The Cryosphere Discuss., 7, C2512–C2519, 2013
www.the-cryosphere-discuss.net/7/C2512/2013/

The Cryosphere Discussions
Open Access

Interactive comment on "Transition of flow regime along a marine-terminating outlet glacier in East Antarctica" *by* D. Callens et al.

R.G. Bingham (Referee)

r.bingham@ed.ac.uk

Received and published: 9 December 2013

General comments

This paper is centred around the analysis of new airborne radar data collected along and across West Ragnhild Glacier (WRG), an outlet of Dronning Maud Land (DML), East Antarctica. The glacier has otherwise been mostly unstudied but the authors demonstrate with this contribution that it has potential to be considered a significant contributor to the overall mass flux from DML. This is further made important as WRG could become more responsive to oceanic forcing with the removal of the buttressing Roi Baudouin Ice Shelf. Using the radar data, the authors present and discuss the basic topography beneath WRG, and further analyse both bed roughness and bed reflectivity. They show that the bed is deeper than previously interpolated in BEDMAP,

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

hence that ice flux is greater than previously assumed (notably in Rignot et al.'s 2008 compilation which used BEDMAP for ice thickness in the absence of other data). The authors also suggest that the bed is significantly different in character (manifested by bed roughness and bed reflectivity) between its downstream zone (within $\sim$ 65 km of the grounding line) and upstream zone (> 65 km from the g.l.). The ice-dynamical implications of this change in the character of the bed are investigated using a flowline model, and it is concluded that downstream motion in WRG is dominated by basal motion. As part of the study the authors have also generated a new map of ice-surface velocities from satellite data across the region which contributes to several aspects of the analysis.

Overall I found the paper interesting and the acquisition and analysis of these data in a previously unstudied region which perhaps has the potential to be more significant to outflow from East Antarctica (specifically the DML sector) than previously considered merits eventual publication. I do think, however, that some clarification on several issues is required with some rewriting and perhaps extra analyses in places, which I will discuss immediately below. Further below I also suggest various technical corrections.

Specific comments

Q1. One of the themes of the paper is that the mass flux of WRG is more significant, and proportionally a greater component of mass outflow from DML, than previously known. This conclusion is apparently reached predominantly on the basis that the bed is deeper than previously interpolated, i.e. from BEDMAP (Lythe et al., 2001). Yet I feel there are some ambiguities to this conclusion. My first query is what actually was the estimated mass flux for WRG individually prior to this study? In Rignot et al. (2008) WRG is conflated with several other outlets along the Queen Maud Land coastline, so from that paper there is no individual estimate. I suspect from this there has been no published estimate for WRG individually; but nevertheless the authors could easily work out what it would be if one didn't have the new bed data, i.e. using the bed from BEDMAP to calculate mass flux from WRG. That way one would have a

TCD
7, C2512–C2519, 2013

better sense of how much difference the new bed actually makes. (What I am getting at here is whether it's the new bed that makes us realise WRG is a bigger contributor than previously thought, or whether it's just been overlooked as a significant contributor because previous studies never actually looked at it individually.) My second query is how well do we know the bed for the flux gates of Shirase Glacier and Jutulstraumen? - Is there a possibility that these beds too could be deeper than in BEDMAP's interpolation, and therefore that their mass flux is ALSO greater than previously thought – and that, therefore, as a proportion of mass flux from DML, that of WRG could still be less significant compared with Shirase Glacier and Jutulstraumen? Thirdly, can the authors confirm that the comparison between Shirase/Jutulstraumen and WRG also uses satellite-derived ice-surface velocities across the flux gates from the same eras?

Q2. It would be useful to have explained in the paper why the authors generated their own surface-velocity map from satellite data across the region, i.e. rather than using the MEASURES product of Rignot et al. (2011). It's a fine product but I just didn't appreciate from the paper as written what added value is given by doing this rather than using MEASURES. Was this a case of generating a product with improved spatial resolution for this area? Are the 2000 data actually the most recent for this area?

