Detecting high spatial variability of ice-shelf basal mass balance (Roi Baudouin ice shelf, Antarctica)
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Summary

This paper documents the high-resolution basal mass balance for the Roi Baudouin ice shelf for 2013-2014 using TanDEM-X DEMs. The authors provide a good description of the Lagrangian basal mass balance derivation (building on previous literature for analyses of other ice shelves), and compare DEM-derived basal mass balance with in situ pRES data and GPS-derived basal mass balance. In general, the methodology, results, and figures are well done. There are several issues that require further attention before publication. The authors claim to use a novel method to improve quality of velocity divergence products. The resulting products still contain fairly significant noise and the authors provide only one comparison with a coarse smoothing approach which generally looks similar. There are some significant discrepancies between firn air content from a dynamic firn densification model (RACMO-FDM) and the in situ radar data, with different assessments of significance. The authors offer a detailed analysis of an ice-shelf surface depression, which they interpret as a moving, unfrozen englacial lake 30 m below the surface. There is limited evidence to support this speculative interpretation. The LBMB derived from pRES/GPS measurements shows some significant differences when compared to the DEM-derived LBMB. On the whole, this is a good paper that should be published after these and other issues are addressed.

General comments

The abstract ends with a mention of challenges for full coupling between ice and ocean models, but this is never addressed in the paper.

Methods section needs some reorganization. Recommend separating DEM generation from hydrostatic ice thickness calculation.

No meaningful analysis of TDM DEM accuracy is provided, only “estimated relative vertical accuracy better than 1 m (based on the standard deviation in overlapping areas)” – where are these areas? Are they all flat, or does this include higher surface slopes (like the surface depressions over channels discussed in the text)? Are you certain these areas are not evolving during the 2013-2014 period due to SMB or dynamics? A much more convincing approach would be to show a figure of DEM standard deviation over static surfaces (exposed bedrock, slow-moving ice with limited SMB). No dates (month, day) are provided for the TDM DEMs, and we don’t know over what period the “32 from 2013” and “11 from 2014” actually cover. Were mosaics generated for each year, centered on Jan 1 of the respective year? Did you compute Lagrangian Dh/Dt for each pair of DEMs, or for the annual mosaics? What about offsets between DEMs within a single year – if they are months apart, won’t there be significant advection for areas flowing 300 m/yr?

Are you convinced that you are not seeing penetration of the radar into snow and ice in the TandDEM-X DEMs? Other studies have shown this can be several meters, potentially up to 10 meters in cold, dry snow. This could impact the comparisons with GNSS surface elevations.

The authors indicate that they smoothed their input 10-m posting TanDEM-X DEMs with a gaussian filter (no filter dimensions provided, but 7-sigma implies a large kernel). This inherently reduces the resolution of these DEMs, and will smooth edges of small-scale features like channels.

It is important to be clear that the DEMs are measuring surface elevation and the surface expression of features interpreted as basal channels. You are not directly measuring channel depth using surface elevation.

Are there any airborne radar profiles (OIB, BAS, etc.) over this shelf? Seems like there must be something available. If so, you can use observed ice thickness and deviation from floatation thickness calculated from your surface elevations to estimate firn air content [*Holland et al.*, 2011].

The ordering of the figures is a bit odd – the comparison showing divergence for regularization vs. smoothed velocities should be shown after describing the method, or early in the results. The LBMB figure (the main result) should come after the component figure

Specific comments (and additional general comments that came up for specific sections)

Page 1:
Line 12: Is the lake 30 m or the surface depression is 30 m below surrounding surfaces?
Lines 16-17:
Line 21: Not sure "Marine Ice Sheet Instability" should be capitalized,

Page 2:
Line 10: delete "the" before "the BMB"
Line 11: "which form"
Line 11-12: I don't understand this 50% claim, also, the sentence ends abruptly – missing "surrounding ice"?
Line 10: What about GPS receivers [*Jenkins et al.*, 2006; *Shean et al.*, 2017]?
Line 14: Channel carving does not cause increased crevassing – these are separate processes
Line 16: delete "at 10 m gridding" – not relevant for intro
Line 17 and later: I believe TC should be "Figure" not "Fig."
Line 22: delete "all the way"
Line 23: delete "In the following,"
Line 24: delete "results", also suggest "with focus on…" rather than "special focus"
Line 26-28: "accounts for" is a bit awkward – you are mitigating the noise, not accounting for it
Line 29: change "observational evidence" to "observations"
Line 29: suggest simplifying to "…validated with phase-sensitive radar, ground-penetrating radar, and GNSS observations."

