Point-by-point Reply to Review Comments

by J.J. Fürst, G. Durand, F. Gillet-Chaulet, L. Tavard, J. Mouginot, N. Gourmelen & O. Gagliardini

First of all we want to thank the two reviewers for the critical and useful comments they gave on the manuscript. All comments are considered and helped to improve the quality of our work. In the following the responses to the reviewers comments are indented and denoted in italic.

REFEREE #1

Review of "Assimilation of Antarctic velocity observations provides evidence for uncharted pinning points" by Fürst et al.

This manuscript details a continent-wide analysis of Antarctic velocity and ice domain geometry observations, assimilated in an ice sheet model, to infer the locations and influence of pinning points that offer some measure of ice shelf buttressing. An inverse control method is applied that simultaneously calculates the spatial distribution of basal friction and ice hardness ("viscosity parameter") everywhere. The Shallow Shelf Approximation (SSA) to the stress balance equations is applied for both grounded and floating ice. The analysis focuses on the spatial pattern of the viscosity parameter for select ice shelves as well as the corresponding mismatch between the velocity observations and the model velocity field resulting from the assimilation. The viscosity field and the velocity mismatch field are both used to determine the possible locations of important pinning points in various ice shelves, the location and geometry of which are not present in the Bedmap2 data set (Fretwell et al., 2013) used to constrain the ice sheet/shelf geometry, nor in the locations of grounded ice determined from published differential InSAR analysis (Rignot et al., 2011). The results of this analysis will be beneficial to future ice sheet modeling investigations that rely on accurate representations of ice shelves and their buttressing influence on the ice sheet, as well as to planning flight lines for airborne campaigns (e.g. Operation IceBridge), since these tiny but apparently important pinning points have been overlooked by such campaigns.

Overall I am enthusiastic about this work. It covers some important topics related to ice shelves, model initialization, and the importance of even very small pinning points around the continent. There is a great deal of detail in the manuscript, which is commendable although it was difficult to follow in many places. A lot of this detail is related to topics other than the identification of uncharted pinning points, the subject of which only seems to sneak in toward the end of a long manuscript. I think this is primarily a matter of manuscript organization and presentation. I think that many of my concerns can be addressed with a careful re-write of the manuscript.

Following the reviewers suggestions we restructured the results section, splitting it along three distinct subjects: (1) First we present now "A-prioir decisions" on the regularisation, the initial guess of the inferred parameters and the way of how to impose floatation. All this concerns technicalities of the inversion. (2) Second, the "General performance" of the inversion is assessed in terms of velocity mismatch on a global and ice-shelf level. And finally, (3) we *inspect "Pinning point locations" known from complementary sources and discern a velocity mismatch pattern that enters the identification of the uncharted pinning points.*

Corrected. Result section restructured.

General comments

1. Detailed attention is paid to the initialization of the geometry of the ice shelves (Table 1 and Section 2.4.1) and a careful sensitivity analysis is carried out using a range of possible assumptions. This is a welcome contribution, as this topic has not seen much (as far as I can tell) open discussion in the literature on modeling ice shelves. Although it is convenient to work with an "equivalent solid ice thickness" for modeling an ice shelf, the actual geometry of ice shelves determined from observations (e.g. Fretwell et al., 2013) indicates, as the authors point out, that the bulk density of the ice is spatially variable and lower than typically assumed when modeling an ice shelf as a "solid ice" body. Although the model results appear somewhat insensitive to the geometric setup of the ice shelf (according to a single metric of velocity mismatch (RMS), which is somewhat reassuring), in my opinion even more attention could be paid to the spatial details of the inferred viscosity that results from different model setups. There are other implications that result from making different assumptions about how to initialize the density, thickness, and lower and upper boundaries of an ice shelf, especially the vertical stress distribution in the ice column. This is important for thinking about (for example) crevasse formation and propagation, which admittedly is beyond the scope of the present analysis but it strikes me that this careful sensitivity analysis could be more broadly beneficial to future ice shelf modeling studies if the results were presented in a bit more general detail.

   The reviewer mainly asks for a spatial analysis of differences between the inferred viscosity field using dynamically more complete flow models. The most common approaches are referred to as higher-order or full-Stokes models, with the latter solving the full complexity of the underlying force balance. Such models indeed allow to resolve vertical variations in the stress regime and would affect the inversion of the viscosity parameter B. Close to locations where shelf-ice is grounded and or shows strong shearing, differences are expected to be most expressed. Yet differences between the flow model variants version become most apparent on spatial scales of a few ice thicknesses (Pattyn et al., 2008). Though we already use high resolution for an Antarctic-wide application, the finest nominal spacing is still twice as large as a typical shelf thickness. For the presented Antarctic setup, the resolution will limit the suggested model intercomparison and it is questionable in how far this could add to our discus-

sion. In addition, there are clear computational limits to the suggested comparison on Antarctic scales. For a comprehensive spatial analysis, we rather suggest a single shelf geometry (type Morlighem et al., 2010) or even a generic setup with appropriate vertical and horizontal resolutions. Altogether, the requested model comparison is clearly beyond the scope of this manuscript. This is even in line with another comment of this reviewer stating that the manuscript already contains a 'great deal of details'.

In response to the reviewers comment, the authors decided to expand on the discussion of the viscosity parameter B whenever they discern different model setups.

***Emphasised viscosity analysis throughout the manuscript.** Reviewer request on 3-D stress analysis exceeds the scope of this large-scale model application.*

2. It would be helpful to show the equations you used to calculate the viscosity parameter for different initializations. You describe the hydrostatically balanced (HB) and damage-corrected (DC), but it's not clear exactly what you mean. An equation for each would leave no room for misunderstanding.