Q3. Much is made in the paper that the downstream (flatter/smoother) sector of WRG is "wide" and smooth and would leave the catchment open to ice-stream lateral migration. However, when I view both Figs. 2 and 3, what I see is that the eastern part of the catchment, all the way downstream, i.e. even in cross-profile 1, appears to be underlain by a fairly rough, undulating bed that is quite topographically high relative to further west. In other words I think the eastern part of the downstream section (< 65 km from the grounding line) actually appears more similar in bed character to the upstream section. There is still a smooth-ish, wide-ish downstream area from the centre to the west of the downstream profiles, but the way the manuscript is currently written, one would get the impression that it's wider than it is. It looks to me as if the rough bed to the east would limit the ice-stream's lateral migration, so from this dataset I'm less

TCD
7, C2512–C2519, 2013

convinced than the authors imply in their discussion that this really is an analogy for the Siple Coast region.

Q4. Can the authors clarify why they did not present bed reflectivity data for the 7 transects across WRG to supplement its analysis along the single long profile. These data would help to test further my contention made in Q3.

Q5. Can the authors also clarify why they present BRP(c) [the simple geometrically-corrected BRP; Fig. 4b] AS WELL AS the englacial-attenuation-corrected BRP (Fig. 4d). As I understand this, one is simply making the first step towards the other, so I'm unclear as to why both are presented here, i.e. why not just present the geometrically AND englacial-attenuation corrected product (as in Fig 4d) alone? The only argument I could see for retaining the result in which englacial attenuation has not been included is that one may be insufficiently unsure of the assumptions that go into generating the attenuation estimate. However, as the results in Fig. 4b and d essentially match each other in all main respects, it seems that presenting Fig 4b and discussing it independently seems a little superfluous.

Technical corrections

Firstly, this isn't a specific point, but I think the writing could be made more concise, and more precise, in several places. The introduction is quite long, and for a paper that essentially presents new data from East Antarctica, there is a lot of material on West Antarctica in the first paragraph. It is contextual, and I wouldn't drop it totally, but I would consider reordering the introduction a little. As a further example, I do not think, with the general format this paper has followed (i.e. mini methods sections followed by mini-results/discussion sections) that both the final sections (7. Discussion and 8. Conclusions) are required. Section 8 just seem to provide unnecessary repetition. Another issue to clean up is that there is much alternation between use of past and present tenses in various sections, and it would be better to try to stick to one type. Throughout manuscript: check that all instances of "Antarctic Ice Sheet" are written with

TCD
7, C2512–C2519, 2013

uppercase “I” and “S” and all instances of West Ragnhild (or other) Glacier are written with capital “G”. Also I think it’s unnecessary to use “the” in front of “West Ragnhild Glacier” which occurs in quite a few sections of the writing.

Throughout manuscript: check spelling is consistently UK English or US English – currently it’s mostly UK English but some examples of US English (to be changed?) are color (UK = colour; especially this is in the figure captions text) and center (UK = centre).

Throughout manuscript: Some overuse of hyphens for East-Antarctic and West-Antarctic. At some point, it might be noted that WRG is the same glacier/ice-stream as that which Rignot et al. (2011, p.1428) refer to as an “unnamed glacier. . .which for convenience is referred to as Sor Rondane. . .Ice Stream.”

P4914 L11: Better written as: “..regions, and is consistent with the downstream bed being draped with sediment.”

P4914 L12: This is a little awkwardly written, and I think it would be easier in the abstract to remove the reference to 20 km and just say the BRP varies across the transition. Suggest: “. . .varies by 30 dB across the bed-character transition, which. . .”

P4914 L13: Rewrite: “. . .that the water content at the bed/ice. . .”

P4914 L14: “All the above lines. . .”

P4914 L17: “. . .relatively. . .”

P4915 L3: Also could reference Pritchard and Vaughan (2007) JGR here.