Page 3:
Equation 1: This is 3 equations! Should probably split. You might start with the standard mass conservation rather than rearranging at the start.
Line 6: "vertically integrated" (no hyphen needed) – perhaps "column-average" is a better descriptor here. You are assuming surface velocity is equal to column-average velocity, this should be stated somewhere
Line 6: "In principle, "

Page 4
Line 1: Not sure what you mean here - Eulerian dH/dt involves two thickness measurements
Line 6: "can only be adequately done" is subjective
Line 7: Add [*Shean*, 2016; *Shean et al.*, 2017] for additional derivation and discussion of Eul/Lag elevation change
Line 10: "from 2013 and 2014"
Line 10: Suggest deleting ", clearly resolving ice-shelf channels" – you are claiming that the 10-m resolution is novel (a claim some might reject). This resolution can resolve many small-scale features on ice shelves, not just channels.
Why so many details in paragraph 2? Details like "we calculate Lag thickness change by cross-correlating the TanDEM-X DEMs (using 5x5 km2 patches…) should be in Seciton 2.5.
5x5 km^2 – I think you mean 5 km x 5 km.
OK, so you have 10-m DEMs, and you are cross-correlating using a 500x500 pixel kernel. Using a kernel this large inherently reduces the resolution of the output velocity products. Did you do this for every pixel, or for some sparse interval?
Line 17: "freely floating"
"viscous inflow in ice-shelf channels" is a process, not a "small-scale feature"
Line 24: "of 1996" and "of 2010" should be "from 1996"
Line 24-25: So, you compared the 1996, 2010 and 2014 velocities and found no evidence for temporal variations? What are dates of the input InSAR? What was time period of GNSS? Surely the velocities aren't identical.
Line 25: "This dataset" – ambiguous
Line 27: Replace "cutting edges" with "seams"
"Offsets between the two datasets are over 60 ma-1 in places."
You are blending seams with feathering, but you are not actually correcting the offsets in velocity magnitude. This will lead to smooth gradients for the divergence, but will likely lead to incorrect horizontal path determination for your Dh/Dt. That 60 ma-1 is a significant portion of the ~50-300 ma-1 velocities over the ice shelf. Your Lag Dh/Dt obs could be "off" by 6 pixels at your DEM resolution.
Line 30: "The SMB is based on…" is awkward start. You are using RACMO, not basing your SMB on RACMO.
Line 32: Need a reference for these processes that RACMO is reproducing. Also, RACMO doesn't predict anything. Add reference to Figure 2b when discussing SMB spatial distribution.

Page 6
Seciton 2.4: This section should be split into 1) DEM processing and correction and 2) Hydrostatic thickness calculation.
Switching between "were" and "are" – should all be past tense

Add reference to TanDEM-X mission.
Add reference for SARscape software?
How did you co-register the DEMs? How do we know there aren't horizontal and vertical offsets between your input DEM data that will lead to artificial elevation change (and LBMB) signals?
Looks like a seam artifact is present in Figure 1 between labels 3 and B. LBMB values are positive (~3-5 m/yr) and then immediately adjacent, close to 0.

Line 12: Don't start sentence with acronym
Line 11: Based on my experience with Greenland data, I am skeptical of this 1 m vertical accuracy for TDM. This needs stronger justification.
Line 18: What are dimensions of your Gaussian filter – 7 sigma is large, implying a large kernel, which will significantly reduce the resolving power of the output DEM (definitely not 10-m)
Weren't the DEMs and GNSS data collected during different time periods? Are you assuming that no change occurred between the two collections? What is the "mean and standard deviation" of the differences? The GNSS data are isolated to a small area, how do we know that this is representative of the larger ice shelf?

