We have appreciated all the reviewers' constructive comments on our manuscript. In the following we reply within the open discussion to the main points that all or two of the reviewers have raised. Then, a point-to-point response of the major problems highlighted by each reviewer is given. On the other hand, a full response, with the address of the minor problems (such as editing tips) will come with the edited version of the manuscript.

Best regards,

Shuji Fujita on behalf of co-authors

For the paper, three are three reviewers, Dr. A. Wright, Dr. O. Eisen and Dr. R. Bingham. In this document for the interactive discussions, the three reviewers are is referred to as the Reviewers #1, #2 and #3, respectively, using the timing order of the review comments that they gave. We write reviewers' comments and the authors' comments using bold letters and italic letters, respectively.

**1. The general need for improvement for the structure of the paper and conciseness (All of the reviewers)**

*We first make a list of relevant comments from the reviewers by copying.*

[Reviewer #1]
**The structure of the paper does not help the reader. The long sections 3 and 4.2, which discuss each of the survey tracks in turn, contain significant repetition and a level of detail that is unlikely to be of interest to anyone not directly concerned with work on this particular survey. The results should be described and discussed in a more concise way, probably through more consideration of the dataset as a whole rather that the dissection of each individual element.**

[Reviewer #2]
**A major problem for the manuscript in its present state is the lack of conciseness. Especially section 3 and 4 are somewhat lengthy to read, partly repeat information and do not fully separate what the section headings promise: a clear separation of results and discussion. Some examples (though not all) are p1794 L1 and L14ff, p1795 first paragraph and L19ff. The first paragraph in p1800 is rather a description of the analysis than a discussion of the results. Currently, there are three different places with info for each study area: sections 2.4, 3 and 4. To streamline the manuscript in this respect the authors might consider changing the structure such that the properties of the major leg classes are pooled and described at once, including information from the current section "Study area", followed by the appropriate discussion of the results and then directly a discussion afterwards. Doing so it could be easier for the reader to focus. This would, however, require that the methodology is exemplarily described for one leg class right at the beginning.**

[Reviewer #3]
**Structure: The paper is certainly over-long. Firstly, the first 3 paragraphs of the introduction can certainly be trimmed (for example, the fact that 90% of the ice sheet is drained by ice streams appears again in the Discussion section), while the final paragraph, extensively summarising the results/conclusions of the paper, is both out of place and unnecessary. Secondly, the splitting of sections 2, 3 and 4 into detailed introductions/discussions of 6 different “sites” is something that leaves room for trimming: in section 2, for example, much of this info could be presented more succinctly in a Table. In the results, it’s probably fine. But in the discussion, Section 4, I think it would be more valuable to write this section in terms of general patterns found in the entire dataset, rather than the site-by-site discussion that is presented. In Section 2, The justification for presenting of the 73.3 kya internal layer (P 1792 19) is unclear until much later (i.e. Section 4.4; also see my comment on this below); some reorganisation between methods and discussion sections on this topic could provide a more concise manuscript. Fourthly, I believe the conclusions section would have more impact if it were reduced to a single paragraph stating the main outcomes of the paper, rather than being the more expansive point-by-point summary of the paper that is presented.**

*We fully agree that one of main problems of the manuscript is lengthy conditions as all the reviewers and the editor pointed out. In the revised version, we will attempt to make concise paper by applying the suggested modifications. Major revisions will include (i) removal of repetition of explanations, (ii) restructure based on suggestions by reviewers and (iii) moving some portions to supplementary materials. The reviewer #3 advised us concrete ideas for trimming each section. We will attempt to do all suggestions to make the paper concise and well focused.*

**2. The reviewers commented that the authors should explain concrete criteria for the radar diagnosis, definition of errors and definition of echo strength**
**(All of the reviewers)**

[Reviewer #1]

**2. Diagnosis of wet or dry bed – I found it very difficult from reading the manuscript to identify what the exact criteria were that were used to classify the bed as wet or dry. Presumably a reflection of some value of dB greater than would be predicted by using the linear trend from the upper part of the ice column to account for englacial absorption was used? I could not find this stated clearly anywhere in the manuscript. The method of classification needs to be described much more clearly.**

[Reviewer #1]

**(1785,17-19) - "For the majority of the investigated locations, we were able to infer bed conditions. The possible error was estimated to be within several percent" – several percent of what? Basal conditions are not estimated quantitatively (e.g. basal temperature) so how can a percentage error be given? In any case a summary of the results is probably out of place in the introduction.**

[Reviewer #2]
**Error analysis: Error estimates are stated to be a few percent throughout the manuscript, but clear derivation and discussion of the uncertainties is missing. Finally saying that "several percent are quite acceptable" emphasizes the need for a more thorough error analysis to show that these are indeed acceptable.**

[Reviewer #3]
**Derivation of bed reflection strength:**
**While the wider principles of the bed-reflection derivation are well conveyed, the authors make no comment on how or why they choose to use peak amplitudes from the bed. How are these extracted – manually/automatically? Do they simply use peak amplitudes – this is implied in the text**
**– or define a time window around them, e.g. Gades et al., 2000)? The latter would be a better way of reducing signal to noise ambiguities. At least a comment or two to clarify this issue would be beneficial.**

***Definition of the peak strength***
*As $[P_{bed}]_{dB}$, we simply extracted peak power of the time-series of echoes from the bed. From the original raw data, it was extracted semi-automatically. Semi-automatically means that basic processing is based on computer programming. Data was always checked by an operator's eye using graphics of the A-scope and Z-scope images on the computer, to verify continuity of the bed signal in lateral direction. This point will be described in the revised paper or the supplementary information for it.*

***Criteria for the radar diagnosis and errors of diagnosis***
*We did not use particular numerical criteria. Practically, our method of diagnosis is to find locations of temperate/frozen boundaries. Actual "temperate/frozen" diagnosis was done if the $[P^{c}_{bed}]_{dB}$ was larger/smaller than H in the X-PH plots. Basically, in H-P plots, distribution of the data points has a shape like "hockey*