P4915 L8: I would replace “currently” with “conventionally” here, just to make it clearer that ideas are changing a little here now that it’s confirmed from several recent surveys that there are several substantial “marine” basins in East Antarctica too.

P4915 L11: Very ambiguously written. It’s not “contact with the ocean” per se that makes the WAIS vulnerable. This needs a little rewrite.

TCD
7, C2512–C2519, 2013

P4916 L26. No need for the brackets here. Just make this its own sentence; also rewrite as "...250 km upstream from the grounding line.

P4917 L1-16: Lots of use of present tense here where I would suggest using past tense – the survey is something that has been done in the past.

P4917 L8: The Nixdorf and Steinhage references are over a decade old – can the authors clarify if the radar system and its way of operation really has remained unchanged over all this time, or should they be using a more recent reference?

P4917 L10: "...data gaps slope down steeply..." "...Therefore, the data..."

P4917 L12: "...and hence loss of signal." (instead of "which may explain...")

P4918 L3-5: Slightly unsure as to what is meant by the landward valley between Sor Rondane and Belgica Highlands... Does this mean the downstream basin is flatter than the bottom of the deep valley along its long profile?

P4919 L11-13: I'm unclear what this means. Smoother than what?

P4919 L24: "...can be treated as a proxy for bed reflectivity..."

P4920 L1: "In the decibel scale..."

P4921 L11: singular, "field"

P4921 L22: Can you offer an explanation for why the attenuation decreases $\sim$ 20 dB just here? Further upstream you attribute the change to changing ice thickness, but proffer no explanation for this change here.

P4922 L25: I think "DEM" is a better word than "profile" here.

P4923 L1: "...data have been..."

P4923 L3-8: Clarify quantitatively what is short and what is long.

P4923 L16: Replace "i.e.," with "of"

TCD
7, C2512–C2519, 2013

P4923 L18: plural – “procedures”

P4923 L19: “. . . (2012), which allow us to perform diagnostic runs. . .”

P4923 L24: “. . .derived the corresponding. . .”

P4923 L26: Remove “However” (you use it again in the next sentence). Also remove “probably” in next line.

P4923 L30: No hyphen in Fox Maule (and also change in reference list)

P4924 L3: “. . .heating have virtually. . .”

P4924 L24: “. . .though the exact onset. . .”

P4925 L1: plural, “experiments”

P4925 L2-5: Needs a rewrite, maybe splitting into two sentences.

P4925 L8-10. Here again I rather question the assertion that WRG has been so relatively ignored as a contributor of discharge. Rignot et al. (2011) clearly highlight the importance of “Sor Rondane Ice Stream” (which is in fact WRG) already.

P4926 L12: I question the phrase “devoid of distinct lateral constraints.” This goes back to the points I made in my Q3 above.

Section 8: Is this section actually necessary?

P4930 L21 sp. Database

Fig 1 caption. Is the phrase “a couple of PALSAR imagery” correct? You also need to clarify in the caption that the annotations SRM and BH refer to Sor Rondane Mountains and Belgica Highlands respectively.

Fig 2. It would be useful if the 7 cross profiles were annotated 1 to 7 in this diagram to speed up ease of reference when flicking between the text and this diagram.

Fig. 3 This diagram would be really helped if some surface-velocity contours could be

TCD
7, C2512–C2519, 2013

added to guide the viewer as to the main flow over the area, so as better to compare with the subglacial roughness signal. Fig 3 caption line 3 should also read “result” (not “results”) and line 4 should read “corresponds to a” instead of “refers”.

Fig 4c. It’s really hard to see three distinct lines in this diagram, and although the statement is made in the caption that colours correspond to those in Fig 5, I think it would also help to see A,B and C labelled. As I’ve mentioned above I’m also not clear why

Fig 4b is even presented. If it were not, the vertical scaling of Figs 4c and d could be improved, better to see the distinct lines (even if one point of presenting Figc is to show general agreement between models).

Interactive comment on The Cryosphere Discuss., 7, 4913, 2013.