Line 22: Wording is awkward, suggest something like "We calculate freeboard ice thickness assuming hydrostatic equilibrium…"
Line 25: Firn air content accounts for total air content of the firn, not variable firn density.
What are typical mean dynamic topography offsets for this location?
Are you accounting for density errors in your uncertainty estimates? I've found that including a +/- 5 kg/m3 uncertainty in density can dominate the freeboard thickness error, much more than a few meters of firn air content error.
I've found that IMAU-FDM estimates over ice shelves in West Antarctica are biased high, in some places 5-10 m too high, compared to radar-derived firn air content using techniques from

Review was interrupted for several weeks at this point. Picking up again. I apologize for discontinuity in comments.

Line 28: What is approximate mean dynamic topography correction for this location?

Page 7
Line 5: I think you mean "these approaches are not well-suited…"
Line 9: What is meant by "wiggliness" of the derivative? Need a little more explanation about what you are solving for and how the process works. This is emphasized as a novel method, so it should be documented clearly. So, you are computing horizontal and vertical gradients separately, then combining in a large inversion?
How does the velocity map resolution impact alpha?
When I look at Figure 2D, I still see plenty of noise in the velocity divergence map.
Line 18: replace "such as" with "including"
OK, so you de-tided the GPS surface elevation data. Did you also detide the TDX DEMs?
What depths were the reflectors used to determine strain thinning? Did you have to account for firn compaction?

Page 8:
Lines 1-2. I don't understand this. You are saying the strain correction is small compared to the basal melt rate, with both provided in units of m/yr. This makes sense. The 10-day interval should be irrelevant here – why is it mentioned?
This result about strain correction suggests that the velocity divergence term (and the regularization) is not necessarily important for the larger shelf LBMB calculation. The
Line 6: "seaward"
In Figure 1, what is the band of large positive (blue) values along the grounding line to the right of Label "1"? Artifacts or real refreezing signal?
My guess is that the Depoorter grounding line is in the wrong place, the ice at this location is grounded, and this area should be masked.
Line 8: "stoss" is a relative term – could be leeward for ocean circulation, different direction for wind, different direction for ice flow – suggest changing to absolute direction ("south")
Line 11: Where are these overlapping areas? Are you sure that the LBMB is not changing over the period for which you are performing the analysis (could some of your observed std be due to real changes in melt rates?) I think you are saying that formal error estimates are larger than the magnitude of the measured signal.

The last sentence on Page 8 and first sentence on Page 9 have no real context. I think you are making an argument that dh/dt from sparse or low-res measurements is problematic. But you have high-res DEMs, so you don't need external datasets for high-res Eulerian elevation change.

Page 9
Line 7: Is this order of magnitude difference present everywhere? Seems like DH/Dt values are close to 0 in the middle of the shelf, so the vdiv and smb terms become much more important.
Line 11: Is this convergence within channels present across the full channel width, or just on the sides, or do you lack the resolution to determine this?

I don't see negative Dh/Dt across all channels in Figure 2f. In fact, I see positive Dh/Dt in several places (e.g., just northwest of the right-most arrow in figure 2d). So, the convergence is causing thickening of the ice shelf at this location? More likely is that you are picking up surface elevation change due to snow redistribution

Page 10
Line 3: At the channel center, LBMB values are positive, potentially even +2-3 m/yr. This is not close to zero. What is going on here? Is that refreezing (unlikely) or is this an artifact? Need to address this if you are going to interpret the signals on the sides with confidence.
Line 4: not km2 here, just km
Line 4: elliptical, not ellipsoidal
Line 5: You don't necessarily know that the lake is connected. It's location is adjacent to the channel, but careful about wording that could be misinterpreted here.
Line 6-7: This interpretation about tributaries needs more support if it is going to be included.

I haven't seen the Lenaerts et al (2017) paper, but I'm puzzled by this interpretation. Are you suggesting that the lake is liquid water, 30 m below the surface of the ice shelf? Why have these not refrozen?

Line 12: "blocks the penetration" suggest "attenuates"

This interpretation is inconsistent with the radar data shown in Figure 6. There are many reflections beneath the "lake" feature. This to me suggests there is no way that this is liquid water. There may be an interface that is attenuating the radar signal, but definitely not salty water, which seems like the only way to prevent refreezing.

Line 15: I disagree – this interpretation is important. I would not call it an englacial lake if you have no direct evidence for this interpretation. Stick with "elliptical surface depression" and be consistent throughout.

I am not entirely convinced that the apparent LBMB over this feature is not due to variable snow accumulation and redistribution over the periods when you have elevation measurements.

