Dear editor of the Cryosphere and referees,

We have appreciated all the reviewers' thoughtful comments on our manuscript. In the following we reply 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. We hope that the referees and the editor find that the manuscript was fairly revised based on reviews of the TCD paper.

Best regards,

Shuji Fujita on behalf of co-authors

For the paper, three reviewers are, Dr. A. Wright, Dr. O. Eisen and Dr. R. Bingham. In this document, they are is referred to as the Reviewers #1, #2 and #3, respectively. We write reviewers' comments and the authors' response using bold letters and italic letters, respectively. Each item was numbered.

#1. A need for improvement for the structure of the paper and conciseness (advised by all 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 agree that one of main problems of the TCD paper was lengthy conditions. The TCD version had ~12,000 words in the main text. Based on revision, it is now ~8,000 words. Main points of the revisions to make a more concise paper were as follows.

(i) We completely removed discussion on the age of very deep ice. In addition, We completely removed discussions on the isochrones. We focused on the radar diagnosis. As a result, size of the paper is approximately two thirds of the TCD version.

(ii) In the TCD paper, study areas were classified into 7areas. We simplified classification into 3 areas, Plateau region, Midstream region and Coastal region. By doing so, we avoided repetitive statements. Leg names were also changed. Please see the text.

(iii) Generally, trimming was done as much as we could do.

(iv) The conclusions section was reduced to a few short paragraphs, rather than being the more expansive point-by-point summary of the paper.

(v) Previous Figs.2 and 10 were merged as new Fig. 2. Previous Fig. 8 was sent to Supplementary Information D. Previous Fig. 13 was removed.

(vi) Previous Table 2 was sent to Supplementary Information A as Table A. Previous Table 4 was removed.

#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]

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 directions. This point was briefly described in the revised paper at Section 2.3.

Errors of diagnosis

Based on given comments, we examined explanation for errors and uncertainty.

We noticed that previous error analysis had problems because it underestimated the uncertainty. Indeed, in the real data, there are some data that cannot be easily classified either frozen or temperate. In the TCD version of the paper, we insisted on clarifying the temperate/frozen boundaries. But in reality, it was sometimes difficult. We reanalysed the data to make uncertainty of the diagnosis clearer. Followings are points of improvement.

(i) We introduced X-δP plot to tentatively highlight anomaly of the bed returned power.

(ii) Instead of delineating simply temperate and frozen, we introduced the intermediate category "uncertain or intermediate". By introducing them, we tried to provide better analysis and explanations.

(iii) In the H-P plot, we show trajectories (tracks) of the data instead of showing dots. By this, readers can see behaviours of the data more clearly. We added explanations for the data behaviours.

(iv) We gave a new analysis on behaviour of dielectric attenuation in Section 4.1. and in new Fig. 8.

Based on the re-analysis, results were updated showing "beds with uncertain conditions or intermediate conditions. Statistics in Table 3 was also updated.

We must give up a concept of H_0 which was used regional representative ice thickness for the temperate/frozen boundaries. In reality, there should be variations of ice thickness even in a partitioned leg. We introduced a wording "hockey stick like distribution" to explain behaviours of the data in H-P plot. We explained that it is a feature as superposition of many hockey stick like features that has variety of H and P values.

#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.

[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 concept of H_0 was not proper. Ice thickness for temperate/frozen boundaries should be variable. We provide explanations for corrected/improved concept in Sections 3.1 and 4.2.

There are many factors that control the temperature field, such as surface temperature, accumulation rate, advection of ice mass by ice flow, geothermal flux and so on. The partitioning is a practical procedure assuming that conditions of the ice sheet does not

vary much within it. Even with partitioning, there should be variability of H and [P] for the temperate/frozen boundaries. We explained it to readers.

#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. The reviewer's view is that they are sufficiently similar. At least, between these two wavelengths, we found no indications that one of these wavelengths gave particularly weak or strong reflections depending on wavelength or depending on difference in H. Please find very close resemblance between Fig. 3a and Fig. 3c for two different frequencies. Thus, the bed roughness was excluded from major cause of the anomalously high bed returned power.