*stick". In the method of diagnosis, the analysis of the regression line is to find the "inflection points" of the "hockey stick" shape easily in the X-PH plots. Such inflections near the critical depth $H_0$ are suggested features of water. In H-P plots, the exact point of the inflection cannot be determined because the data covers some area of the leg of a few hundred kilometres. However, by making X-PH plots, the locations of the inflections (in other words, jumps or steps of $[P_{bed}]_{dB}$) are easily visible.*

*Our estimation for errors of diagnosis is highly associated with this method. In Fig. R1, we show two examples for the bed diagnosis for the coastal area and for the inland plateau. They are modification of the Fig. 3b and Fig. 6d of the TCD paper. These examples show that profiles of $[P^c_{bed}]_{dB}$ are deviated from the profiles of H suddenly by several dB at some locations. We judged that such locations of jumps or steps as the locations of the inflections, that is, temperate/frozen boundaries. Such locations can be identified because sudden increase of $[P^c_{bed}]_{dB}$ often happens at H near the estimated critical $H_0$. It is natural to question that such steps or jumps of $[P^c_{bed}]_{dB}$ compared to variations of H have some ambiguity. However, in most cases, like we can see in examples of the two areas, we can identify such boundaries, both in inland plateau and coastal area.*
*Our estimation of error is practically for locating such boundaries. Even if we use slightly different gradient of regression lines from H-P plot, the locations of the diagnosed boundaries are deviated at most one or two kilometres. Within a leg (a distance of several hundred kilometres), we often find just a few or several major boundaries. By summing up such possible errors and by calculating the ratio of such possible cumulative error versus total distance each leg, we derive that the error ratio is well below a few %. To make this error estimation much more conservative, we expressed that the maximum estimation of the possible misjudgement for each leg.*

*We plan to describe this concept of errors in the main text and in supplementary information of the revised manuscript.*

**3. The critical thickness $H_0$ for the regression varies along all legs**

**(Two of the reviewers)**

*Two of the reviewers (#2 and #3) gave comments about the partitioning of the data analysis. We understood that we need to explain background of the partitioning more carefully. First, we write the reviewers' comments.*

[Reviewer #2]

**· It took me a while to figure out how much $H_0$ varies among the different legs. I think a clear statement in the conceptual overview in the introduction on the methodology, that the critical thickness for the regression varies along all legs, could help to avoid such.**

[Reviewer #3]

**Partitioning of data analysis:**

**I can appreciate why, in the early stages of the data analysis, the authors have broken down their data analysis into different sections of the radar tracks, with different geographical characteristics. However, I don't understand why some of these data-sections, and the analyses of them, are not combined at any stage in the paper. I am particularly perplexed as to why sections F1 and F2 are even analysed separately, and what it is that makes them separate sections anyway. If an H-P plot were done for both F1 and F2, surely the difficulty encountered with creating a regression line for F2 would be resolved (in effect the authors do this anyway in their Step 4, consideration of neighbouring data – but why even do this, rather than pooling the original data?) What would an H-P plot for all the**

**data presented in the paper look like? At least I think we need an explanation for why this is not presented. Even if it is considered that pooling all data is not appropriate, I cannot see an argument against pooling data for tracks A, F and C.**

*We understood that our explanation on this point was insufficient. Indeed, a reason for partitioning the data analysis is that temperature field and $H_0$ varies from one location to another. There are many factors that control the temperature field, such as surface temperature, accumulation rate, advection of ice mass by ice flow and so on. As we try to cover wider area for single analysis, larger variations of temperature field and $H_0$ are included in the data. Then we are not able to determine $H_0$ by a regression analysis. The partitioning is a practical procedure assuming that both temperature field and $H_0$ do not vary much within it. In the present stage of the analytical method, we need to keep the partitioning. As the Reviewer #2 suggested us above, the authors need to provide a clear statement in the conceptual overview in the introduction on the methodology, that the critical thickness for the regression varies along all legs.*

**4. Roughness as one of possible causes of the variation of bed echoes**

**(Two of the reviewers)**

[Reviewer #1]

**1. Roughness – The authors assert that, while the basal reflectivity is sensitive to basal roughness at the scale of the radar wavelengths, this can be rejected as an explanation for their results because the same pattern is found with both of the radar frequencies. The wavelengths of the two radars compared in this study, however, (0.94 m and 2.40 m) are sufficiently similar that a transition from sediment to bedrock might easily affect roughness at both these length scales. The presence of basal water may be the correct explanation for their observations, but I am not sure that roughness can be rejected so easily. Some analysis of the basal roughness should be included to demonstrate that both radars would not be affected equally by the magnitude of the roughness changes observed.**

[The reviewer #2 also pointed out that the roughness were not considered]

*Our observations are for two wavelengths, in ice, 0.94 m and 2.8 m being different by a factor of 3. At least, between these two wavelengths, we found no indications that one of these wavelengths gave particularly weak or strong reflections depending on wavelength. The reviewer #1 asked us a possibility that a transition from bedrock (rough) to sediment (smooth) might easily affect roughness at both these length scales.*

*Interaction between electromagnetic waves and the scattering objects is found in literature. We find examples of analysis in textbooks of microwave remote sensing (e.g., Fung, 1994; Ulaby, 1986). It is clear when roughness scales are close to wavelengths, some interactions occur, and that amount of interactions is dependent on wavelength (and frequency). In the wavelength of light, Mie Scattering or Rayleigh Scattering are well-known examples of such interactions.*

*We agree that we cannot completely exclude potential effect of the roughness from possible causes of the "hockey-stick"-like distribution and the "inflection points". It is still possible that the roughness may also change at ice thickness near $H_0$.*

*We will describe these ideas and conditions in the revised manuscript (or in the supplementary information of it).*