   Indeed we deliberately avoided to give the equation as it involved the introduction of many variables and even additionally equations for clarity. Following the reviewer's advice, we decided to directly refer to two equations in Gudmundsson (2013), which give the means to the interested reader to repeat our calculations.

   ***Added extra phrase with reference.** Theory is retrievable in Gudmundsson (2013). Added following sentence:*
   'Apart from a different choice on the direction (namely perpendicular to the grounding line), Eqs. (6) and (8) in Gudmundsson (2013) determine the two required pressure terms while B becomes accessible assuming a model-specific constitutive equation (as Eq. 20).'

3. On a related note, are you starting from a full 3-D temperature field for the ice sheet? Or are you specifying B from a depth-integrated temperature? It would be helpful to have a bit more information about this procedure, as a 3-D B field which is then depth-integrated would be different than a B field specified from a temperature field which is first depth-integrated. How do your starting temperatures compare to the temperature values/fields that other ice shelf studies have used?

   As specified in the manuscript, we started from 3-D temperature information from Van Liefferinge and Pattyn (2013). This field covers the *grounded part of the Antarctic ice sheet while ice shelf temperatures are based on the original methodology presented in Pattyn (2010). The 3D temperature field is first translated into a viscosity and then depth-integrated for usage in the SSA model.*

***Added more details** on underlying temperature fields as follows:*

'The initial field for the ice viscosity parameter B is calculated from an ice temperature reconstruction (Van Liefferinge and Pattyn, 2013). Ice shelf temperatures are inferred from assuming a local balance of surface accumulation and basal melt, as described in Pattyn (2010). The 3D-temperature information is translated into an ice viscosity, using a standard Arrhenius relation (Paterson, 1994). Viscosity values are thereafter vertically averaged.'

4. I don't understand the description of how you use the velocity observations. You say that "velocity components are not interpolated on the model grid but directly used...." This would make sense to me if you were using a regular model grid with vertices that correspond with the velocity observation grid. What does this mean for your irregular mesh though? I'm missing something here.

   The fact that velocity differences are calculated at the location of the observations is very elegant. It is made possible by the finite element method which inherently gives the modelled velocity solution everywhere, though we only save the information on the grid nodes. Therefore independent of the mesh, we can directly compare observations whereever they are taken on the domain without interpolation on the model grid.

   ***Reformulated passage** to clarify as follows:*
   'The velocity observations are not interpolated onto the model grid, because the underlying finite element approach intrinsically allows to compute the velocity solution at any location. During the minimisation of the cost function, differences between modelled and observed flow speeds are calculated at the data locations in the velocity mosaic.'

5. Maybe I don't understand your description, but if you select the T-geometry (using Bedmap2 thickness and enforcing flotation to specify the lower and upper surfaces), are you then using the calculated average ice density from the Bedmap2 geometry, or enforcing a solid ice density everywhere? (e.g. in p. 1473, line 22, what is the "model ice density"?)

   The reviewer refers here to the 'Results' section, where the description stay condensed Details on how to impose flotation are however given before in Sect. 2.4.1. We want to cite a passage there:
   'If one accepts prior changes, three geometry fields of the Bedmap2 product could be

re-adjusted, i.e. the upper and lower ice shelf surfaces and the ice thickness. Prescribing one of them, the other two follow from the standard model densities for sea water and ice, respectively at 1028 and 917 kg m^{-3}. The three options are thus the prescription of either the thickness ($\mathbb{T}$), [...]'

This description seems very clear to the authors, even specifying the constant value for ice density in the model. Yet in response to the reviewer, we reformulated two passages.

***Minor reformulations** in Sect. 2.4.1. and 3.1.3 (old Sect. 3.2.).*

6. Other ice shelf modeling studies adjust the ice shelf geometry by lowering the ice surface according to the (estimated or modeled) firn air content before adjusting for hydrostatic equilibrium and using this equivalent ice thickness in the model (e.g. Khazendar et al., 2009, 2011; Borstad et al., 2012). How would this technique fit into your sensitivity analysis for the geometry initialization? It might be worth at least mentioning this technique for completeness.

   We are grateful for the detailed information the reviewer gives on how other models deal with ice shelf flotation before similar inversion. The approach pursued in the given publications is a physically-based adjustment of our $\mathbb{T}$ option. The thickness is a-priori adjusted, on the basis of assumed firn thickness and densities, before adjusting the upper and lower shelf surface. For typical shelf geometries, their approach would give an a-priori thickness reduction of 5m, lowering the upper surface by half a meter. Therefore, the sensitivity of inferred viscosities to this approach is covered by the $\mathbb{L}$ flotation option. Though the suggested new approach is limited by the sparse information available for shelf-firn densities and thickness, we refer to it now in the manuscript.

   ***Added reference to this approach** as follows:*
   'For completeness, we want to present another more physically based approach to guarantee flotation, not pursued here. It hinges on the sparse information on the firn density and thickness distribution over ice shelves (Khazendar et al., 2009, 2011). In this approach, the ice thickness is a-priori reduced based on the firn properties before putting the geometry afloat (similar to $\mathbb{T}$).'

7. You use the RMS velocity misfit as your metric for selecting which geometry initialization is "best" in your sensitivity analysis. I would be more convinced by some kind of physical rationale for which approach to take. My comments above about the resulting vertical stress distribution in the ice is just one potential consideration, though there may be others. More importantly perhaps is the fact that the RMS will depend on the choice of

regularization, a choice that is not "objective." The differences in the RMS misfit aren't that large anyway, so I'm not convinced that this is the best way to choose a "best" approach.

