strongly suggestive of a isotropic distribution, we have been unable to find any published criteria. This is probably no surprising as there will be a spectrum of correlation coefficients from anisotropic to isotropic with any boundaries being somewhat artificial. In light of this, the text "The latter was assessed from auto-correlations (AC) of ice thickness as a function of displacement (see Fig. 7) which show little scatter ($r^2 = 0.97$) from a least squares linear exponential fit ($AC = a_0 + a_1 exp(-D/a_2)$) with displacement (D). Such a fit characterises an isotropic distribution (Banerjee et al., 2004)." has been deleted. Further, the next paragraph has been concatenated into this paragraph, and the text “The high frequency” replaced with “This high frequency” Figure 7 has also been deleted.

- P679 - P680 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?* The following text has been added at the end of P665 L3 to clarify this point. “This is demonstrated by the greatly reduced inter-quartile distances (IQD) for TELVIS in Fig. 5) (especially in the mid range thicknesses where IDC methods cluster the calculated ice thicknesses). The increased number of outliers (1.5–3.0 IQD from the median) and extreme values (> 3.0 IQD from the median) is still small for TELVIS (0.3% and 1.7%, respectively) and is a function of the TELVIS algorithms ability to deepen thicknesses beyond the local measurements (if the flux and slope indicate that this is appropriate) rather then revert towards a mean value. These outlier and extreme values are eliminated by the Gaussian smoothing discussed below.”

4 Additional modifications

In addition to modifications to the manuscript to address the reviewers comments and concerns, the following modifications were made to improve clarity.

- P659 L1 Changed “The available ice thickness data (see Fig. 1)” to “The historical ice thickness data (Fig.1 displays the location of the data superimposed on a schematic of the BEDMAP dataset, Lythe et al., 2001)”
- P659 L6 Changed “meter” to “metre”
- P659 L8 Changed “yielding” to “namely”
- P660 L25-26 Changed “from numerical diffusion” to “associated with numerical diffusion”

- P661 L7 Changed “Two” to “In the dynamically motivated approach two”
- P662 L5 Changed “involves” to “using”
- P662 L6-7 Changed “as at the observational points” to “since at the observational cells”
- P662 L12 Changed “weighted to favour” to “once again weighted to favour”
- P662 L14 Changed “caveat that points” to “caveat that the selected points”
- P663 L12 Changed “radii simulating” to “radii to simulate”
- P663 L14-15 Changed “with large ‘mega-scale ice thickness ranges’ (see Table 2). Mega-scale ice thickness ranges is herein” to “within large mega-scale ice thickness ‘roughness’ regions (see Table 2). Mega-scale ice thickness ‘roughness’ is herein”
- P663 L20-21 Changed “where to remove directional biases, the two nearest neighbors in each octant are used” to “where in the latter method the two nearest neighbors in each octant are used to remove directional biases”
- P664 L29 Changed “ranges” to “roughness”
- P665 L9 Changed “represents” to “characterises”
- P667 L1-2 Changed “most sparse” to “sparsest”
- P668 L10-11 Changed “Several anonymous reviewers and Prof. J. Bamber provided helpful reviews of the manuscript.” to “Prof. J. Bamber, Prof. E. Waddington, Dr. C. Ritz and several anonymous reviewers provided very constructive reviews of the manuscript.”
- P668 L18-20 Deleted the Banerjee et al. (2004) reference

– P669 L12 Inserted Pettit and Waddington (2003) reference

– P669 L20 Inserted Testut et al. (2003) reference

– P670 L1-4 Updated Young et al. (2011) reference from submitted to published details

– P672 Caption Changed "range" to "'roughness'"

– P672 Table Changed "range" to "'roughness'"

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