**5. Comparison of the diagnosed bed conditions with InSAR-based ice flow velocities**

**(A comment from the editor, Prof. J. Bamber)**

*The editor, Prof. Jon Bamber, provided us a comment that it would be useful to compare between the diagnosed bed conditions with InSAR-based ice flow velocities published by Rigout et al. (2011a, 2011b). We agree that the comparison is very useful. We provide a few figures R2 and R3 for this attempt. We hope to include these figures either in the main text or in the supplementary information of the revised paper.*

*Near the coast, our temperate/frozen diagnosis generally agrees with contrasts of faster/stagnant ice flow speed. In the inland plateau near Dome Fuji, distribution of the stagnant ice (ice flow velocity is ~1 m/year) agrees with area where we find distribution of the frozen bed. Enlarged maps are given for areas near the Shirase Glacier (Fig. R3(a)) and in the Western Dronning Maud Land (Fig. R3(b)). We find that there are correlations between diagnosed bed conditions and distribution of ice flow velocities.*

**RESPONSE TO MAJOR COMMENTS OF THE REVIEWER #1 (C910–C914, 2012)**

[Reviewer #1]

**Equation 1 – Since all the quantities are expressed in units of dB are the cumbersome square brackets and subscripts necessary? A line stating that all quantities are in logarithmic units might be more elegant.**

*We hope to keep present way of square brackets and subscripts, because removal of them may cause misunderstanding of readers that H, $\alpha$, T, x, and z are also in logarithmic scale. We hope to avoid such possibility of misunderstanding.*

[Reviewer #1]

**(1793,12) - An important feature of Fig. 3b, d is that we adjusted the scales of the left-hand and right-hand axes using the gradient of a regression line found in the region of thinner ice (< 2800m) of the H-P plot, as indicated by the red lines in Fig. 3a, c.**

**I didn't really find this description quite sufficient. It sounds like you have corrected the reflection strengths in figs 3 b and d for englacial absorption using a linear fit to the part of the H-P plot between H=2200m and H=2600m. If so this sentence, as well as the figure caption, should be re-written to make this clear.**

*We plan to correct this portion by adding explanations as follows. Figure caption will be re-written as well.*

*"An important feature of Fig. 3b, d is that we adjusted the scales of the left- hand and right-hand axes using the gradient of a regression line found in the region of thinner ice (< 2800m) of the H-P plot, as indicated by the red lines in Fig. 3a, c. This way of data plot is equivalent to an action that we have corrected the reflection strengths in figs 3 b and d for englacial absorption using a linear fit to the part of the H-P plot."*

[Reviewer #1]

**(1796,20-24) – Figure 6d, f show the X-PH plots for these regression lines, which clearly indicate the x locations at which the profiles of $[P^{c}_{bed}]_{dB}$ and H agree or disagree. In leg E1, disagreement occurred at locations at which the traverse route crossed the**

**Veststraumen ice stream (Näslund et al., 2000) (2625km < x < 2725km) and another ice stream at x = 2560km.**

**It may not be immediately clear what is meant by 'agree' and disagree' here, in fact, if this is after a correction has been applied for englacial attenuation, I'm not sure I understand at all?**

*We note that the expression here is highly related to the structure of the present paper. The section 3 is a result section where we did not develop discussions. At this stage, we did not discuss yet that our procedure was equivalent to corrections for englacial attenuation because there is not such a consensus in our community. We simply described features of the graphs before developing discussions in the section 4. We will attempt to solve this condition when we restructure the paper.*

[Reviewer #1]

**(1798,12-14) – Point (ii) does not make sense as it is, I think I can gather what you are trying to say but it really needs re-writing.**

*The reviewer is right and the sentence needs rewriting.*

*Suggestion of rewriting*

*"The H-P plots show that an anomalous increase in $[P^{c}_{bed}]_{dB}$ at larger H occurred, which was independent of the choice of radar frequencies or radar-pulse widths. If the anomalous increase in $[P^{c}_{bed}]_{dB}$ at larger H did not occur, it is very likely that the bed will not be detected by the radars used in the present study."*

[Reviewer #1]

**(1799,1-3) – Similarly in point (vii) it is unclear which gradient is being referred to and how it would be different if a different depth range were taken. These bullet points are a good idea to sum-up the results but they should stand independently as complete sentences.**

*In the revised version, we will specify that the gradients of the regression lines in many legs are referred to. As we described in the major point 2, our purpose to use the regression line is to identify the inflection points of the "hockey-stick"-like distributions of the data. With our procedure of using X-PH plots, we can identify locations of the temperate/frozen boundaries relatively sharply. Thus, small changes of the gradient will not change the results of the analysis, as far as the regression lines go through central part of the inflection points.*

[Reviewer #1]

**(1808,9-19) – Not all ice-stream/tributary locations are controlled entirely by the substrate, topography can have a very great influence. This work shows warm conditions beneath some ice streams, but that could be the result of increased frictional heat generation due to fast flow caused by high driving stresses. It is quite a jump between wet basal conditions and an ice stream location controlled by the condition of the substrate. This point probably needs to be backed up with further evidence or the reasoning of the authors needs to be stated more clearly.**

*The reviewer is right. Present work showed the plausible distribution of the temperate bed and frozen bed. But we did not carry out any systematic analysis of 3D distribution of the bed topography or generation of frictional heat within ice. Thus, what we should do here is to cite relevant earlier papers and to discuss possible links. We will introduce earlier works but we withdraw arguments that seems beyond our results.*

## RESPONSE TO MAJOR COMMENTS OF THE REVIEWER #2 (C972–C977, 2012)

### Examination of the Raymond Effect

[Reviewer #2]