We agree that our RMS-based choice might depend on the regularisation parameters. In the results Sect. 3.1.3 (old 3.3.), it was already stated that three geometry choices result in very similar RMS values. Our final choice took therefore into account that such an inversion is used as an initial state for prognostic runs. In this light, $\mathbb{U}$ and $\mathbb{D}$ entail further complications (Sect. 3.1.3.), from an a-priori perspective. We also expand now on the discussing viscosity differences on Larsen C between different options. However, also this discussion could not give a final argument for which option is preferential. We are not sure how the analysis of the vertical stress distribution can help in this decision, not to mention that a comprehensive analysis lies beyond this large-scale application (see answer above). It is unclear to the authors which other posterior rationale could be appropriate. Anything we can think of boils down to a derivative of the thickness and velocity observations (for instance grounding line flux, strain rates, ...). The former are an input and the latter are already quantified in RMS form on individual shelves and the entire domain. At least, we understand the concern on quantifying our choice as the "best" approach and we removed this label.

Reformulation Sect. 3.1.3 *to avoid labelling our geometry choice the "best" approach. An example passage reads now:*
'In principle, the $\mathbb{D}$-, $\mathbb{U}$- and $\mathbb{T}$-options give very similar RMS results. Though the $\mathbb{D}$-option performs best in terms of RMS deviation, it is not pursued here in view of a model initialisation. Forward modelling is faced with advecting this density field while the underlying flow model comprises no processes that would cause a spatial variability in the first place. For this study, we use the $\mathbb{T}$-geometry adjustment as this is the quantity which was originally inferred on a physical basis for the Bedmap2 product (Griggs and Bamber, 2011; Fretwell et al., 2013). Among the remaining options, $\mathbb{T}$ is preferable in terms of rms deviation (Table 1).'

8. There seems to be a lot of discussion of results in Sections 3.1 and 3.2 before the Figures get discussed in much detail in Section 3.3. Some of this figure/discussion presentation seems a bit out of order.

   All figures appear in order. However, we hope that we could clarify the succession by a more intuitive division of the Results section 3 into three distinct parts. See details in above answer to overall comment of this reviewer.

   Restructured Section 3 *on the results and reformulated passages accordingly.*

9. Section 3.5 needs a better explanation, as I'm kind of confused about what you've done here. I think you mean that you have supplemented the Bedmap2 mask of grounded/floating ice using the dInSAR grounding line data of Rignot et al. (2011), which do show some isolated grounded locations in various ice shelves. Is this the case? If so, you can describe it in just one sentence. The four paragraphs of this section really confused me. I don't understand what PIN1, PIN5 and PIN10 mean from the description. Perhaps a schematic diagram could clarify this?

   We understand the concerns of the reviewer that passages in this section were badly fromulated. The reviewer however succeeded in rephrasing what we have done. To avoid further confusion, we reformulated the entire section aiming at readability and clarity.

   Reformulated Sect. 3.3.2 (old 3.5) accordingly.

10. Is your L-curve analysis for the entire model domain, grounded plus floating ice? Since you're interested primarily in the ice shelves, I would guess that you would find a different "optimal" value of the regularization term for the viscosity parameter if you conducted an L-curve analysis for just the ice shelves (or just an individual ice shelf). This would then give you different RMS misfit values, which would call into question the heavy reliance on this metric for judging how "good" your results are. I caution against relying too heavily on the velocity misfit for judging results; some physical analysis and intuition should also come into play.

    The L-curve analysis is indeed for the entire model domain, including grounded and floating parts. The idea to split the cost function for these two regions sounded reasonable. We pursued this idea to verify if parameter selection for the regularisation changed. Using the same criteria for there selection, the selected parameter combination (λ_B, λ_{β^2}) are confirmed considering the velocity mismatch either only on the grounded part or only on the floating parts. We want to admit that the L-curve analysis is not fully objective and one could certainly argue for choosing another parameter combination. No matter how the choice is justified, it seems that the same parameter combination arises for the grounded and floating parts. The authors want to stress that they understand the concern on relying exclusively on the RMS misfit for judging their results. Certainly, whenever the authors make a clear distinction between different inversion setups (direct Bedmap2 geometry – floating geometry $\mathbb{T}$ or no extra PPs – include extra PPs), they first consult the RMS value *but they also use the viscosity field B to hihglight non-physical biases or artefacts as extra justification (see Sect. 3.2 & 3.3.2).*

***No corrections necessary** as viscosity parameter B is used to interpret and confirm discrepancies seen in the RMS misfit. L-curve analysis robust for floating and grounded parts.*

11. How important is it that you infer both the basal friction and the viscosity parameter simultaneously? Since the basal friction doesn't come into play on the ice shelves, how different would your ice shelf results be if you inverted for just basal friction on the grounded ice sheet? Since you are relying on a method (Arthern et al., 2015, which is in preparation) that is not published, you should probably describe this dual-inversion technique in a bit more detail either way.

