Referee #2: Thomas Kleiner

First, we would like to thank Thomas Kleiner for his insightful comments on our paper.

General comments

Unfortunately, the authors do not conclude about the most appropriate friction law for the ASE study site. In addition to that, the discussion of the results is based on the numerical flow modelling only. Constraints on the friction law based on observational data are entirely ignored, but should be included to gain the scientific outcome of this study. The ASE is probably the area in Antarctica that is covered by observations the most (e.g. Rippin et al., 2011; Smith et al., 2013; Brisbourne et al., 2017). If discussed with respect to observations, the study could already be a significant contribution "on getting a better understanding of the physical processes at play at the ice/bedrock interface in order to constrain the form of the friction law which needs to be used in models."

The scientific outcome of the present study is to unequivocally show that projections regarding the future dynamical contribution of an ice sheet to SLR are highly sensitive to the chosen friction law, even on timescales as short as 100 years. Using observations to constrain the form of the friction law which is best suited for a given application would surely constitute a huge leap towards producing reliable projections of future SLR, but, as explicitly stated in the introduction, is not the objective in the present study. It is true that several set of observations regarding the conditions beneath part of the ASE - usually limited to Pine Island as in the studies you cited - are available. However, for a given unique set of observations, the inferred basal stress must satisfy the global stress balance, so the solution of the inverse problem leads to the same basal stress whatever the chosen friction law. Constraining the form of the friction law would then require observations at different times with significant differences in basal velocities, basal stresses and water pressure at the ice/bed interface. Unfortunately, these multiple sets of observations are not available or incomplete. In particular, the water pressure at the ice/bed interface is largely unknown and the assumption of perfect hydrological connection to the ocean is too gross for the purpose of constraining the form of the friction law. In addition, although the dependence of basal shear stress to basal roughness is well acknowledged, formulating a mathematical relationship relating parameters regarding the topographical properties of the bed (local roughness, maximal slope of bumps,...) to the basal shear stress is far from being straightforward.
For all these reasons, all the friction laws commonly used in modelling studies are derived from theoretical arguments (e.g Weertman, 1957; Schoof, 2005; Tsai et al., 2015) or from laboratory experiments (e.g Budd et al., 1979; Iverson et al., 1998) but none of them has been validated *in situ*. Since they report the presence of soft sediments beneath Pine Island Glacier, the studies of Smith et al. (2013) and Brisbourne et al. (2017) tend to support, in this region only, the use of a Schoof law, rather than a Weertman or a Budd law, as the former induces a Coulomb friction regime in the vicinity of the GL, which has been shown from laboratory experiments to be the most adapted friction regime to represent the deformation of sediments (e.g. Iverson et al., 1998). Yet, this is in no way a validation of the Schoof law, and it does not provide any information regarding the spatial distribution which should be adopted for the parameters C_S, C_{max} and n.
These aspects have been briefly discussed in Brondex et al. (2017), and a bit more thoroughly in Gillet-Chaulet et al. (2016). Anyway, we have added a small paragraph within the discussion section to state that, although available observations are not sufficient to constrain the form of the friction law which ought to be used, they tend to support the use of a Schoof law rather than the two other laws.

I have overlooked this several times, but the rheology parameters A_0 and Q, given in Table 1, are very uncommon. Especially the pre-exponential factor A_0 for temperatures above $-10°$ C differs by several magnitudes from the commonly used Paterson and Budd (1982) parameterization of the Arrhenius Law Eq. (4). I have checked Elmer-Ice (Gillet-Chaulet et al., 2012), ISSM source code and PISM source code. They all use very similar values as in EISMINT (Payne et al., 2000). With the reported values in this study, the viscosity would be much larger (see Figure 1) and thus, the friction law more important. Given the specific ice rheology in this study, I have strong doubts, whether the results can be transferred to other models. If these values are not just different (wrong?) in the table, I would highly recommend to re-run the experiments with a more common set of parameters.

