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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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General

Fujita and others interpret an extensive radar data set from Dronning Maud Land, Antarctica, to deduce the binary conditions of the ice–bed interface: whether it is wet or frozen. To that end they extent existing methods to analyse the variation of bed reflection power as a function of ice thickness. Their new approach assigns wet bed properties to the locations of such radar data, where the bed reflection power (in dB) does not linearily decrease with increasing ice thickness. This is mostly the case for ice thicker than around 2500 m. They comprehensively analyse and interpret their data sets to provide the overall distributions of bed conditions along their profiles for several regions, which are discussed in terms of the prevailing glaciological conditions and

possible reasons for having wet subglacial conditions.

As a second topic, they trace and discuss the origin of a prominent internal layer they attribute to the Toba eruption around 73 ka. Together with the deduced bed properties, they then provide a separate discussion of their implications for retrieving Ma old ice in the Dome Fuji area, with a recommendation for a drill site.

Overall, the study is of considerable scope and relevance to The Cryosphere. The heuristic method presented here complements other existing ones, which are physically more sound but also require more in-situ data from ice cores to pin down the contribution of impurities to radar power attentuation. In this respect, the analogous application to other data sets will likely improve our understanding of the distribution of wet subglacial environments to a first order in Antarctica.

Major Issues

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 anaylsis 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 examplarily described for one leg class right at the beginning.

Error analysis

Error estimates are stated to be a few percent throughout the manuscript, but clear derivation and discussion of the uncertainties is 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.

Raymond effect:

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 wether the suggested effect is large enough to cause profound changes at the bed.

Internal isochrone:

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 interupted 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.

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.

Y-axis scaling of X-PH plots:

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) dependeny? 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 carefuls wording. Maybe I overlooked something, but then this could happend to other readers as well.

Structure of statements:

At several instances it occured 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 ..."

Proposing drill site:

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

Other Issues

- A number of comments and suggested (and not least significant) corrections are annotated in the accompanying pdf.
- 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.
- 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.
- 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.
- 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.

- The manuscripts often states ". . . m deep ice coring site". I suggest to rather refer to drill sitess and the boreholes, which are still there.

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.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C972/2012/tcd-6-C972-2012-supplement.pdf

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