    Unfortunately the manuscript from Arthern et al. (2105) is still not officially published. From personal communication with the first author, we are informed that the manuscript received some last minor review comments and is now as good as 'in press' in Journal of Geophysical Research. This publication is a first attempt of a dual inversion on Antarctic scales but with main focus on model initialisation for prognostic runs. Issues arising with simultaneously inversions of two parameters were however described earlier by Arthern and Gudmundsson (2010), as clearly stated in the manuscript and now emphasised. Our approach does not rely on the methodology in Arthern et al. (2015). The reviewer suggestion to invert one parameter on each side of the grounding line is interesting. If we pursued his suggestion, we still needed to prescribe a viscosity parameter B on the grounded side. Whatever choice we make, the shelf-side viscosities would be affected via the regularisation (counting 1^{st} spatial derivative in B). Without regularisation, we performed equivalent experiments prescribing ice velocities on the grounded side from observations and only inferring the B on the shelves. The RMS misfit decreased more rapidly on the shelves (less degrees of freedom) and we found comparable magnitudes and pattern in the inferred viscosity field. This approach was abandoned as regularisation was found to be important to avoid overfitting and as we wanted to create a fully self-consistent model state.

    ***Emphasis is put on Arthern & Gudmundsson (2010)** for details of and issues arising from a dual inversion.*

12. The manuscript and figures go to great length to discuss the inversion results for the viscosity parameter and velocity misfit for numerous ice shelves (Larsen C, Filchner-Ronne, Brunt/Stancomb-Wills) and compare them to the

results from previous studies. As outlined above, much of the discussion hinges around arguing that you produced "better" results by using the RMS velocity misfit metric, which I find questionable. I think that much of this material could be shortened, as it is not really relevant to the later work that is reflected in the title: finding uncharted pinning points. The sensitivity analysis is nice to see in Figure 3 for Larsen C, but otherwise the results for these ice shelves could be shown with a simple and short claim for each that the results are comparable to those from previous studies. Much of the detailed discussion of these results is not so relevant, and shortening it might facilitate readability of the manuscript.

The authors understand the concern of the reviewer on arguing that produced results are 'better' on the sole basis of the RMS (as mentionned above). The manuscript text was thoroughly edited to keep the discussion on the results on a more objective level.

Avoided argumentation for 'better' results.

The comparison of our approach to previous work was indeed intended to be elaborate and we understand the reviewer's concern on readability. In response, we removed the section on Brunt/Stancomb-Wills ice shelf, as it did not add anything new to the discussion. In addition the authors tried to condense the discussion in Sect. 3.2 (old 3.3). The reorganisation of the entire Result Section (see reply to general comment) also contributes to the readability.

Removed passage and figure 5. Condensed discussions. *Reorganisaed section on results.*

13. I was confused for a while about the way you discussed "additional pinning points" which sounded kind of vague to me, and I got confused with the "uncharted pinning points" that came later (e.g. Section 3.5 is "Introduction of missing pinning points" versus Section 3.6 "Identification of uncharted pinning points"). I think I now understand that the "missing" or "additional" pinning points are areas of grounded ice, not a part of Bedmap2, that are indicated in the dInSAR grounding line data of Rignot et al. (2011). I think it would be more descriptive (and appropriate) to label these "pinning points indicated by Rignot et al. (2011)" or something similar.

    The reviewer is right that the chosen adjectives were misleading not consistently used throughout the text. The authors decided to limit the usage to two labels: 'missing' and 'uncharted'. The former labels pinning points missing in Bedmap2, but identified from complementary information (either manual delineation (on LCIS and Thwaites ice tongue) or *direct dInSAR data (Rignot et al., 2011a)). Uncharted pinning now only refers to locations where we had no direct, referable evidence for basal friction. The expression 'additional pinning points' is completely abandoned and replaced by 'complementary information on grounding line locations'.*

***Adopted consistent vocabulary** t.*

Line-by-line comments and technical corrections

- p 1462, line 8: "reduced to" implies a reduction from some baseline. So the RMS is reduced from what?

  ***Reformulated sentence** as follows:*

  'After the assimilation, the root-mean-square deviation between modelled and observed surface velocities attains $7.8 m\,a^{-1}$ for the entire domain, with a slightly higher value of $14.0 m\,a^{-1}$ for the ice shelves.'

- p 1463, line 5: the vast majority are confined and exert control

  ***Corrected** as suggested.*

- p 1464, line 7: attention has to be paid

  ***Corrected** as suggested.*

- p 1464, line 9: for granted

  ***Corrected** as suggested.*

- p 1464, line 21: not clear what you mean by a "nonlocal mismatch" here

  ***Removed sentence** as it was not necessary.*

- p 1466, line 1: unclear what you mean by "alignment of the local flow into the dynamic state of the surrounding ice"

***Reformulated** as follows:*

'In this approximation, gravitational driving is balanced by basal friction and by an overall adjustment of the stress regime, which is communicated by gradients in membrane stressesHindmarsh (2006). Basal friction is considered negligible for floating ice shelves.'

- p 1466, line 6: the constitutive equation links deviatoric stress to strain-rate, whereas deformation is most commonly associated with strain.

  ***Corrected** as suggested.*

- p 1466, line 25: why the lower-bound target of 1.4 km? This seems rather arbitrary. Is there some specific reasoning behind this number?

  Computational limitations impose this limit.

  ***Added information**.*

- p 1467, line 13: why 950 iterations? is this based on experience? do you have any other metrics to confirm convergence?

  We now increased the number of iterations to 950. The decision is made on the basis of the RMS decrease and that observational precision is reached. Any further convergence will only entail over-fitting of an observational record.

  ***Specify reasoning** as follows:*

  'Then, the cost function decrease saturates and the overall RMS mismatch compares to the error in the velocity observations. Any further convergence is considered to entail over-fitting.'

- p 1469, line 7: unclear what you mean by "changes are most expressed"

  ***Reformulated phrase** to clarify.*

- p 1472, line 13: not clear what you mean by "revert this initial bias." Revert to what? What is the initial bias?

