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The Cryosphere Discussions

Interactive comment on "Radar diagnosis of the subglacial conditions in Dronning Maud Land, East Antarctica" *by* S. Fujita et al.

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This paper is primarily concerned with analysing over-snow radar traverse data collected during the Japan/Swedish (JASE) traverses conducted across DML during 2007/08 (with some 1996/97 data from the Dome Fuji area also supplementing the overall dataset). The main objective is to use the data to derive information about the condition of the bed beneath the ice, chiefly whether it is temperate or frozen to the bed. A secondary objective is to present an innovative and practical technique to distinguish wet/frozen basal conditions without the need for accurate englacial attenuation parameterisation (normally a significant source of error). It is found that below a critical ice depth the relationship between bed-power and ice depth follows a quasi-linear trend. The spatial characteristics of this relationship are then interrogated to infer basal conditions of the ice-sheet. Addressing the spatial variation in this way this work represents a significant development of work by previous researchers who have used this relationship to derive englacial attenuation rates (e.g. Gades et al., 2000; Jacobel et al., 2009; 2010).

On the whole I found the paper interesting, and judge that the topic, the impressive data set, and aspects of the analysis, are undoubtedly of wide interest to TC readers. In general, but bearing in mind what I am about to say, the writing is of a clear and lucid standard, and the diagrams are well put together. However, there are some aspects of the paper that could be improved to help the paper achieve a wider impact. Firstly, the paper requires some restructuring to improve its conciseness. Secondly, for a paper that discusses at length the derivation of bed-reflection strength, more detail could be provided as to how exactly the bed reflection strength is actually determined from the radar signal in the first place. Thirdly, I am unclear as to why the analysis is undertaken entirely in such a partitioned manner, i.e. results for section A, section B, section C etc. - when some answers might be obtained by pooling all of the obtained data. Finally, I think the discussion and conclusions sections are rather unfocussed in their current forms. I will try to expand on these points below.

The below may appear rather critical but I should emphasise that overall the paper represents an impressive effort in data collection and analysis, and for this reason would recommend the paper continues to be considered for publication, in the context that my concerns below can be addressed by the authors.

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.

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.

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.

Discussion/conclusions: As discussed above, the conclusions section is simply overlong 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. 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. 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.

Minor issues:

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 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.

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?

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 –

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

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?

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.

Interactive comment on The Cryosphere Discuss., 6, 1781, 2012.