The rheology parameters used in our study are calibrated from values recommended by Cuffey and Paterson (2010). For $T < -10°$ C, these authors suggest to use $Q_1 = 60$ kj mol^{-1}, which was already the value suggested by Paterson and Budd (1982). For $T > -10°$ C,Cuffey and Paterson (2010) suggest to use $Q_2 = 115$ kj mol^{-1}, whereas Paterson and Budd (1982) recommended $Q_2 = 139$ kj mol^{-1}. In addition, according to Cuffey and

Paterson (2010), the value of the rate factor at $T = -10°$ C should be $A(-10°\text{ C}) = 3.5 \times 10^{-25}$ $\text{Pa}^{-3}\text{s}^{-1}$ rather than $A(-10°\text{ C}) = 4.4 \times 10^{-25}$ $\text{Pa}^{-3}\text{s}^{-1}$ as recommended by Paterson and Budd (1982). These differences in the values of Q_2 and $A(-10°\text{ C})$ explain the difference of several order of magnitudes in the value of the pre-exponential factor $A_0(T > -10°\text{ C})$ between the one we used (Table 1 of the manuscript) and the one suggested by Paterson and Budd (1982). Note however that, as it can be seen on Fig. 1 of your review, the rate factor A used in our study and the one deduced from the values of Paterson and Budd (1982) differ at most by a factor of 2 (i.e. when $T \to 0°$ C), despite the gap in $A_0(T > -10°\text{ C})$. Note also that, although the values of Paterson and Budd (1982) were the ones adopted by most of the authors a few years ago (e.g Payne et al., 2000; Winkelmann et al., 2011; Larour et al., 2012; Gillet-Chaulet et al., 2012), many recent modelling studies, including studies based on ISSM (e.g. Morlighem et al., 2016; Yu et al., 2016; Seroussi et al., 2017) or Elmer/Ice (e.g. Gillet-Chaulet et al., 2016), make use of the new values recommended by Cuffey and Paterson (2010). In addition, in many studies an enhancement factor is included in order to modify the viscosity. In such a case, it makes no sense to compare the rate factors without including the effect of the enhancement factor, which often differs from one study to the other.
For all these reasons, we did not consider running the experiments with other rheology parameters. We have simply added the reference to Cuffey and Paterson (2010).

Specific comments

- P. 1, L. 3: “Amundsen Sea Embayement” → “Amundsen Sea Embayment”
- P. 1, L. 16: “to the oceans” → “to the ocean”
- P. 1, L. 21: “trustworthy” consider “accurate/reliable”
- P. 1, L. 22: “subcentennial timescales” → “sub-centennial timescales”
- P. 1, L. 25: “a long standing problem” → “a long-standing problem”
- P. 2, L. 19: “geometry and velocity field” → “geometry and (the) surface velocity field”
- P. 2, L. 23: “Yet, Adhalgeirsdottir et al. (2014) have shown . . . ”
- P. 2, L. 24: Consider “initial state of the model” instead of “model initial state”

We followed your suggestions for all the points listed above.

- “Our work being based on a schematic perturbation scenario, the results ... of the ASE to SLR.” This sentence appears to be incomplete.

We think that this sentence is actually complete.

- P. 3, L. 1?: “two-dimensionnal” → “two-dimensional”

It has been corrected.

- P. 3, L. 1?: Consider “shelfy-stream approximation (SSA)” instead of or in addition to “shallow shelf approximation (SSA)” here, because of the basal shear stresses. This is widely used in the literature for MacAyeal’s equations (e.g. in Morlighem et al., 2010).

We followed your recommendation.

- Although formal correct I would recommend to rewrite Eq. 1 with $\bar{\eta}$ as the vertically averaged effective viscosity with units Pa s (instead of integrated units: Pa s m). Thus, ...

We followed your recommendation.

