

Review of “Glaciological settings and recent mass balance of the Blåskimen Island in Dronning Maud Land.”

General Comments

Goel and coauthors report field observations from a relatively large ice rise that sits between Jelbert and Fimbul Ice Shelves in Dronning Maud Land. They use two ice-penetrating radars with different frequencies, stake measurements and surface GPS, to constrain the topography, thickness and internal stratigraphy of the ice rise as well as obtain several spatially-distributed estimates of mass balance.

The observations are well reported and are likely to be of interest to the glaciological community. Specifically, they may interest those interested in the surface mass balance and long-term evolution of the ice rises in Dronning Maud Land. As the authors mention, ice rises are important because they influence regional ice dynamics and encode information about the stability or otherwise of these dynamics in their internal stratigraphy. Ice rises also influence atmospheric circulation and therefore surface mass balance and are useful locations for drilling ice cores. Overall, any information we can gain on the glaciological setting and mass balance of ice rises, as well as their long-term stability is useful and worth publishing. In my opinion, this work should be published in the Cryosphere after revision to address the comments below.

Overall, the manuscript is written well, with some exception. There are many grammatical and other errors, as well as some passages that I found it difficult to understand. These difficult-to-understand phrases, various errors and other places where I propose that rewording could improve the paper, are indicated below in “Technical Corrections”. Note that due to the high density of errors, there are likely to be many that I missed and I suggest that any revised manuscript is read carefully for this kind of error.

Individual scientific comments are described below in the “Specific Comments” section.

Specific Comments

Section 5.2 describes how shallow radar layers are used to derive SMB. This involves multiplying the depths of three dated layers by the snow/firn density to compute the mass accumulated since each layer was laid down. Spatial variations in density are taken account of using two methods. Very little detail is provided of the second method. An in preparation manuscript is cited and one sentence (L3-5, P7) describes the inverse method. Because (1) very little detail is provided (certainly too little to be able to reproduce the results) and (2) this method is apparently used to take account of a spatial variation in near-surface density of only $\pm 2\%$, I suggest this alternative method is removed from the manuscript. The first method takes account of the much larger vertical variation in density and seems adequate. I would suggest removing the second method and simply stating that in this case the $\pm 2\%$ variation in density is small and that it is possible to alternatively take account of this at the expense of taking into account the vertical variations and that a manuscript is in preparation that will report ongoing work related to doing this with an inverse method.

Page 5, L14-15: A value for the SMB of the whole island is quoted and an uncertainty is estimated based on measurement errors and uncertainty accounting for snow densification. Does this uncertainty estimate also take into account uncertainty associated with the interpolation? This is difficult (as the authors point out later in the manuscript), so if not, perhaps it is better to quote the SMB as the average of the stakes measurements, rather than as the average for the whole island, as this reads to me.

Conclusions, page 13, L21-22: As sastrugi patterns can change from one day to the next, I am not sure that this a robust conclusion to draw about SMB patterns from unquantified observations of sastrugi in the

field. I agree that in general it is tempting to conclude that there must be lower accumulation where you see sastrugi in the field (I have seen this myself on lower accumulation sides of ice rises), so I think it is reasonable to mention this earlier in the manuscript, but I don't think it belongs here as one of your main conclusions.

Technical Comments

Title: To my reading the 'the' before 'Blåskimen Island' is unnecessary. Indeed, in the abstract the name of the ice rise appears without the 'the'.

Page 1

L7: Suggest replacing the sentence starting with "Radar stratigraphy..." with "Arches in radar stratigraphy observed with radar suggest that the summit of the ice rise has been stable for ~600 km." This avoids introducing the concept of characteristic time in the abstract, which may be confusing to those not familiar with it.

L11: Is there a reference for the statement about 74% of the coastline being surrounded by ice shelves?

L14: 'are' → 'is'.

L15: Suggest delete 'eventual'.

L16: 'the ice shelf' → 'ice shelves'.

L16-17: this sentence about buttressing seems to repeat what was said in the previous paragraph.

L23: Two more recent papers that use isochrones to constrain millennial scale ice-rise evolution that could be referenced here are:

Drews, R., Matsuoka, K., Martín, C., Callens, D., Bergeot, N. and Pattyn, F., 2015. Evolution of Derwael ice rise in Dronning Maud Land, Antarctica, over the last millennia. *Journal of Geophysical Research: Earth Surface*, 120(3), pp.564-579.

and

Kingslake, J., Martín, C., Arthern, R.J., Corr, H.F. and King, E.C., 2016. Ice- flow reorganization in West Antarctica 2.5 kyr ago dated using radar- derived englacial flow velocities. *Geophysical Research Letters*, 43(17), pp.9103-9112.

Page 2

L1: Suggest replace 'using an ice core from the summit' with 'using ice cores drilled through ice rises'.

L4: insert comma after '(Fig. 1a)'.