  ***Reformulated** as follows:*

'Locally, where the DC and the HB option prescribe a very low initial B value, the inferred viscosity field remains low after the regularised inversion.'

- p 1473, line 25: I believe this should be the "Gipps" Ice Rise (also in Figure 3).

  Thank you. ***Corrected.***

- p 1474, line 6: volume may be preserved, but not mass if you are using the wrong ice density. Shouldn't mass be the more appropriate metric to be conserved here?

  The Bedmap2 data set only presents ice geometries and does not contain a suggestion for average ice densities. Deliberately there is no statement made on ice mass.

  No correction necessary.

- p 1474, lines 7-8: I agree that advecting a variable density field presents a numerical challenge, but shouldn't the modeling community address (or at least discuss) this challenge if products like Bedmap2 indicate that the actual density of ice shelves is quite variable spatially?

  Advecting ice densities is not so much of an issue. What really bothers the authors is that inferring ice densities from flotation reveals such strong variability (even beyond physical bounds). This issue should certainly be discussed in the community when assessing a following geometry data set.

  No correction necessary.

- p 1475, line 11: the mismatch of 50 m a^{-1} in Larour et al. (2005) was not reported as an RMS misfit, rather an "average misfit," but given the fact that the misfit values in this paper were reported with one digit of precision and with plus/minus values, this may have actually been a qualitative assessment of misfit from the figure. For this reason you cannot really use your RMS alone to say that your results are "better" than the previous study. Larour et al. (2005) used the original Bedmap data, and this alone might explain the difference.

We agree with the reviewer that Larour et al. (2005) reported on an average misfit an, in our manuscript, we never implied to interpret their values as an RMS deviation. Yet this qualitative assessment makes inhibits a direct comparison to this study, which we consider a pity. On top of that, input data sets are different as we already highlighted. We therefore decided to even less evaluate the approach in Larour et al. (2005).

Reduced comparison to Larour et al. (2005) *as suggested.*

- p 1476, line 11: a bit of a picky point here, but something that seems to be often overlooked. You compare your "average" viscosity for Brunt/Stancomb-Wills to a previously-reported value, but the average is only meaningful as a measure of central tendency if the distribution (e.g. histogram) of your viscosity values is symmetric. There is no physical reason (that I can see) to believe that the spatial viscosity of an ice shelf should necessarily be symmetrically distributed, in which case the median viscosity might be a better measure of central tendency to compare against other studies.

  Section on BSW ice shelf was removed *and with it this comparison.*

- p 1476, lines 20-21: the "shelf-wide velocity mismatch of 50 m a^{-1} that you reference here from Larour et al. (2014) is a qualitative assessment of the misfit from the figure, not an RMS that you can compare against to claim that your results are better.

  The shortcoming identified by the reviewer is associated with referenced studies which only provide a qualitative assessment which can not be objectively compared. Therefore, our comparison stays weak. As a consequence, our comparison was already rather 'qualitative. During the revision, we removed further strong statements from these comparison.

  Reformulated comparison. *In particular, the passage on BSW ice shelf was removed.*

- p 1478, line 28: should this be "Shackleton" ice shelf?

  Thank you. ***Corrected.***

- p 1483, line 13: you have not made any statistical comparison of your results against other studies, so you cannot claim that your RMS is "significantly" lower. This is especially the case if other studies did not report the exact same metric for comparison, i.e. the RMS misfit (similar comments above).

> ***Removed this quantitative comparison*** *in the summary.*

- p 1483, line 23: can you quantify how "much softer" the shelf is is when using different ice shelf geometry assumptions? This might be a useful contribution, related to comments above.

> *The sentence addresses the difference in ice viscosities without and with imposing flotation on the Bedmap2 geometry. A quantification of the resultant viscosity difference is academic as ice modellers normally want to have that the shelf geometry is afloat in the model. We are not convinced that thia quantification would be of interest, let alone similar for each individual ice shelf.*
>
> ***Reformulated sentence*** *for clarity as follows:*
>
> *'In our case, putting the ice shelves afloat involves a general lowering of the upper surface and, after inversion, less viscous shelf ice, as compared to the unadjusted Bedmap2 product.'*

- p 1484, line 28: airborne radar could also indicate the geometry of these pinning points, not only in-situ measurements.

> *Bad choice of wording caused misunderstanding.*
>
> ***Reformulated sentence*** *as follows:*
>
> *'If the bed contact was confirmed, only direct measurements, either in-situ or airborne, could answer to what extent these pinning points pierce the ice body.'*

- p 1486, line 9: I don't recall seeing the acronym ASS defined, did I miss it?

> *You missed it in Sect. 3.2 (old 3.3).* ***No correction necessary.***

- Table 1, caption: I don't understand the statement "Avoiding redundancy, complementary information on pinning points is excluded on the basis of how far they are away from grounded ice in BEDMAP2"

> ***Rephrased passage*** *for clarity as follows:*
>
> *'Complementary information on pinning points (Sect. 2.4.3) is accounted for in the inversion dependent on their distance from the Bedmap2 grounding line. Data is included*

if the distance is larger than either 1, 5 or 10 km, referred to as setup PIN1, PIN5 PIN10, respectively.'

- Table 1: the sensitivity of the model results to different geometry initializations is an interesting and valuable contribution here. However, I wonder how these results would compare to the sensitivity for different initial (assumed) ice temperature distributions. In other words, take your temperature guess according to case "TB" and vary it, then see how different your results are. The results of Borstad et al. (2012) and Borstad et al. (2013) seem to indicate that uncertainty in temperature might lead to just as much variation in inversion results (though admittedly in these studies the temperature was varied ultimately to determine damage, though the inversion results were sensitive to the different initializations). If you're using a temperature field from a model, how well constrained is this field? It might be worth at least commenting on this.