**The authors use the Raymond effect underneath transient (with v>0) divides to explain why temperate ice could appear at the bed where H is considerably smaller than $H_0$ =2800 m. This seems somewhat contradictory, as the Raymond effect to have a considerable effect on the thermal regime operates best with a frozen bed. Martin et al (doi:10.1029/2008JF001025) in fact show that sliding "can damp or eliminate the operation of the Raymond effect" under certain conditions. If the Raymond effect is strong enough to change the temperature field at the bed then it should also find an expression in isochrone arches (Raymond bumps), which are best seen in radar data perpendicular to the ice divide. I think that clarification of this issue requires further data analysis, both from this radar data set but maybe also drawing on profiles available from other data sets in internal layer stratigraphy not discussed here. For example, the section B3 between NCR62 and MP runs approximately perpendicular to the topographic divide. So if the Raymond effect is large enough to have the consequences suggested by the authors, then there should also appear an isochrone arch in the B3 section. In addition, I would not necessarily expect a full numerical model run to proof the author's statement, but at least some numbers to estimate whether the suggested effect is large enough to cause profound changes at the bed.**

*Both observations of the isochrones and a three-dimensional, thermo-mechanically coupled ice flow model indicate that the bed near the exact ridge is under conditions that can be called as Raymond effect, as we discuss below.*

*(i) Appearance of the Raymond effect in the morphology of the isochrones*

*We demonstrate shape of isochrones on the legs A1-A2 and B3 in Fig R4. They are across the ridge at Dome F and MP. Data at the legs A1-A2 are partly shown in Fig. 13 in the TCD paper. Along these cross-ridge legs, isochrones are highly fluctuated with amplitude of several hundred metres, due to fluctuation of the bedrock elevations. However, we notice that near Dome Fuji, the deep isochrone L2 shows several ten kilometres wide bump in spite of the trough shape of the bedrock. We suggest that this is a feature of local effect at the ice divide known as the Raymond effect. As for the 40-km long leg at MP, we cannot specify clear features of some bump. However, the data of the isochrone in (b) covers a distance shorter than the visible width of bump in (a).*

*(ii) Assessment of dynamical conditions along the ridge with numerical models*

*Dynamical conditions at the ridge near Dome F were studied by Seddik (2008) and Seddik et al. (2011). A three-dimensional, thermo-mechanically coupled ice flow model with induced anisotropy has been applied to a 200×200 km domain around the Dome Fuji drill site. Steady-state simulations for present-day climate conditions are conducted. Distribution of the basal temperature is given in Fig. 10 of Seddik et al. (2011). This figure shows that basal temperature is at the pressure melting point below the ridge between DF and MP. At locations away from the exact ridge by approximately more than 20 km, the basal temperature is colder than the pressure melting point by a few degrees.*

*There are factors that may obscure feature of the Raymond effect. They include (i) complex bedrock topography with heterogeneous distribution of the temperate/frozen conditions and (ii) migration of the ridge locations in glacial/interglacial periods. Discussions for these factors will be complex, which seems beyond the scope of the present manuscript.*

*We will comment on these in the main text of the revised manuscript or its supplementary information.*

**Internal isochrone**

[Reviewer #2]

**p1792L19-28: The authors identify a continuous internal isochrone, date it at Dome F and EDML, and attribute that to the Toba eruption. A thing that puzzles me is the stated depth uncertainty of +/- 10 m for a pulse > 30 m. Moreover, the internal layer is interrupted along C2, so how can one be sure it is the same on either side of the missing section? Fair enough to have to independent estimates on either side, but this has to be stated and briefly discussed.**

*For detection of internal layers, near Dome Fuji, we used the 179-1 radar using a pulse of 60 ns. In ice, the resolution for the 60 ns pulse is ~5 m (page 1786, line 10). This pulse width was applied to the legs A1, B1, B2 and C1. The radar specification is similar to the 150 MHz radar used by Alfred Wegener Institute (Nixdorf et al., 1999). That is, longer pulse (500 ns) and short pulse (60 ns) were transmitted in turn, repetetively. As we stated in the TCD paper (page 1789, lines 21-22), relation between depth and echo timings were determined based on a down-hole radar target experiment (top of ice coring drill with known depths were detected by radar to depths of ~2000 m). The uncertainty of +/- 10 m is based on this processing.*

*For the C3 leg at EPICA DML site, we used the 179-2 radar using a pulse of 500 ns. In ice, resolution is ~42 m. Thus the reviewer is right that we should correct the uncertainty of the depth at EPICA DML.*

*As for the interruption of the internal layers at C2, we were able to connect them with confidence. Huybrechts et al. (2009) gave 10 internal layers along the ridge between DF and EPICA DML. Their layer number 10 (see their Fig. 3 and Table 1) agreed well with our isochrone. Our gap of the internal layers were well connected with their No. 10 layer. In addition, we also verified that many other isochrones in the gap zone were well filled with the data of Huybrechts et al. (2009).*

*We will comment on these in the main text of the revised manuscript or its supplementary information.*

[Reviewer #2]

**Regarding the origin of this reflection, I recommend to verify the two-way traveltime of this reflector in the JASE data with the results published by Eisen and others (J.Glac., 2006), which provide a detailed analysis of a reflector origin in airborne RES data at**

**22128 ns TWT (1866-1869 m, Table 2 in their study), which corresponds to more than one conductivity signal. By a brief intercomparison, Fujita and others can confirm that their conversion of traveltime to depth is correct and provide a much more accurate uncertainty estimate for the internal layer, as Eisen et al.'s results are accurate in depth to less than +/- 1 m.**

*It seems to us that comparison of the very precise timing is not very important in the context of this paper. It is because measurement locations are not exactly the same from one measurement to another at locations near EPICA DML. As the reviewer suggested, the reflector origin is highly likely a few conductivity peaks. We comment that in Dome Fuji Station ice core there are a few major ECM peaks at the depth of this radar isochrone. At EPICA DML, estimated depth in our TCD paper was 1882 m. Eisen et al. (J.Glac., 2006) gave 1866-1869 m. On the other hand, Huybrechts et al. (2009) gave a number "1885.4 m". We comment that the all are within depth of 20 m, which has already good agreement. We do not hope to adjust one data to another at the analysis stage of this TCD (and TC) paper, unless we can be sure that the radar sites are exactly the same among the three different measurements.*

