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Interactive comment on "Detecting high spatial variability of ice-shelf basal mass balance (Roi Baudouin ice shelf, Antarctica)" *by* Sophie Berger et al.

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Current mass losses from the Antarctic ice sheet are dominated by ice-shelf basal melting, but yet we know relatively little about the variability of basal melting and refreezing at the scale of individual ice shelves. This paper uses various high-resolution data sets from remote sensing to derive a detailed map of basal mass balance for the Roi Baudouin ice shelf in East Antarctica. The applied data sets and methods have several novel aspects, and the results are interesting in both a glaciological and oceanographic context.

The paper is well written and easy to follow. The methodology is well described, the

figures are clearly presented, and the discussion is straight to the point. I have a few general issues/questions and some smaller comments/edits as given below.

Lagrangian vs. Eulerian: I think the authors exaggerate about the "necessity of the Lagrangian approach" (P8, L15), at least if they mean it to be generally applicable. I agree that it is by far the best approach with the data sets they have at hand; i.e. two high-resolution DEMs that can probably be more accurately co-registered to each other (Lagrangian) than to an absolute reference system (Eulerian). But if consistent elevation and velocity data were available, there would be nothing in the way of getting reasonable Eulerian results. In fact, the authors fail to show that the Eulerian approach does not work in their case because they do not try to calculate Eulerian thickness changes. As long as the DEMs can be consistently georeferenced, it should not be much extra work to calculate and account for that to obtain real Eulerian BMB in Fig. 3. I do not doubt Lagrangian is better, but it would be nice to see it demonstrated as a comparison to the patterns in Fig. 1.

Ice shelf mass balance: The authors do not provide overall estimates of any mass balance components. That could be because the data sets do not cover the entire ice shelf or because they think that inherent biases are too large to do it confidently. However, I think that even some rough area-averaged estimates would be useful to include, or at the very least you should explain why this was not done and which challenges remain to be able to do it. Would flux gate methods be more reliable for that purpose? Potential biases between the 2013 and 2014 DEMs should be possible to correct quite well with CryoSat-2, whereas changes in firn air content and ocean properties are probably more difficult to assess.

Specific comments and questions in chronological order:

P1, L9: Is this range an estimate of actual min/max BMB or does it also contain impact from measurement noise? I guess that some erroneous values would be even larger.

P1, L11: Can be interpreted as if the radar profiling is an error source. Perhaps more clear to say something like: "...although independent radar profiling show..."

P2, L1: has emerged

P2, L17: ice-sheet promontory

P2, L24: specify "several uncertain quantities" rather than just "large numbers"?

Fig. 1: I got a little bit confused about the letter labelling (a, b, c) and the panel labelling (a, b, etc.) in the later figures that they are connected with. I suggest to rather label the frames with the figure numbers they refer to (4, 5, 7) and then the three regions of interest with letters, like A, B, C.

P4, L1: Euelrian also requires two thickness fields in time to calculate dH/dt. In that sense, I do not see any difference with the Lagrangian approach. It is only the reference frame that is different (fixed or moving). See the general comment about this issue.

P4, L28: Any suitable reference to this technique?

P4, L24: Can the coverage of these three velocity datasets be indicated in Fig. 2c? That would be helpful for interpreting noise and smoothness in the divergence field in 2d.

P6, L13: Depoorter et al. (2013) is a composite grounding line from several other published ones. What is the real source in this case?

P6, L6: Do you use a steady-state firn air content or a time variable one? In any case, changes in firn air content (mainly due to accumulation anomalies) are a major uncertainty for the derived thickness changes because errors get incorrectly magnified by a factor 10 in the freeboard-to-thickness conversion. This should be mentioned.

P4, 13: This level of detail does not really fit here in a general description of the sections. I would rather describe the DEM differencing at the end of section 2.4 with a little bit more detail than here. The method and 5x5 km processing is not completely clear to me.

P6, L28: What about corrections for ocean tides and the inverse barometer effect?

P7, L11: Fig. 7 is a very nice illustration of this improvement, but is not shown until page 15. I think it should be moved forward here (Fig. 3?) since it helps to understand the purpose of the methodology.

P7, L27: Should be mentioned earlier together with the DEM methodology.

Fig. 3: I do not really see the relevance of this figure since we know that Eulerian elevation changes have a very variable pattern due to advocating topography. Assuming steady-state dH/dt is not a valid approach for determining spatial patterns of BMB, only area-averaged BMB. As mentioned earlier, I would rather like to see 2-3 panels with dH/dt from DEM differencing, maybe also u*div(H), and Eulerian BMB accounting for both of those contributions.

P8, L8-9: Label 2 and 3 are switched.

P9, L1: Why does it need to be “prescribed from external datasets”? Why not use your own DEMs?

P9, L3: The term steady-state is confusing here. I would rather highlight that it violates the ability to derive spatial patterns of basal melt.

P10, L2: Nice to be able to see this!

P10, L20: I would say opposite. Surface lowering is caused by negative LBMB.

P13, L34: I agree, it is better to use the same data directly than external sources.

P14, L5: This is a good novel approach. Well done.

P14, L27: I agree, but what about the channels? The effect of incomplete hydrostatic equilibrium across the channels is not discussed much (e.g. in relation to Fig. 4).

P14, L30: Is the gradient more important than the absolute thickness? The latter is not discussed, but is important for the pressure melting-point of the ice.

P15, L17: Reference Drews et al. 2017, Nature Comm.

P16, L14: Good point!

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