  The temperature field is an input variable to compute the first guess for the viscosity parameter B in the TB initialisation. This field is very different from the hydrostatically balanced (HB) first guess, for which shelf-ice can be strongly viscous. Despite this initial difference, a similar B field is found after the optimisation and the velocity RMS values are comparable. As the HB initialisation could be interpreted as a large temperature perturbation, both for the magnitude and the pattern, we conclude that the intialisation would be robust under uncertainties in the underlying temperature field. Upstream and near the grounding line, temperature uncertainties are several degrees Celsius (Van Liefferinge and Pattyn, 2013), which implies small B variations of a few ten per cents in the initial field.

  ***No additional initialisation pursued** as temperature uncertainties are covered by other initialisation strategies for B (see Sect. 2.3). Authors provide now a uncertainty estimate for ice temperatures in Sect. 2.3 as follows:*

  'Underlying temperatures come however with a certain uncertainty of several degrees Celsius (van den Broeke, 2008; Pattyn, 2010; Van Liefferinge and Pattyn, 2013).'

- For all figures that show the velocity mismatch, is this $V_{model} - V_{observation}$ or vice-versa? This should be specified.

  ***Clarified velocity difference** by reformulating caption to Fig.1:*

  'Difference of ice velocity magnitudes (simulated minus observed) [...]'

- Figure 1: too small make out the pink squares at the print size of the figure. Is it necessary to show the whole continent, since you're only really interested in ice shelves?

  This figure is central, as it gives a complete overview of the continent (used to locate the other figures) and gives an overall impression of the performance of the underlying inversion. A similar map is for now unavailable and therefore of interest to a large community to promote similar assimilation techniques. The authors therefore refrain from removing this figure.

  Figure not removed *but increased the size of the pink rectangles.*

- The panels in Figure 3 are small, and it is difficult to make out detail (dashed black and white lines are indistinguishable, I do not see any pink squares as described in the caption, nor a black dashed line for the 100 m a^{-1} isoline).

  The reviewer is right that these streamlines and the pink rectangles are difficult to discern. We therefore increased their sizes such that they become more visible in a printout.

  Adjusted figures *as requested.*

- Figures 6 and 7: the ice shelves themselves are too small to really resolve in these figures. I do not think it is necessary to show so much grounded ice and open ocean by drawing a giant rectangle that includes the whole region. Can you not zoom in and just show each individual ice shelf? This is where the interesting results are, yet I cannot see them.

  As both reviewers had difficulties to see details on the ice shelves for Figure 7, we now present zoomed in versions around the respective pinning points. Their respective positions are indicated in Figure 1. For Figure 6, the authors decided to remove most of the grounded area which made the velocity mismatch information more visible. In addition, streamlines and pink rectangles were increased in sizes.

  Both figures adjusted *as requested.*

REFEREE #2

Interactive comment on Assimilation of Antarctic velocity observations provides evidence for uncharted pinning points by J. J. Furst et al.

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This study is about finding pinning points on Antarctic ice shelves from a modelling inversion of ice geometry and speed. The authors use a shallow shelf approximation version of Elmer/Ice to simultaneously invert for basal friction and ice viscosity. The concept is novel and interesting. The manuscript is well written and provides informations in great details. This study is an interesting modelling exercice highlighting the need for a better sub-ice shelf bathymetry in order to accurately model ice shelf flow. I am not sure however that this methodology is the most effective way of mapping pinning points in Antarctica as measurements from satellite altimetry (ICESat, CryoSat), interferometry (InSAR) or imagery (Landsat, RADARSAT) would be more straightforward and comprehensive. My comments are directed towards the datasets part of the study. I believe this study would make a nice contribution to the ice shelves modelling community after addressing a few minor issues.

1. General comments

P1468, l 22. This point is about Section 2.4.1 Ice sheet geometry: - It should be stated more clearly why the authors take a multiple approach for assuring floatation. I believe it is because Bedmap2 (Fretwell et al., 2013) ice thickness can be in contradiction with its own mask around the grounding line. - For ice shelves, inverting the thickness from the surface or the basal topography of Bedmap2 does not make sense. Indeed, Bedmap2 ice shelf thickness and basal elevation both stem form an elevation inversion taking into account firn air content and geoid corrections (Griggs and Bamber 2011). - There will however be a positive bias in elevation around the grounding line as Bedmap2 elevation 5 km around the grounding line is an interpolation of two different products, the ice sheet DEM, and the ice shelf DEM (Fretwell et al., 2012). See Griggs and Bamber (2011) to understand the positive bias. The grounding line position of the Bedmap2 mask is a potential source of error here.

The reviewer rightly points out issues in the underlying geometric data set. Griggs and Bamber (2011) describe a strong positive bias near the grounding *line where the applied flotation criterion is likely violated. For continuity reasons with the grounded geometry, the Bedmap2 shelf thickness near the grounding line is an interpolation product (certainly within the first 5 km) (Fretwell et al., 2013). For the ice shelf geometry, additional uncertainty arises from the sparse information on firn density and thickness. Therefore, authors agree with the reviewer that ice thicknesses near the grounding line in Bedmap2 have to be taken with a pinch of salt. In our manuscript we show that the data assimilation is rather insensitive to different shelf geometries (prescribing upper and lower shelf surfaces, ice thickness or ice densities). In addition, the focus of this manuscript is certainly not on the grounding line but on pinning points near the ice front. Therefore the authors argue that though there are certainly unresolved issues in the input geometry, they have secondary influence on the presented results.*

***Added comment** on Bedmap2 geometry product as discussed here.*

P1470, L2-4. The meaning of this sentence is unclear to me: Therefore, details in this generic density field should not be interpreted in terms of snow/ice transformation.