**Y-axis scaling of X-PH plots**

[Reviewer #2]

**I do not fully understand why this scaling issue (p1793) is emphasized so much in the text, as it cannot be applied to all sections anyway. What would be the difference for simply taking the max and min P and H values in the considered data subsection with linear P(H) dependency? Statements on the variation of P as a fct. of x, like the one on p1797 L19f ("Within the give scale of axes, P fluctuates more than H.") tentatively imply a degree of reliability of a physical interpretation of results which I doubt, as issues like the roughness are not considered. At most one could compare the fluctuation of P(x) among different sections, but not the variation of P(x) and H(x). The result (viii) on p1799 does not clearly follow from the presented analysis and results, which I partly attribute to the lengthy description of the results for each individual leg. This needs more attention for focused presentation in the text of this issue at one place and more careful wording. Maybe I overlooked something, but then this could happened to other readers as well.**

*The scaling issue was emphasized because this procedure will distinguish between signals that simply follow the thickness of dielectric attenuation and signals with additional (wet bed) reflections. A single scaling cannot be applied to all sections, as the reviewer pointed out. But without this scaling procedure at legs where the method is well applicable (mainly in inland plateau and coastal area), it is quite difficult to carry out diagnosis of bed conditions. It is because both inland plateau and the coastal area often give relatively simple results of diagnosis. Based on such simpler results, we can examine difficult zones between them, such as intermediate areas like F legs and C legs.*

*If we simply take the max and min P and H values in the considered data subsection with linear P(H) dependency, we will see difficulty to identify signals that simply follow the thickness of dielectric attenuation. It means that we will see difficulty to find the "inflection point" of the "hockey-stick"-like distributions in the data plot. And thus, we will see difficulty to identify the temperate/frozen boundaries.*

*The reviewer commented on the statement of the variation of P as a function of x, like the one on p1797 L19. Variation of the bedrock elevation along the F leg is one of possible explanations for the highly variable echo strength. However, the highly variable echo strength is often found in our diagnosed temperate bed. It is still possible that roughness changes may contribute to some amount. However, heterogeneous distribution (thickness) of water is more likely explanation, as our explanation at page 1805, lines 13-17. We do not agree with the reviewer's comment that we cannot compare between variation of P(x) and H(x). Variation of P(x) compared with variations of H(x) has some physical meaning (temperate/frozen distinction) . Increased (or decreased) roughness at $H_0$ does not provide likely explanations.*

*The reviewer is right that we did not provide sufficient analysis and results for the result (viii) on p1799. In the revised version we will carefully repair this part. Overall, we agree that we should provide careful explanations.*

[Reviewer #2]

**Structure of statements:**

**At several instances it occurred to me, that first a general statement is made (e.g. section 3.7 (i)), which seems to apply to all data. But then a limiting sentence follows. This is confusing at times. I suggest to rephrase such statements to a form like: "For ice thinner than ..., the H-P plots show that ..."**

*As suggested, we will repair such structure of statements in revision.*

[Reviewer #2]

**Proposing drill site:**
**I find it suitable to include the analysis for a possible future drill site in this paper, which is currently buried in section 4.4. As this section is completely different from the rest, I suggest to devote an own section to this issue.**

*As suggested, we will make a section for the drill site topic.*

[Reviewer #2]
**Other Issues**
**• A number of comments and suggested (and not least significant) corrections are annotated in the accompanying pdf.**

*Many thanks for the comments in the annotated pdf. We will respond one by one in revising the paper.*

[Reviewer #2]
**• Bed reflection power: The manuscript elaborates on the variation of $P_{bed}$, but I did not fine a single note on how it is determined from the data. Automatically, semiautomatically, peak magnitude, power integrated in a time window (how long is the time window)? Compare Gades et al., J.Glac., 2000.**

*Bed reflection power was peak magnitude in the time-series of the radar return signals, which will be clarified this in the revised manuscript.*

[Reviewer #2]
**• Section 3.7 "Results summary": this list contains some statements which are no results in the strict sense, e.g. (iii). Point (viii) is difficult to understand and should be rewritten. I suggest to reorder this list to have the important results on spatial variations first and then the rather technical issues.**

*We will examine each items, and then reorder from more important items.*

[Reviewer #2]
**• Section 4.2.5 "Coastal sites": Although legs E1 and E2 are in coastal regions (in the authors' definition), I find it difficult to clearly separate the results from both regions (western DML and Shirase) while reading. Currently, they are both discussed even in the same paragraph. Doesn't make the understanding easier.**

*When we revise, we will attempt to separate them to make the understanding easier. We will attempt to distinguish more between "Coastal area in the Shirase Glacier drainage basin" and "Coastal area in the western DML.*

[Reviewer #2]
**• The manuscripts often states ". . . m deep ice coring site". I suggest to rather refer to drill sites and the boreholes, which are still there.**

*We will repair the manuscript as suggested.*

[Reviewer #2]
**p1787L20 Conceptual error: an inclined reflector does not yield a different R than a flat one, the main reflection just happens at a different place. Unclear, rewrite.**

*We will repair the manuscript as suggested.*

## RESPONSE TO OTHER MAJOR COMMENTS OF THE REVIEWER #3 (C987–C991, 2012)

**Discussion/conclusions**

[Reviewer #3]

**As discussed above, the conclusions section is simply over-long and some care needs to be taken to ensure this is used more effectively to convey the main message of the paper. However, I think the discussion section is actually the section of the paper that most misses its opportunity. One valuable message that can be conveyed is that a new method is presented that, despite its simplicity, presents very plausible results (a trimmed down version of Section 4.1). Section 4.2 does not need to be written in a site-by-site manner, and arguably could be dropped entirely from the Discussions section with some aspects discussed in the Results section of the paper.**

*We will attempt revisions and improvements by following to the suggestions given by the reviewer. We will consider to move some portions to supplementary information.*