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 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. Thus, it seems to us that development of theoretical or empirical analysis on this paper is beyond the scope.
At the moment, we find no indication to think roughness played some role for the hockey-stick-like distribution of the H-P plot. Thus we argued that water is a primary cause of the observed features of the data.

In the last paragraph of the Section 4.1, we gave our argument.

#5. Comparison between radar diagnosis of the bed with data of ice motion in Antarctica

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

The editor 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 provide new figures in the Supplementary Information B. We did not include these figures in the main text considering conciseness and readability.

Near the coast, our results of temperate/frozen diagnosis often correlate with contrasts of variations of ice flow speed.

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

#6

[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, T, x, and z are also in logarithmic scale. We hope to avoid such possibility of misunderstanding.

#7

[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.

Please find revised section 3.1. In the 2nd, 3rd and 4th paragraph, concept of the H-P plot is given, using a new expression hockey sticks.

#8

[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 previous expressions here are highly related to the structure of the 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 rough 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. In the revised manuscript, coastal area is discussed in a short section 3.4. We modified expressions largely, for a better understanding of readers. We removed expressions "agree" or "disagree".

#9

[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. We removed this item from the result summary because it is rather a discussion. In the revised manuscript, at the 4 last lines of the conclusion, we mentioned advantage of the temperate bed for radar observations.

#10

[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.

By revision, this statement was removed from the result summary (present section 3.5).

#11

[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.

It seems that our previous expressions to introduce Rignot et al.'s paper was not proper. In the revised paper, it is mentioned in Section 4.4. third paragraph (page 20, lines 3-9). With these expressions we do not give impressions to deny or to discount basis of ice sheet flow. That is, (i) driving force is gravity; (ii) basal topography is one of boundary conditions.

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

#12. 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 Section 4.3.1 (last paragraph) of the main text and in the Supplementary Information E.

#13. 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.

Finally, topic of the isochrone was completely removed together with the topic of the very old ice near Dome Fuji, in order to make the paper better focused and concise.

#14

[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.

Same as above.

#15

[Reviewer #2]

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.

In the result section, we showed Figure 4 and mentioned meaning of it. It contains some discussion on basic meaning of data. However, it seems to us that this section 3 is more proper location to show this to readers.

The previous first paragraph of p 1800 is description of analysis based on the observational result. Thus it seems to us the section 4 is proper location.

Generally, in revising the paper, we carefully tried to make clearer separation of results and discussions.

#16

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.

Meaning of the scaling is now more explained in the revised paper in section 3.1. The scaling issue was emphasized because this procedure 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 this scaling procedure is useful to tentatively identify anomalies of the "hockey-stick"-like distributions in the data plot.

In the revised paper, we gave some explanation for the roughness at section 4.1, last paragraph. We find no indications that roughness played some important roles for observed features of the data.

The reviewer commented on the statement of the variation of P as a function of x, like the one on p1797 L19. The highly variable echo strength is often found in our diagnosed temperate bed. In addition, we find no effect of frequency. Thus it is most likely due to water distribution.

In the revised paper, we attempted to better explain basic idea of the data plots, minor role of bed roughness to explain features of the data, and our argument on the features of the data.

The result item (viii) was removed this item from result summary in the revised paper because it was not sufficiently discussed in the main text.

#17

[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 ..."

When we revised the paper, we tried to detect such portions and tried to repair.

#18

[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.

Finally, topic of the isochrone was removed completely. Discussions on future drill site and isochrones should be developed elsewhere.

#19

[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 responded one by one in revising the paper. Some notes are as follows.

Between equations (2) and (3), we gave an explanation on how we handle potential errors due to $[B]_{dB}$*. The reviewer commented that explanation is difficult for non-radar specialists. We simplified the explanation to tell to readers that* $[B]_{dB}$ *does not cause major problem. We just suggested a reference paper for further reading on that point.*

As for Fig. 10 (new Fig.2), the reviewer suggested to show 2800 m thickness in the graph. However, this addition makes the graph noisy. In addition, we gave up our idea of "H_0*". Thus there is not much benefit to show the 2800 m thickness in the graph. Instead, new Fig. 11 is a graph showing variation of temperate/frozen boundaries. This new Fig. 11 plays a role that the reviewer expected. As the reviewer suggested, leg names were labelled in Fig. 11.*

The reviewer suggested Horgan et al. (2012)'s seismic work in introduction. However, we hope that we do not include this paper in citation in introduction because it jeopardizes the flow of the logic. In the introduction, we are trying to place our radar work in a larger context of polar science. There are many other useful techniques and nice works but not all of them should be mentioned here.