- P. 4, L. 1: “... η_0 is the viscosity given by ...” It is very misleading to call η_0 a viscosity, because it is obviously not (see units, e.g. in your Fig. 4). The equations (2) and (3) are correct and also how they are applied is correct, but your η_0 is only a substitution for the temperature dependent contribution to the viscosity, thus

$$XXX = \frac{1}{2}A^{-1/n} = \frac{1}{2}B, \tag{1}$$

where A is the rate factor depending on the temperature relative to the temperature melting point and B is the associated rate factor (Greve and Blatter, 2009, p. 56). I am specifically asking for a better name and symbol for XXX.

It is true that η_0 is not, properly speaking, a viscosity. However, since adjusting this quantity is an indirect way to adjust effective viscosity itself, we think it will be clearer for the reader if we keep the notation as it was in the previous version of the manuscript. Therefore, we have simply replaced η_0 by $\bar{\eta}_0$, as well as $\eta_{0,ref}$ by $\bar{\eta}_{0,ref}$, in order to stress that these quantities are vertical averages. In addition, we have modified the manuscript so that these quantities are no more referred to as "viscosities" in the text.

- P. 4, L. 3: Although A is called "fluidity parameter" already in Brondex et al. (2017) consider to use the commonly used term "rate factor" instead (Gillet-Chaulet et al., 2012; Gagliardini et al., 2013). Consider to use T' instead of T to account for the different meaning. Please state clearly, if you have used the temperature or the pressure corrected temperature from Van Liefferinge and Pattyn (2013).

We have changed "fluidity parameter" for "rate factor" as suggested. We have also changed T for T' and explicited the fact that T' is the temperature relative to the pressure-melting point.

- P. 4, Eqns. (5-7): The "-" signs in front of $\tau_{b,x}$ and $\tau_{b,y}$ appear to be wrong in Eq. (1) with this notation of the different friction laws. Consider to use $\tau_b = ...$ as in Brondex et al. (2017, Eqns. (1-3)).

The "-" signs in Eq. (1) were indeed wrong and have been corrected. We decided to keep the form of Eqs (5-7) as in the previous version of the manuscript, as we want to stress the fact that, in the present study, $\boldsymbol{\tau}_b$ and $\mathbf{u}_b$ are vectors, which are aligned and with opposite directions. In Brondex et al. (2017), the geometry was unidimensional and we could directly write $\tau_b = ...$

- P. 5, L. 6: "where a_s is the meteoric accumulation rate applied to the top surface of the whole domain and a_b ..." Use "surface mass balance" for a_s as on page 6 line 9. I would suggest something like "where a_s is the surface mass balance a_b applied to the top surface of the whole domain ...". It should be stated that basal melt is ignored for the grounded part of the ice and why in another sentence.

We have changed "meteoric accumulation rate" for "surface mass balance". We have also added "Basal melt at the ice/bed interface is neglected."

- P. 5, Eq. (13): "$\bar{\eta}$" → "$H\bar{\eta}$" with $\bar{\eta}$ being the average effective viscosity. See also P. 4, L. 1 above.

This has been changed.

- P. 5, L. 25: Although this can be guessed from the figures, it should be stated that the calving front is not evolving.

This information has been added.

- P. 6, L. 30-33: Why is the Budd law only applied to one of the inferred states?

See the answer to the general comments of Referee #1.

- P. 6, L. 33: "one of the inferred state" → "one of the inferred states"?

This has been corrected.

- P. 7, Table 1: The numbers for the pre-exponential factors A_0 and and activation energies Q for 'warm' and 'cold' ice are very unexpected. See the Major concerns and suggestions section.

See the answer in the previous part.

- P. 7, L. 6: "ice temperature map" I am not sure what this means. The word map suggests something two-dimensional for me, but the temperature is used for the rate factor A and thus η_0 within the integral of Eq. (2). May "three-dimensional temperature field/distribution" fits better. If the temperature is a three-dimensional field, than it is not clear what is shown as map in your figure 4g.