I disagree with the interpretation that the pRES and DEM-derived LBMB values agree "well" or show a "near-perfect" fit. Figure 6b shows major disagreement (+/-2-6 m/yr) between the two in places. These offsets are large and significant compared to the magnitude of the LBMB signal.

This paragraph is very long, and should be broken up

Line 19: I don't think "low" is the right term here, try "large negative"

In Fig 6c, I don't understand why the surface is getting lower over the depression, but getting higher between 0.5-2.0 km. So, if I understand correctly, the P-P' profile was extracted in a fixed Eulerian 2016 location? So some/all of the observed elevation change for this fixed profile could be due to advection? Why was not extract the profile in a Lagrangian sense, moving with the feature?

Are you convinced that you are not seeing penetration of the radar into snow and ice in the TandDEM-X DEMs? Other studies have shown this can be several meters, potentially up to 10 meters in cold, dry snow. This could impact the comparisons with GNSS surface elevations.

It looks like the large negative values in LBMB along the channels is mostly coming from the velocity divergence term. Are you confident that these negative values on channel sides are not artifacts velocity resolution and regularization approach are a

Line 25: Again, no evidence for connection. Inactive in what sense? You don't see an elevation change signal, so it is not actively experiencing melting or refreezing.

Line 28: How does an englacial lake creep through an ice column? Is there a reference for this, or is this your interpretation? Still don't understand how this could remain unfrozen.
Lines29-30: Not sure what this sentence contributes. I'm still thinking that much of the observed elevation change within the depression could be due to local surface accumulation and wind redistribution.

Page 13:
Line 5: This is also highly dependent on the density ratio of ice and ocean water, not just the firn air correction.
Line 11: Do you mean overlapping DEMs?
Line 13: "cutting edges" – I think you mean "differences exceeding 13 m/yr across seams"
Line 15: OK, but isn't there a 2-year time difference between the DEM timestamps and the GNSS measurements? Is this an appropriate comparison?

These offsets in the firn air content are significant. What percentage of the total ice thickness is this? In some cases ~5-10%?

Line 29: Also, can have surface and basal crevasses, filled with ice/water/air that will affect the air content. Maybe less relevant at these locations.
Line 34: "To get our LBMB in Lagrangian geometry" – I think you mean "to determine the relative offsets between surface features in the two DEMs, needed to compute Lagrangian Dh/Dt…"
These are length and width in km, Not km^2.
Line 35: Correlating DEMs is not always better, esp for smooth, featureless surfaces or sparse altimetry data.

Careful about comparing magnitude of "SMB" here, which is m w.e. – you are using the expected elevation change output by a firn model driven by SMB, right? Not the same thing.

Page 14
Line 3: Bimodal is not the right word. I think you mean a positive/negative signal due to misalignment.
Line 4: So which did you use again for the paper? I thought you were using existing flow fields, not your cross-correlated flow fields? Make sure this is clear wherever it is discussed in the text.

The differences between the two approaches in Fig 7 are not as drastic as one would expect. Why did you choose 5.125 km window? This is a 41 pixel window for 125 m velocity maps! I would chose something far smaller, like a 3x3 px, 5x5 or even 9x9 px. My guess is that such a filter would remove noise but preserve the same channel-scale divergence as the regularization. It's not really a fair comparison to say that your method is better than smoothing when you only tested one very large smoothing window. I'd also like to see the original data, so we can assess the improvement offered by the different approaches.

Line 15: OK, earlier you made the argument that the velocity divergence term was small, so the error in firn air and hydrostatic thickness was negligible. Now you are making the argument that the velocity divergence term is significant. What is the error from a 10 m error in firn air correction for the entire shelf? Might be an informative map, which could be used to produce a map of uncertainty in LBMB. I think Moholdt had some nice figures like this in his paper.
Line 23-24: You're not really applying an atmospheric model, you are using output elevation change products from a firn densification model (FDM) driven by regional climate model (RACMO) outputs.
Line 24: Could also be overestimation, which would have negative bias.

It’s important to separate SMB from elevation change due to SMB. SMB is in m w.e., while the actual elevation change will depend on density of snow and firn. Fresh snow will have a lower density, and potentially a greater impact on elevation change.
Line 28: What is the approximate length scale of the tidal flexure here?
Line 29-30: Suggest that you state the datasets used – ICESat-1 was used to infer ice thickness.