L12: Is it narrower by a factor of two and slower by a factor of two or does the factor of two only apply to the 'slower'? Suggest switch order to '...slower by a factor of two and narrower...' if appropriate, to avoid confusion.

L16: Reword the last sentence. 'Whereas towards east...' doesn't read well to me.

L19: 'summer' → 'summers'.

Page 3

L3: 'were' → 'was'

L4: How many were lost and why were they lost? Were they buried? Also did you have a criterion for rejecting stakes which were tilted too much? Surely all stakes are tilted slightly. If so, it's not correct to say here that you rejected stakes that were tilted.

L5: Suggest move 'relative' to between 'stakes' and 'to'.

L6: Suggest 'infer approximate' → 'estimate'.

L13: Suggest 'to the sea level' → 'to heights above local sea level'.

L13: This reads as if you used the gravity product to subtract the value. Suggest reword to say that you subtracted the value that was supplied by the gravity product.

L19: 'profiles' → 'surveys'. It looks like you did more than two profiles.

L21: Delete 'a'.

L22: Delete commas after 'radar'.

L23: Insert 'a' before dewow and Ormsby. Also 'Post processing was made...' does not read well.

L24: 'the' → 'a' and insert 'of' after 'speed'.

L25: How is the firm correction computed? Using the firm cores presumably, but this is not mentioned here.

L29: Transported the samples where?

L30: Suggest delete 'backed'.

Page 4

L1: 'to' → 'in', insert 'the' between 'from' and 'surface'.

L5: delete 'of'.

L9-10: suggest delete 'giving a relative dome-shaped topography to the ice rise'.

L11: Not clear what 'a flatter basin northeast' means and it is grammatically incorrect. I cannot see a clear distinction between slopes in the northeast and in the other directions. Also I can't see clear distinctions between basins in figure 2a.

L12: The line 2-2' isn't strictly a flow line as it passes through the ice divide. I guess it is two flow lines connected together, but describing it as a flow line I don't think is correct.

L17: The estimate of the vertical uncertainty of $\pm 5\text{m}$ could be more fully explained here. The center frequency of the deep radar was 2 MHz, corresponding (I think) to a wavelength of $c/n_i/2 \times 10^6 = 84.2\text{ m}$, where c is the speed of light and n_i is the refractive index of ice. This wavelength is considerably more than your estimated uncertainty in digitizing the bed reflector. Is the higher precision achieved due to the signal being quite broadband? Perhaps this can be explained in more detail, as one might expect a bed reflector imaged with an 84 m wavelength radar to manifest as a layer thicker than 5 m.

L27: Suggest delete 'below'.

L28: The surface velocity measurements are described as a surface velocity field here, when they really appear as just point measurements in figure 2c, rather than a field.

L28: Suggest 'from' → 'of'.

Page 5

L9: Is this surface density or the density averaged over the top 3 m of all the cores? It is confusing because the previous sentence mentions "surface density variations", but this sentence only says 'firn density'.

L10: Suggest replace 'To estimate..' with 'When estimating SMB below...', or similar.

L17: It is stated here that *most* SMB values lie between the first and third quartiles, but doesn't this by definition mean that half the measurements lie between these two values (i.e. half ≠ most)?

L22: I am not sure I see the relevance here of the parentheses about wind direction.

Page 6

L6: Is it not more precise to say that it is the larger vertical strain rates near the surface at the divide (rather than strong horizontal variations in vertical strain rates) due to the Raymond effect that could mean that the shallow-layer approximation may not be valid at the divide. Gillet-Chaulet et al. (2013) and Kingslake et al. (2014) and probably others have measured higher magnitude vertical strain rates near the surface on ice divides and could be cited here to support the point.

Gillet- Chaulet, F., Hindmarsh, R.C., Corr, H.F., King, E.C. and Jenkins, A., 2011. In- situ quantification of ice rheology and direct measurement of the Raymond Effect at Summit, Greenland using a phase-sensitive radar. *Geophysical Research Letters*, 38(24).

Kingslake, J., Hindmarsh, R.C., Aðalgeirsdóttir, G., Conway, H., Corr, H.F., Gillet- Chaulet, F., Martin, C., King, E.C., Mulvaney, R. and Pritchard, H.D., 2014. Full- depth englacial vertical ice sheet velocities measured using phase- sensitive radar. *Journal of Geophysical Research: Earth Surface*, 119(12), pp.2604-2618.

Eqn 2: This equation appears incorrect. The z on the top of the fraction in the integrand on the right side shouldn't be there.

L30: I do not see why using a constant density (when in fact the density varies with space) would make the problem ill-posed. It may make the MB inaccurate, but why ill-posed?

L32: Is there a word missing from near the start of this sentence?