The reviewer suggested us to cite possible contribution of subglacial melting to the mass balance the ice sheet quantitatively in introduction. We thought a possibility to mention quantitatively. However, it cuts stream of the introduction. Therefore, we simply mention Pattyn (2010) for readers who want to know more.

#20

[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.

In section 2.3, our procedures to extract bed reflection power are described .

#21

[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.

This section (new 3.5) was rewritten as a short paragraph without itemization. We tried to better organize this section.

#22

[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.

Please see Section 2.4(iii). In the revised manuscript, we classified all the legs into three groups (Plateau area, Midstream area and Coastal area). As coastal sites, we kept discussion of the two legs in the western DML near Wasa and in the eastern DML near Shirase in the same paragraph. It is because feature of the data have many common points as coastal area. However, to inform readers of this situation more clearly, we modified description. Symbols of the legs "CW" and "CE" were used to help readers to understand one is Coastal West and another is Coastal East.

#23

[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.

An expression of "borehole" was used at Section 4.3.4 (L2).

#24

[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 removed the statement at that point, as suggested.

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

#25. **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.

Many thanks for this comment.

We trimmed the section of concluding remarks, avoiding itemization. Avoiding repetitive statements, we attempted to convey most important messages to readers. We made three short paragraphs. The first paragraph emphasized advantage of the new method. The second paragraph is for new insights into distribution of the water production beneath Antarctic ice sheet. The final paragraph is our implications for the future scientific actions.

In addition, section 4.2 was written in less site by site manner. We believe that we cannot drop it because discussions should be clearly separated from results. To respond to the review comments, we avoided both repetitive statements and itemization.

#26

[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 the Supplementary Information F. We put this topic in the supplementary information not to cut stream of the discussion. We provided our comments on both common points and differences. In general, we believe that the comparison is very useful, providing insights into spatial distribution of the subglacial conditions.

#27

[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.

To make the paper better focused and concise, we removed a section on siting another ice core at Dome Fuji, as suggested.

#28

[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.

In the stage of the interactive discussions, we commented that we would revise as suggested. However, to think more, fast flow areas does not necessarily mean that there is shear heating or frictional heating, because of lubrication by water. And even if it happens, it is likely that the heat is localized near the bed because of weak heat conduction within ice. Such heat only contribute melting late but not much for the temperature distribution. Therefore, it seems that it is not appropriate to provide a statement that the method is not applicable to fast areas. If heating really occur due to basal shear, it means that dielectric attenuation gets larger in such heated ice column. But in reality of the fast flow both in the Veststraumen or Shirase area near Mizuho, we find no features indicating some increase of dielectric attenuation. It seems to us that thermal structure within the fast flow ice is a topic for future studies. From a viewpoint of conciseness of the paper, we do not hope to develop this statement in the present paper.

#29

[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.

In the revised manuscript, we avoided to mention total number of legs.

#30

[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 added the mark of the Svea Station as suggested.

#31

[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 –

In the revised manuscript, we kept the word "midstream". But to clarify meaning of this word in this paper, we provided an explanation in the Section 2.4 (ii).

#32

[Reviewer #3]

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

We repaired the letter of the Shirase Glacier label. Also, dotted black lines were explained in the figure caption.

#33

[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.

#34

[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 was proofread by a professional English scientific proof-reader.

#35. **Other changes**

(i) The editor introduced us a recent paper Wright et al. (2012). We understood that the paper is very relevant to the topic of our paper. We mentioned comparison between their

scientific output and ours in the last paragraph of Section 4.4. A paper Gades et al. (2000) was also added to citation.

References

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Seddik, H. (2008). "A full-Stokes finite-element model for the vicinity of Dome Fuji with flow-induced ice anisotropy and fabric evolution." Doctoral thesis, Graduate School of Environmental Science, Hokkaido University, Sapporo, Japan. Hokkaido University Collection of Scholarly and Academic Papers (HUSCAP).

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