The Cryosphere Discuss., 7, C2344–C2347, 2013
www.the-cryosphere-discuss.net/7/C2344/2013/

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.

Anonymous Referee #1

Received and published: 9 December 2013

Review of "Transition of flow regime along a marine-terminating outlet glacier in East Antarctica" by D. Callens et al., for The Cryosphere Discussion. General Appreciation

This paper addresses the physical glaciology of an East Antarctic outlet glacier, the West Ragnhild Glacier (WRG), specifically one that flows out through a mountain range. The paper contains (1) an Introduction, which sets out the importance of WRG in terms of its flux; (2) a brief discussion of the data acquisition, namely the ice surface velocities from Radarsat; airborne radar surveys to determine ice thickness and bed geometry; and laser altimetry for the ice surface; (3) an account of how the new sub-glacial topography differs from previous estimates; (4) a spectral analysis of the bed topography which investigates how the bed roughness varies along the flow; (5) an analysis of bed return power , again looking at along-flow contrasts; (6) some ice-flow

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

modelling to determine the proportion of ice flow accomplished by internal deformation; (7) some discussion, and (8) Conclusions.

This is a noteworthy paper as it brings to attention the consequences of the revised bed topography for WRG. The paper is mostly well written and well executed; the exception is the modelling section, which in my opinion is poorly executed. I think that the authors would be better advised to compute the deformational velocity simply using the shallow ice approximation; at the moment their attempt to do this with a higher order model seems to me to run into trouble for reasons I expand upon below. They seem to have a choice of 'down-grading' to a SIA –based approach or adopting a more sophisticated higher-order approach – I suspect the latter won't provide much greater precision.

The paper content is novel and has direct relevance to interest in exactly how 'stable' East Antarctica is. For that reason it should eventually be published. I wouldn't like to see the modelling as a published example of how to deal with the glaciological issues it is concerned with, so I recommend substantial revisions.

Major Points

Section 5. I understand that you don't know the impurity content when estimating the attenuation, but some discussion of the implications of increased attenuation from impurities on the bed reflection power is needed. I guess it doesn't make much difference to the interpretation, but it needs to be spelled out.

Section 6: 4923 11-13. I don't understand this – it looks like you are trying a clever way of avoiding tuning the sliding coefficient. Is it correct that both velocity components set to zero? Vertical I understand, but horizontal I don't, given that you are trying to estimate deformation and sliding components. I think that you could get away with your approach if the sliding velocity were uniform, which would be indicated by a spatially uniform mismatch for each temperature case. You don't have this, so in consequence you aren't computing the contribution the changes in sliding velocity make to the longitudinal stress gradients. In summary, I think that this is wrong. Actually I would

TCD
7, C2344–C2347, 2013

adopt a balance velocity approach, perhaps using the approach I suggest in the next paragraph.

Section 6. 4923 17-23 This is a bit odd. I don't see that there is a problem in assuming a surface temperature, starting off with say a linear vertical velocity profile, calculating the temperature, using this to calculate a modified vertical velocity profile and iterating in order to get the deformational velocity profile. At the moment your uniform temperature assumption of -2C etc. are somewhat plucked out of thin air, and I don't really see why the results are of general interest.

Minor Points

âĂć 4914: 10 "Spectrum analysis" -> "Spectral analysis" (and presumably many other places)

âĂć 4914: 19 "region which" -> "region, which" ('which' only appears after comma)

âĂć 4915: 7 Statement about E Antarctica needs to be a bit more nuanced, see e.g. Rapid, climate-driven changes in outlet glaciers on the Pacific coast of East Antarctica B. W. J. Miles, C. R. Stokes, A. Vieli & N. J. Cox Nature 500, 563-566 doi:10.1038/nature12382

âĂć 4915: 15 "are not yet" -> "have yet to be"?

âĂć 4915: 20 "reach up to the continental shelf" -> "reach out to the continental shelf edge"?

âĂć 4915: 26 "one" is redundant.

âĂć 4915: 26 "away" -> "upstream"?

âĂć 4916: 4 "characterized" -> "dominated"?

âĂć 4917: 11 "low" -> "deep"

âĂć 4917: 21 "topography" -> "topography between . . .." – sentence doesn't quite scan

TCD

7, C2344–C2347, 2013

at the moment.

âĂć 4919: 3; “perform” -> “to be able to peform”

âĂć 4919: 19: Are the characteristics of the upstream area roughness such that you can rule out glacigenic sedimentary landforms such as drumlins?

âĂć 4922: 22: “compared to the” -> “compared with our”

âĂć 4923: 24: “derived” -> “derived the”

âĂć 4923: 25 “use” -> “used”

âĂć 4923: 27 “because of the presence of sediment” – this seems to imply that the sediment is releasing heat – doesn’t seem right to me.

âĂć 4925: 1: The reference to Hindmarsh 1993 is a bit bizarre in this case, I’m not convinced it’s relevant. I would start with looking at Barcilon and MacAyeal (J. Glac., early 90s)

âĂć 4926: 3: How far upstream of the grounding line would buttressing slow the ice-stream? There is a paper by Van der Veen and others (J. Glac, 2011?) on Jokabhavn Isbrae which addresses this issue, and comes to answer of kilometres. Maybe you could try the same approach – I suspect the answer will be quite small.

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