Page 15
While slope may very well be an important factor here, the shelf draft is significantly deeper near the grounding line, where we would expect a suppressed freezing point and enhanced melting. Like some combination of these two factors.
Line 3: “observed variability” in what? LBMB?
Line 4: Why only surface lowering? Really it’s more general – surface elevation that is not representative of hydrostatic ice thickness.
Line 8: “This indicates” – the datasets don't’ make the region active
Line 10-11: I still don’t understand how a liquid body can “creep” through the ice column unless it has high salinity.
Line 12: “appears passive” – not sure what you mean by this. I think you mean that the apparent basal melt rates are small.
Line 15: “connected to the grounding line” – the channels appear to originate near the grounding line, but this does not necessarily imply a direct connection. Some of the channels can also be inherited from bed topography at the grounding line, which leads to feedbacks in basal melt magnitude/distribution.
Line 17: Be sure to specify that you are talking about subglacial meltwater originating upstream beneath grounded ice.

Page 16:
Line 1: Cite Shean (2016)
[*Dutrieux et al.*, 2013] noted that melting appeared to be focused in channels near the grounding line and on keels near the outer shelf. The full-shelf thickness gradient at PIG is substantial, 1-1.5 km near the grounding line, and 300-500 m near the calving front.
I believe there is also some component of Coriolis that can lead to asymmetric melt within the channels.
Line 11: “sub-kilometer”
Line 14-15: So, over what length scales are pRES point measurements representative?
Line 16: “uncertain in their magnitude” – so you are suggesting that satellite LBMB on its own is not useful? I disagree. I think it’s also a matter of LBMB signal magnitude. At PIG, melt rates exceed 200 m/yr in places, so 5-10 m/yr error is negligible.
Line 18: “we derived”
Line 19: This makes it sound like you ran an atmospheric mode. Replace “atmospheric modelling” with “elevation change output from a dynamic firn model driven by regional climate model output”
Line 20: deepest and steepest.
Line 22: Really, you are not observing large basal melt rates below ice-shelf channels, you are observing high melt rates beneath surface depressions that form over basal channels

Line 25: I'm still not clear on the matching procedure – did you combine your independent velocities and your velocity maps derived from DEM correlation? Restate the actual procedure that offers improved quality here.
Line 27: "…small-scale flow anomalies (e.g., channel margins)"

Tables:
Table 1: Mixing km and degrees in the "gridding" column

Figures:
I might reorder these figures to build to the LBMB map. You could show the components (Fig 2) and the Eulerian map (Fig 3) first, then LBMB map (Fig 1).

Figure 1: Delete "in slight transparency"

Figure 2: Which surface velocities are you showing – the InSAR-derived products, or your velocities from cross-correlation of TDM?
In panel d, what is the ~30 km long linear feature to the east of DIR? Is this an artifact? There doesn't appear to be any channel in the ice thickness map. I'm guessing this is where you used the Rignot et al velocities. Should add something about this in caption.

Figure 3: What time period is shown here (ie what is dt)? Is this eulerian dh/dt from your 2013 to 2014 DEMs? I don't think you mean steady state here – you are showing observations, right? The shelf could still be thinning/thickening in Eulerian frame.

Figure 5: Mention arrows in caption. Although I don't think these features are necessarily worth noting (there is also a similar depression on the other side of the channel). Are the 5-10 m/yr freeze-on signals (near P) in panel B real?

Figure 6: Why is there a large offset between your GNSS surface (~60 m on the y-axis) and the reflection from the surface in the GPR profile (~10 m on the y-axis)? Shouldn't these be in the same place?
When the GPR data were processed, did you use 10 m or 1 m of firn to convert two-way traveltime to depth?
What dates (month, day) from 2013 and 2014 are you showing here? Don't you have many DEMs from each year over this location?

Figure 7: It would be useful to see another column here with the divergence and LBMB from the original velocity data for comparison, so we can see the improvement offered by your regularization.
I don't understand why there is still so much noise in panel d. This suggests that the elevation change measurements from the TanDEM-X products are the source of most of the noise in the final LBMB maps. Eyeballing this figure, if I were to draw a window around the lower quadrant and compute a standard deviation of these values, it would probably be something like 1-1.5 m/yr. Is this consistent with your stated vertical accuracy of <1 m?

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