[Reviewer #3]

**The spirit of Section 4.3 is worthy, but one could much more meaningfully compare the results in this paper with Pattyn's modelled distribution by presenting comparisons of Pattyn's modelled values along the radar tracks. As it is, the statistics presented (62% versus 23% for the observations, versus 55%/45% for Pattyn's model of the whole of Antarctica) are virtually meaningless.**

*We made more concrete comparison between our diagnosis and the Pattyn's estimation based on models. Please find Fig. R5 below. Predicted bed conditions are shown on mean basal melting rate map of DML. For the comparison, we need to be careful that the very low basal melting rate (<0.2 mm/year in the figure) does not necessarily mean it is frozen. In this comparison, there are frozen zones within the zone of very low basal melting rate (that is, white regions in Fig. R5). We comment that as a large tendency, agreement between the model and our radar diagnosis is good. Major difference can be found near Shirase Glacier where water production is small in the model. However, if we consider that water can drain from the inland plateau to coastal glaciers, it seems reasonable that there are subglacial water there mainly because of water from upstream.*

*In the revised version, we hope to add such a figure for comparison. We will consider to add such a figure either in the main body of the paper or in the supplementary material.*

[Reviewer #3]

**From Section 4.4 I would recommend retaining the interesting comparison of Domes F and A with respect to the contrasting formation mechanisms of the frozen beds, but I am not convinced the section about siting another ice core near Dome F is particularly necessary for this paper.**

*The section about siting another ice core near Dome F is supported by the Reviewer #2. For the problem of the future ice coring, finding a good site is very important. Therefore, we hope to include this topic in the present paper. However, it is also true that some of readers will feel a question if this section is very necessary. We hope the reviewers and the editor that the authors keep some flexibility to consider handling of this section viewing entire balance within this paper. Of course we hope to include it. However, more focused condition is also important.*

[Reviewer #3]

**The linear decrease of bed-power is only expected if ice has similar thermal and chemical characteristics along an entire survey leg. Consequently it would be an improvement to state explicitly that the method outlined in the paper is not applicable to fast flow areas due to shear heating and crystal orientation fabric effects. This is alluded to in P1806 19+ but**

**should be stated as central to the described method, particularly in Fig. 8.**

*We will revise as suggested.*

[Reviewer #3]
**In the text and at least one figure caption, it is mentioned that 14 sections are listed in Table 3. In fact there are 13, but the missing section is C3, for which there were problems obtaining the bed, and I imagine why this is not listed here. But there is a mismatch between this "14" and the 13 that are actually listed which needs clarification in the manuscript.**

*We will describe the numbers as suggested.*

[Reviewer #3]
**The Svea station is mentioned in the manuscript and marked on Figs 2 and 10, so would it be worth adding onto Figs 1 and 11 maps?**

*We will add the mark of the Svea Station as suggested.*

[Reviewer #3]
**P 1792 12 - The term "mid-stream" is rather ambiguous – it implies mid-ice-stream, but the velocities are rather lower than this and are more typical of ice-stream tributary flow. Anyway, I think it is only used in the sense to distinguish the region from coastal and interior zones, so the term "intermediate area" might be preferable. (As it is, the authors alternate between "mid-stream" and "midstream" (e.g. p1797 7, c.f. p1798 10). In the context that the figures are a real strength of the paper –**

*We will use the term "intermediate area".*

**Fig 1: Shirase Glacier label is almost impossible to read, and there is no explanation for the dotted black lines.**

*We will repair the letter and indication.*

[Reviewer #3]
**All X-HP plots – the distance values all seem a bit oddly chosen – I presume they all relate to original distance labels as the traverses were conducted, but why retain these here, rather than just start from 0 on the left of each diagram?**

*So far for the JASE traverse, we have published two papers using the same x scale. Please see Fujita et al. (2011) and Sugiyama et al. (2012). When we compare locations of investigation, the common use of the x-scale makes comparisons easier. We hope to take this benefit by using the present scale.*

[Reviewer #3]
**Spelling/grammar: While the writing/grammar etc is mostly of a very high standard, there are a few detailed typos/grammar issues that I could elaborate on, but since I think the manuscript requires some reworking first I would prefer to leave any such exercise to a future version.**

*Thanks for the comment. Like before submission of the TCD paper, the manuscript will be proofread by a professional English scientific proof-reader. The authors will do our efforts to make better manuscript in terms of English expressions.*

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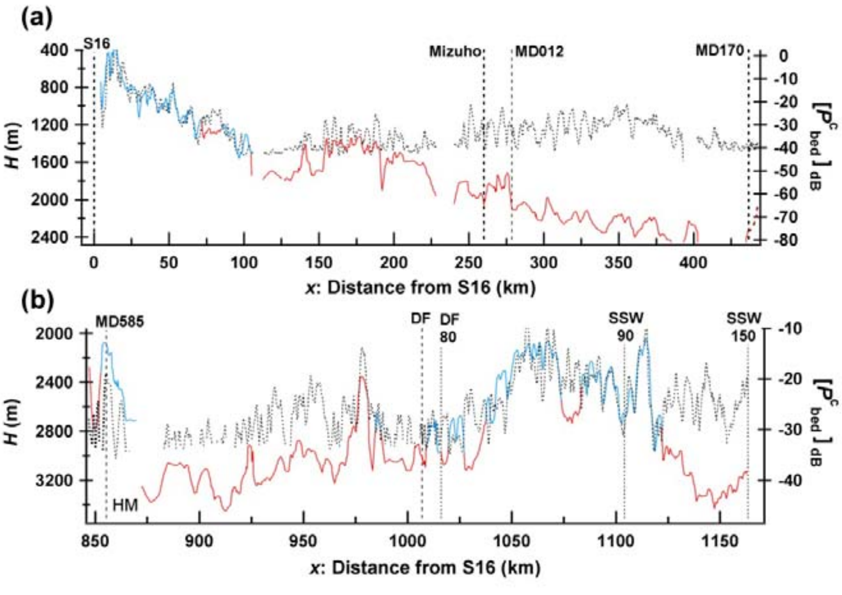