***Removed sentence** as it contained double information.*

P1470, L4-8. The value of 15 meters is typical for firn-air content on ice shelves. As the authors make no mention of it, I wander if a firn correction has been applied for thickness inversions U or L. This is substantial correction to make for the thickness inversion from elevation as 15 meters of firn air content translates into roughly 150 meters of ice thickness.

As discussed in response to one comment from reviewer #1, no firn correction has been applied for any of the presented options to impose flotation. This is certainly a small shortcoming of the manuscript but we now refer to other inversion studies that did so. For this work, the authors refrain from repeating the computationally expensive inversion with a firn-corrected shelf geometry because of three main reasons. First the observational record on firn densities and layer thicknesses is sparse. Sole source could be a regional climate model, limited by its very coarse resolution over Antarctica. Second, our results show that three different input geometries result in a comparable velocity mismatches. A firn correction approach would be a compromise between the extreme options we suggested to guarantee flotation. Third, we rely on the Bedamp2 product which has known deficiencies around the grounding line which are not removable without going back to the original raw geometry data.

***No firn correction applied** but we refer to relevant data-assimilation literature on this topic.*

P1473, L1. This point is about Section 3.2 Geometry at flotation: Again this discussion seems to indicate that firn air content hasnt been taken into account. Thickness U or L should not be considered, see earlier comment.

> ***See reply to above comment.***

P1473, L7-9. How can case T have thicker ice thicknesses than Bedmap2 thicknesses when there are the same?

> *The reviewer refers here to a passage which actually does not compare ice thicknesses but ice volume below sea-level. Here, the full citation:*
>
> *'In case $\mathbb{T}$, the ice shelf volume beneath sea-level is higher than in the original Bedmap2 geometry, resulting in an increased hydrostatic back pressure, compensated by lowering B.'*
>
> *The ice volume beneath sea-level increases from the direct Bedmap2 geometry to the $\mathbb{T}$ geometry, as the ice-flow model uses a rather high constant ice density which causes a general lowering of the ice surface by 15 m. The reviewer is right that the ice has the same thickness for both cases, but the upper and lower ice surface is at a different position.*
>
> ***No correction necessary.***

P1474, L25-28. It is unclear to me how you use the observed velocity in the optimisation in terms of grid and how this affects the shear margins of channelized flow.

> *A similar comment was posted by reviewer* #1 *and the authors adjusted the passage.*
>
> ***Reformulated passage for clarity*** *as follows:*
>
> *'The velocity observations are not interpolated onto the model grid, because the underlying finite element approach intrinsically allows to compute the velocity solution at any location. During the minimisation of the cost function, differences between modelled and observed flow speeds are calculated at the data locations in the velocity mosaic.'*

P1479, L25-27. I dont understand this statement <Almost half of the newly identified grounded shelf positions are located within 2 km of grounded parts of the ice sheet>. What is the newly identified grounded shelf?

> *This section was entirely rewritten in response to comments from the reviewer* #1. *The authors hope that the passages are clearer now and that the terminology is more consistently used.*
>
> ***Reformulated section 3.3.2 (old 3.5)*** *for clarity.*

P1480, L4. How can a large radius include less points? Also, I am not sure I exactly understand the intended purpose of PIN1, PIN5 and PIN10. It is presumably to deal with multiple grounding lines as provided in Rignot et al. (2011) dataset.

> ***Actions undertaken*** *see above reply.*

P1480, L2-9. The fact that including pinning points does not improve the mismatch might be a sign of over-fitting. Indeed, if the modelled velocities are too much forced to resemble the observed velocities, then there is no reason to have differences between the runs with and without pinning points. Could you elaborate on this?

> *The reviewer is right that over-fitting the velocity field cannot be excluded as a source for no improvement after introducing complementary information on pinning points. Yet it is impossible for the model to reduce ice velocities along a flow line by simple adjustment of the ice viscosity parameter B. This is only becomes possible by allowing an optimisation of the basal friction coefficient.* ***Added over-fitting*** *as a potential source for no improvement.*

P1481, L22. Figure 7 is really too zoomed out. I would zoom in onto individual ice shelves. From this figure, it is very difficult to retrieve anything else that the approximate position of the un-charted pinning points. Figure 8. Location of PPP1-7 should be marked in here so that it is clear where you place the pinning points.

> *As both reviewers had difficulties to see details on the ice shelves for Figure 7, we now present zoomed in versions around the respective pinning points. Their respective positions are indicated in Figure 1. In addition, streamlines and pink rectangles were increased in sizes.*
>
> ***Figure adjusted*** *as requested.*

P1482, L11-12. Jugging from the RAMP images in Figure 8, satellite imagery alone seems to be quite effective at spotting pinning points. I believe altimetry data would as well, see also Table 2.

> *The authors agree with the reviewer that pinning points could directly be inferred from the velocity observations Rignot et al. (2011b) or from surface features seen in RAMP images. This is now expressed by the following passage in the Summary & Conclusions (P1484, ines 14-16):*
>
> *'Though the identification could be done on the sole basis of the velocities observations or even directly from RADARSAT imagery, our approach implicitly quantifies the effect of these pinning points on ice dynamics.'*
>
> ***No action necessary*** *as a similar statement was already included in the manuscript.*

2. Specific comments

P1465, L8. give rise to biases.