You are right, it is actually a 3D temperature field, which is used to derive a 3D field of A based on Eq. (4), which is then vertically averaged to get $\bar{\eta}_{0,ref}$ based on Eq. (3). It is this vertical average that is shown in Fig. 4g. This has been made clearer in the manuscript.

- P. 7, L. 7: "a reference ~~viscosity~~ field" and rename $\eta_{0,ref}$ as mentioned above (P. 4, L. 1).

This has been done. See the answer above.

- P. 7, L. 6: The temperature field from Van Liefferinge and Pattyn (2013), based on the model of Pattyn (2010) is a very important part for this study. Therefore, the methods used to get this field should be summarised within a few sentences. Which data set is applied here (ensemble mean, one specific ensemble member)?

In the present study we are using a number of datasets which are all equally important to construct our model initial states. All these datasets are correctly referenced and the interrested reader is free to read the corresponding articles if needed, including the paper of Van Liefferinge and Pattyn (2013). The temperature field that we have been using for the present study was actually provided by Van Liefferinge (personal communication), who made a specific run for this purpose as ice shelves were not included in the original work of Van Liefferinge and Pattyn (2013). This is now explicitly stated in the text.

- P. 7, L. 8: "on each node~~s~~ of a regular grid"
- P. 7, L. 18: "wether" → "whether"

These two mistakes have been corrected

- P. 8, L. 2-4: "Indeed, several model states ... adjusting rather the basal shear stress or rather the viscosity."

We have decided to leave this sentence as it was because we want to stress the fact that both the basal shear stress field and the viscosity field are adjusted at the same time, but with various relative weight.

- P. 8, L. 5: "we construct three inferred states - denoted I_{SV} , $I_{R\gamma,100}$ and $I_{R\gamma,1}$ - by means of the control method" At this place the inferred states are introduced by names and the reader needs to continue reading until page 9, line 17 for the explanation of $I_{R\gamma,100}$ and $I_{R\gamma,1}$. This might be unavoidable as a number of equations must be presented first. Nevertheless, I missed the explanation of the subscript 'SV' in I_{SV} until the end of the document.

It is true that the reason why we use the notations $R\gamma, 100$ and $R\gamma, 1$ becomes obvious only from Eq. (19) or even a bit further. However, as you said, we have no choice as a number of equations must be presented first. It is also true that the subscribe SV does not necessarily make sense in english and we have deciced to change it for $R\gamma, \infty$

- P. 9, L. 3-4: Consider to move "respectively" further to the end of the sentence: "... which are related to the linear Weertman law coefficient and the viscosity, respectively, as follows:", but this is personal preference only.

We followed your suggestion.

- P. 9, L. 32: "occurence" → "occurrence"

This has been corrected.

- P. 10, L. 21-22: "except for the Budd law for which the identification has been done only for the case $I_{R\gamma,100}$ " Why?

See the answer to the general comments of Referee #1.

- P. 10, L. 23: "at every grounded node~~s~~ covered with ice"
- P. 10, L. 24: "which are ice free" → "which are ice-free"
- P. 11, L. 5: "which are ice free" → "which are ice-free"
- P. 12, L. 11: "local adjust~~e~~ment of viscosity"
- P. 12, L. 18: "the inversion algorithm~~n~~"
- P. 12, L. 23: "has already been showed" → "has already been shown"

All these points have been corrected.

- P. 12, L. 21-25: “It is also this same mechanism ... Borstad et al., 2012, 2013).” Although damage could play a role, I am not convinced of this argument. I think, the shear margins are just not well enough resolved in the velocity field that has been simulated in the study by Van Liefferinge and Pattyn (2013, 5 km horizontal resolution). Unfortunately, ice flow velocities are not presented in Van Liefferinge and Pattyn (2013) or Pattyn (2010). The basal drag in an ice stream is usually low, thus the lateral drag at the shear margins balances the ice stream’s driving stress. Similar to the condition at an ice sheets base, the drag leads to deformation of ice (strain) and thus strain heating. As the viscosity depends on temperature the viscosity decreases (see e.g. Bondzio et al., 2017). This is supported by your figure 4 panel g, where no viscosity variations across the shear margins of PIG near the GL are visible. The cited literature is only related to ’damage’ in ice shelves (Larsen B and C) and not appropriate for the conditions in the ASE.