**Fig. R1**. *(a) Modification of Fig. 6f as X-PH plot for leg E2. Data of H are shown with prediction of temperate (red) and frozen (blue) conditions. (b) Modification of Fig. 3b as X-PH plot for legs A1 and A2. Based on the regression lines in the H-P plots, scales of the right and left are determined. In these examples of (a) and (b),we notice that radar echoes often show "jump" at depths near $H_0$.*

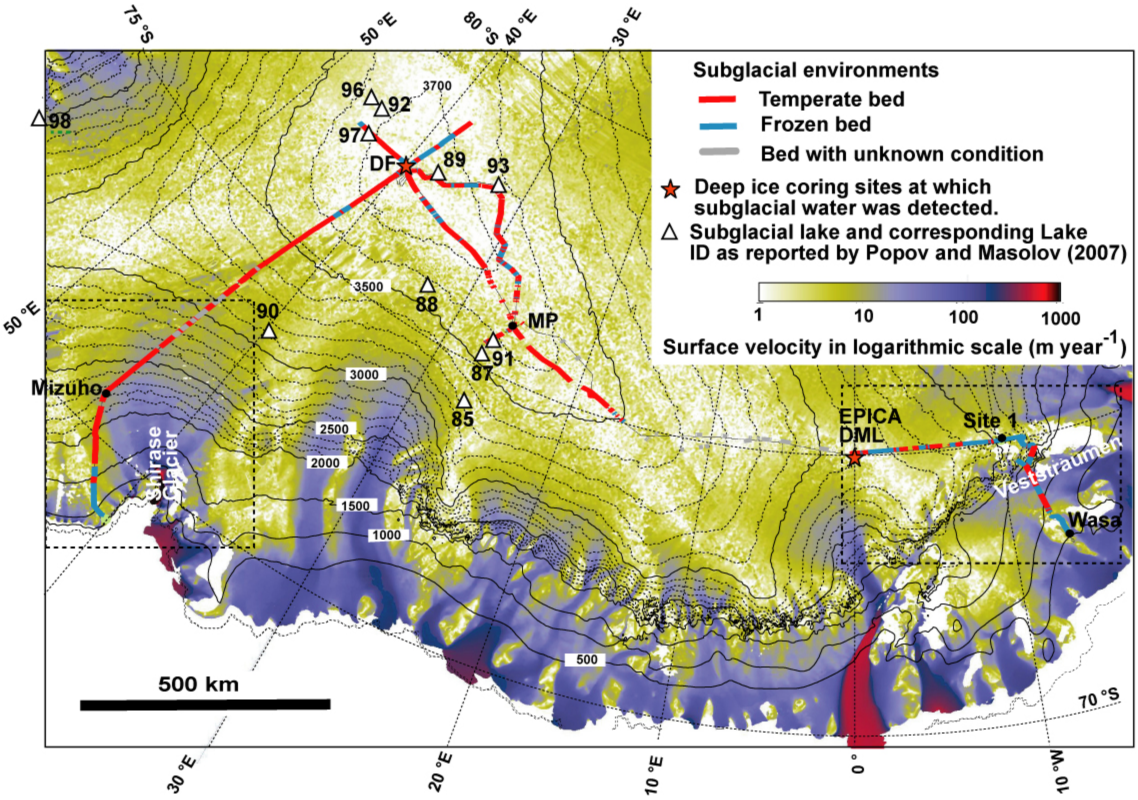


**Fig. R2**. *Predicted bed conditions are shown on ice flow velocity map of DML. Ice flow velocity was compiled by Rignot et al. (2011a, 2011b) based in interferometric analysis for the data of of the satellite-borne Synthetic Aperature Radar (InSAR). The red and blue dots indicate sites of temperate and frozen bed conditions, respectively. The colour scale of ice flow velocity is in logarithmic scale. Surface elevation is shown by thin black contours. The other symbol markers and elevation contour lines are the same as Fig. 1.*

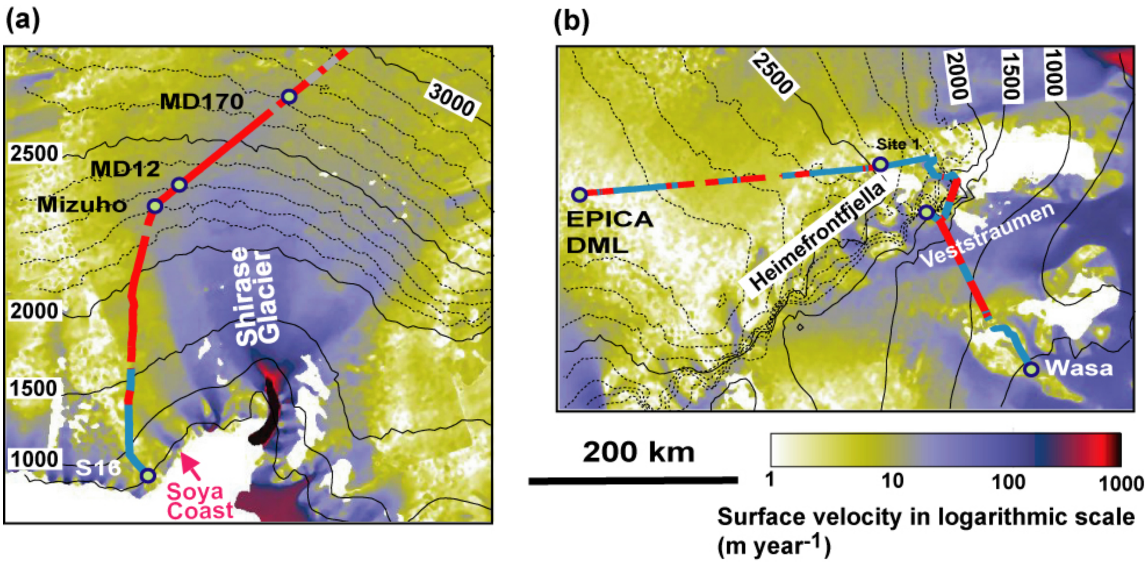


**Fig. R3**. *Image of Fig. R2 was enlarged for two areas near the coast: (a) in the vicinity of Shirase Glacier and (b) in the area between EPICA DML and Wasa.*