***Corrected** as suggested.*

P1473, L25. I am not aware of a Griggs ice rise in LC.

*Thank you. **Corrected.***

P1482, L9. Venable ice shelf

*Thank you. **Corrected** as suggested.*

P1484, L21. ice shelf front

***Corrected** everywhere in the manuscript.*

P1485, L9. Operation IceBridge

Corrected.

3. References

Fretwell, P., et al. (2013), Bedmap2: improved ice bed, surface and thickness datasets for Antarctica, The Cryosphere, 7(1), 375-393.

Griggs, J. A., and J. L. Bamber (2011), Antarctic ice-shelf thickness from satellite radar altimetry, Journal of Glaciology, 57(203), 485-498.

Rignot, E., J. Mouginot, and B. Scheuchl (2011), Antarctic grounding line mapping from differential satellite radar interferometry, Geophys. Res. Lett., 38(10), L10504.

References

Arthern, R., and G. Gudmundsson (2010), Initialization of ice-sheet forecasts viewed as an inverse Robin problem, *Journal of Glaciology*, *56*(197), 527–533, doi:{10.3189/002214310792447699}.

Arthern, R., R. Hindmarsh, and R. Wilams (2015), Inversion for the three dimensional velocity field of the Antarctic ice sheet, *Journal of Geophysical Research*, *??*(??), ???–???, doi:{???}

Fretwell, P., H. D. Pritchard, D. G. Vaughan, J. L. Bamber, N. E. Barrand, R. Bell, C. Bianchi, R. G. Bingham, D. D. Blankenship, G. Casassa, G. Catania, D. Callens, H. Conway, A. J. Cook, H. F. J. Corr, D. Damaske, V. Damm, F. Ferraccioli, R. Forsberg, S. Fujita, Y. Gim, P. Gogineni, J. A. Griggs, R. C. A. Hindmarsh, P. Holmlund, J. W. Holt, R. W. Jacobel, A. Jenkins, W. Jokat, T. Jordan, E. C. King, J. Kohler, W. Krabill, M. Riger-Kusk, K. A. Langley, G. Leitchenkov, C. Leuschen, B. P. Luyendyk, K. Matsuoka, J. Mouginot, F. O. Nitsche, Y. Nogi, O. A. Nost, S. V. Popov, E. Rignot, D. M. Rippin, A. Rivera, J. Roberts, N. Ross, M. J. Siegert, A. M. Smith, D. Steinhage, M. Studinger, B. Sun, B. K. Tinto, B. C. Welch, D. Wilson, D. A. Young, C. Xiangbin, and A. Zirizzotti (2013), Bedmap2: improved ice bed, surface and thickness datasets for antarctica, *The Cryosphere*, *7*(1), 375–393, doi:10.5194/tc-7-375-2013.

Griggs, J., and J. Bamber (2011), Antarctic ice-shelf thickness from satellite radar altimetry, *Journal of Glaciology*, *57*(203), 485–498, doi:{10.3189/002214311796905659}.

Hindmarsh, R. (2006), The role of membrane-like stresses in determining the stability and sensitivity of the Antarctic ice sheets: back pressure and grounding line motion, *Philosophical Transactions of the Royal Society A*, *364*(1844), 1733–1767, doi:10.1098/rsta.2006.1797.

Khazendar, A., E. Rignot, and E. Larour (2009), Roles of marine ice, rheology, and fracture in the flow and stability of the Brunt/Stancomb-Wills Ice Shelf, *Journal of Geophysical Research*, *114*(F4), doi:10.1029/2008JF001124.

Khazendar, A., E. Rignot, and E. Larour (2011), Acceleration and spatial rheology of Larsen C Ice Shelf, Antarctic Peninsula, *Geophysical Research Letters*, *38*(9), L09,502, doi:10.1029/2011GL046775.

Larour, E., E. Rignot, I. Joughin, and D. Aubry (2005), Rheology of the Ronne Ice Shelf, Antarctica, inferred from satellite radar interferometry data using an inverse control method, *Geophysical Research Letters*, *32*(5), doi:{10.1029/2004GL021693}.

Paterson, W. (1994), *The Physics of Glaciers*, Pergamon, Oxford, United Kingdom.

Pattyn, F. (2010), Antarctic subglacial conditions inferred from a hybrid ice sheet/ice stream model, *Earth and Planetary Science Letters*, *295*(3?4), 451–461, doi:http://dx.doi.org/10.1016/j.epsl.2010.04.025.

Rignot, E., J. Mouginot, and B. Scheuchl (2011a), Antarctic grounding line mapping from differential satellite radar interferometry, *Geophysical Research Letters*, *38*(10), doi:10.1029/2011GL047109.

Rignot, E., J. Mouginot, and B. Scheuchl (2011b), Ice Flow of the Antarctic Ice Sheet, *Science*, *333*(6048), 1427–1430, doi:10.1126/science.1208336.

van den Broeke, M. (2008), Depth and Density of the Antarctic Firn Layer, *Arctic, Antarctic, and Alpine Research*, *40*(2), 432?438, doi:10.1657/1523-0430(07-021)[BROEKE]2.0.CO;2.

Van Liefferinge, B., and F. Pattyn (2013), Using ice-flow models to evaluate potential sites of million year-old ice in antarctica, *Climate of the Past*, *9*(5), 2335–2345, doi:10.5194/cp-9-2335-2013.