The low viscosity bands to which we refer are located on Pine Island and Thwaites ice shelves and not within the grounded part of the ice streams (see Fig. 4h), therefore we think that the cited literature is totally relevant in this case. We agree that lateral drag at the shear margins must balance the driving stress as ice shelves do not experiment any basal drag. Beside strain heating which you are mentioning, it also causes locally high shear stresses leading to opening of fractures which makes ice softer. Indeed, several authors have reported the good correlation between these low viscosity bands and aerial observations of crevasses (e.g Borstad et al., 2013). Another mechanism which also tends to decrease ice viscosity at shear margins is the development of crystalline fabric which induces anisotropy in ice rheology, making ice softer in some stress directions and stiffer in others (Minchew et al., 2018). Saying whether the low viscosity bands are due to strain heating, anisotropy, damage, or to a combination of the three, is difficult. Therefore, we have added to the manuscript the fact that strain heating and anisotropy could also be potential explanations for the presence of these soft bands.

- P. 13, L. 1: “loosing” → “losing”
- P. 13, L. 10: “at every grounded node~~s~~”
- P. 13, L. 19: “whithin” → “within”
- P. 13, L. 31: “the relative differences on in the velocity field”?
- P. 14, L. 1: “the gaussian integration” → “the Gaussian integration”
- P. 15, L. 19: “is primary controlled by” → “is primarily controlled by”
- P. 15, L. 23: “significantly different than the” → “significantly different from the” My preference.

We followed your suggestions for all the points listed above.

- P. 15, L. 23: “$z_f = ...$, constitutes the thickness above flotation.” This is only true for grounded ice. Consider to show the flotation altitude (red line in Fig. 9) only for the grounded part.

We have made this point clearer in the text as well as in the caption of Fig. 9. However, we chose not to change the latter as different “grounded parts” are actually represented in each plot, i.e. two for the Budd law, two for the Schoof law and the inital profile which is common to the two laws.

- P. 16, L. 7: “Dotson ice shelve” → “Dotson Ice Shelf”
- P. 16, L. 8: “viscosiy” → “viscosity”
- P. 16, L. 11: “tens of degrees celsius” → “tens of degrees Celsius”

We have corrected these points.

- P. 16, L. 12: “temperature map” See comment above (P. 7, L. 6).

The word “map” has been replaced by “field”

- P. 17, L. 2: “showing a highest contribution” → “showing the highest contribution”

- P. 17, L. 28: "leading to important retreat of the GL" → "leading to an/the important retreat of the GL"
- P. 17, L. 30: "occurence" → "occurrence"
- P. 17, L. 33: "solid black line in bottom left panel of Fig. 9" → "solid black line in the bottom left panel of Fig. 9"
- P. 18, L. 23: "parameters are uncertains" → "parameters are uncertain"
- P. 18, L. 24: "viscosity is not inferred but ~~simply~~ deduced"

We followed your suggestions for all the points listed above.

- P. 18, L. 24: "ice temperature maps" See above.

The word "maps" has been replaced by "fields"

- P. 18, L. 31: "equals to the value of" or "is equal to the value of"
- P. 19, L. 1-2: Consider to rearrange the sentence (personal preference only). E.g. "This procedure induces significant but very localised discrepancies between the recomputed velocity field and the reference velocity field used for the identification, in particular within ice shelves." or "... particularly within ice shelves."

We followed your suggestions for the two points listed above.

- P. 19, L. 4-16: The authors state very clear at the beginning (P. 2, L. 28), that "... the results presented here should not be considered as actual projections of the future contribution of the ASE to SLR." Consider to choose other terms to replace "projections" within this and other parts of the text.