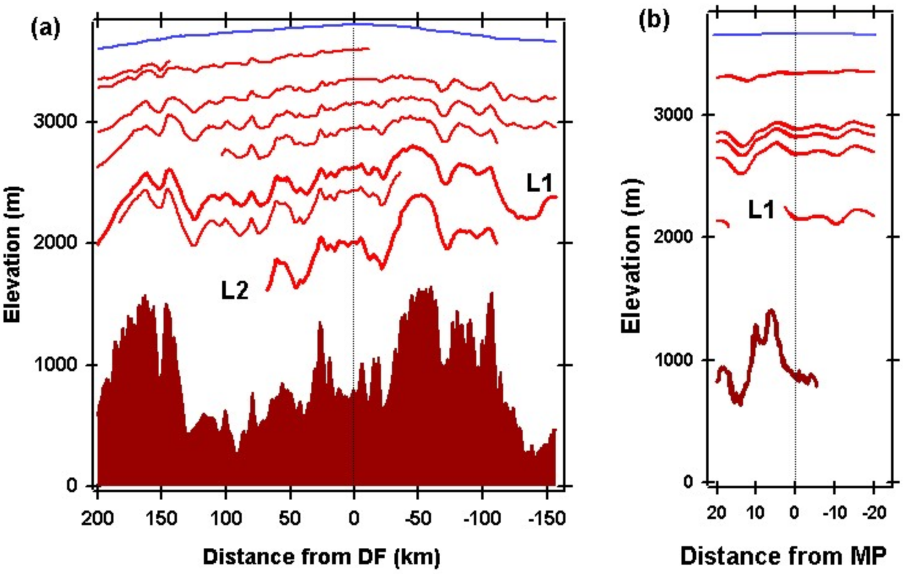


**Fig. R4**. *(a) Distribution of major isochrones for legs A1 and A2, across the ridge of Dome Fuji. The abscissa is the distance from DF. The ordinate is elevation. The isochrones L1 and L2 are the same as those in Fig. 13. The uppermost blue trace is the surface of the ice sheet. Red traces are isochrones extracted from radar images. Shaded area with brown colour is bedrock. (b) Distribution of major isochrones across the ridge at MP.*

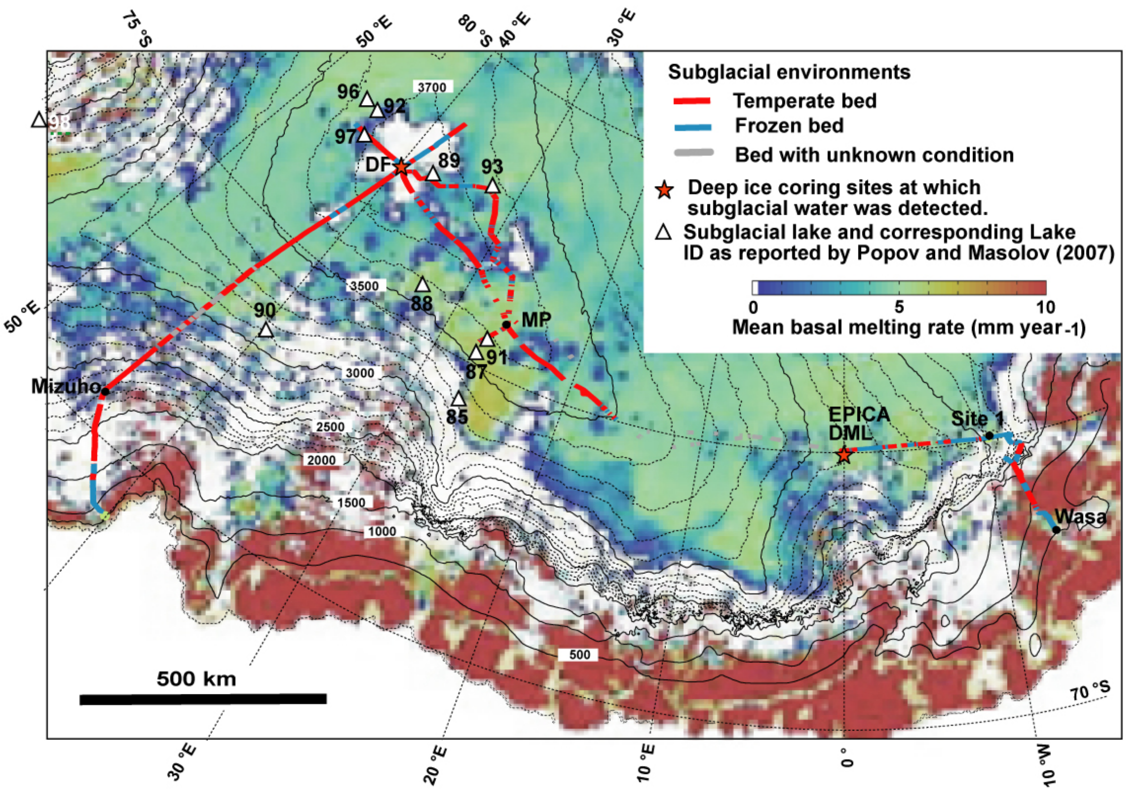


**Fig. R5**. *Predicted bed conditions are shown on mean basal melting rate map of DML. Background image data of the mean basal melting rate was reprinted from Pattyn (2010) with modification of colour scale, Copyright (2012), with permission from Elsevier.*