Page 7

L19-20: There is a comparison made here between SMB derived with two different methods. In my specific comments above I suggest that the second of these methods be removed from the manuscript, but here I would suggest that if both methods must be included that the comparison is made clearer here. As figure 4 stands, we have to compare spatial patterns in the two fields, but much of the structure in these fields appears to depend on the details of the interpolation, i.e. how the surface is interpolated across the large gaps between data points. The statement here that the two methods 'give nearly identical spatial patterns in SMB' doesn't appear to be correct, but the differences that stand out in figure 4 are due to the interpolations. So, I suggest that you include a plot that compares the two datasets without including an

interpolated field. For example, a profile along the line 2-2' of the SMB estimates, or remove the spatial dimension entirely and include a scatter plot the estimates from each method on each axis.

L30-31: There is a missing link here. Is the point that if there were basal melting then the Raymond arches would be smaller? As the arches heights have not been compared to arch heights expected from modelled (Martin et al., 2006) or measured (Kingslake et al., 2016) vertical velocities, I am not sure that their size can be used to support the statement about melting. Note that I think it is fine to assume that the ice rise is cold based if the 1D modelling shows that it is thin enough.

Kingslake, J., Martín, C., Arthern, R.J., Corr, H.F. and King, E.C., 2016. Ice- flow reorganization in West Antarctica 2.5 kyr ago dated using radar- derived englacial flow velocities. *Geophysical Research Letters*, 43(17), pp.9103-9112.

Page 8

The term laminar flow is used at least three times here to distinguish the flow of the ice rise flanks from flow within a region close to the divide where the Raymond effect acts. All glacial ice flow is laminar (as opposed to turbulent), so this seems doesn't seem like the correct term to use. Perhaps a better way to make the distinction is to say that in the flanks the shallow-ice approximation is valid whereas in the divide region it is not. The approximation of γ in line 7 comes from the SIA and Martin et al. (2009) showed that the full-stokes models are required to describe the Raymond effect and discuss how the SIA is incapable of this. So perhaps this works better.

Martin, C., Hindmarsh, R.C. and Navarro, F.J., 2009. On the effects of divide migration, along- ridge flow, and basal sliding on isochrones near an ice divide. *Journal of Geophysical Research: Earth Surface*, 114(F2).

L26-27: Can you expand on the statement that a more accurate determination of γ requires us to know ice-flow history? Is this because ice rheology has a memory through ice temperature and crystal fabric?

Page 9

L1-2: This sentence, starting 'This is because...' is very unclear.

I found section 6.2.3 very difficult to understand. For example, in line 21, is 'this estimate' the estimate of SMB using the flowband setup or the estimate of the variation in flowband width. Also is the second paragraph (which is just one sentence) missing more material? I suggest that the description of the flowband setup is re-written and expanded to make this clearer.

L6: I am confused by this statement that for a given γ the polygon setup gives the largest estimate of mass balance. According to Fig 7, when $\gamma > 0.75$ the flowband setup (dashed line) is higher than the polygon setup (solid line).

L7: Suggest remove 'much'. I am not sure that I would describe 0.05-0.07 m/a as *much* smaller than 0.1 in this context. They are the same order of magnitude.

L8-9: I am not sure that I understand the sentence starting 'Higher γ ...'. Are you saying that the dependence of the mass balance on γ is nearly linear. If so, I suggest you replace 'uniform' with 'linear'.

L11: Delete 'as it goes'.

L12: Rephrase this sentence to avoid the phrase 'varies in a more variable way'.

Page 11

The discussion contains many typos and ungrammatical sentences, that I have not listed in detail.

L15-26: Do these two paragraphs belong in the introduction? They do not discuss any of the new results and I think they can be shortened without losing substance.

Page 12

L20: Another recent relevant reference is:

Kingslake, J., Martín, C., Arthern, R.J., Corr, H.F. and King, E.C., 2016. Ice- flow reorganization in West Antarctica 2.5 kyr ago dated using radar- derived englacial flow velocities. *Geophysical Research Letters*, 43(17), pp.9103-9112.

L26: ‘inferring’ → ‘implying’.

L27: ‘(1000s m)’ is confusing as throughout the manuscript spaces between values and units have not been consistent. Suggest replace with ‘1000’s of meters’.

L29: Delete ‘the’ before ‘Raymond’.

L31: Is a word missing from near the beginning of this sentence?

Page 13

Section 7.5 contains no discussion of your results, only the location of the ice rise. Can any of your findings (for example bed topography) contribute to this discussion of the impact of the ice rise on the adjacent ice shelves? If not, perhaps this belongs in the introduction or the section in which you introduce the ice rise.

L17: Insert ‘a’ between ‘over’ and ‘flat’.

Figures and captions

Figure 4 caption: The second sentence appears to be missing something.

Figure 5 caption: The end of second sentence is ungrammatical.

Figure 7 caption, lines 3: Replace ‘as’ with ‘to’.