Although we do not make actual projections of the future contribution of the ASE to SLR in the present study, the latter shows sensivity of mass loss projections to the friction law and initialisation strategy. In this sense, the use of the word "projection" appears correct to us in most of the text. Yet, we agree that this term was unproperly used in one of the sentences of the conclusion section. This has been corrected.

- P. 19, L. 14: "constain" → "constrain"

This has been corrected.

- P. 24, Fig. 4: I think, maps of the basal shear stress $|\tau_b|$ are required in addition to the stress ratios presented in (d,e,f) for the three inferred states. This would allow to compare your inversion with other modelling studies conducted in this area (e.g. Joughin et al., 2009; Morlighem et al., 2010) and observational data. A large portion of your model domain appears white in the panels a-c indicating that observed velocities are not available here. This is not so easy to see in Rignot et al. (2011), but in Mouginot et al. (2014, Fig. 1). Please explain how do you conduct the inversion in these areas. It is not clear, how the features in the panels d-f can be explained, given the extensive data gap in a-c.

It is true that comparing the inferred fields of $|\boldsymbol{\tau}_b|$ to Figs. 5-6 of Joughin et al. (2009), or to Fig. 2 of Morlighem et al. (2010), can be interresting, although colorscales are different. On the other hand, we don't think that adding maps of $|\boldsymbol{\tau}_b|$ to Fig. 4 of the manuscript, which is already a heavy figure containing a lot of information, would be relevant in the context of the paper. Therefore, we have decided to add these maps of $|\boldsymbol{\tau}_b|$ in a supplementary material.

As stated in the text of the manuscript, the cost function J_v quantifying the misfit between modelled and observed velocities is evaluated at observation points. You are right when saying that there is a large region which is not covered by observation points. As a consequence, the solutions obtained in this region for the fields of α and γ stay close to the initial guesses (i.e. α such that $|\boldsymbol{\tau}_b| = |\boldsymbol{\tau}_d|$ and γ such that $\bar{\eta}_0 = \bar{\eta}_{0,ref}$), except that they are smoother because of the regularisation functions $J_{reg,\alpha}$ and $J_{reg,\gamma}$, which are evaluated over the whole domain. However, the lack of information in this region is not critical as the flow of ice is known to be very slow over there. We have represented, in Fig. 1 of the present document, the norm of the driving stress $|\boldsymbol{\tau}_d|$ which is calculated from the gradient of the surface elevation z_s as follows:

$$\tau_d = \rho_i g H \mathrm{grad}(z_s). \tag{2}$$

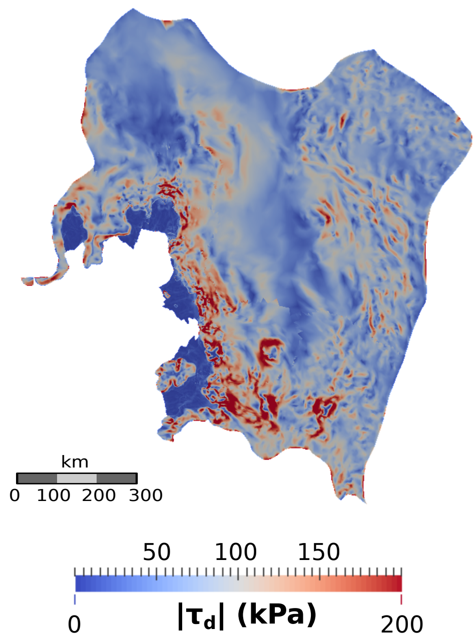

Figure 1: $|\boldsymbol{\tau}_d|$ (kPa) after initialisation.

Comparing this figure to Fig. S1 of the supplementary material shows that the fields of $|\boldsymbol{\tau}_b|$ obtained in the region where surface velocities are missing look like smoothen versions of $|\boldsymbol{\tau}_d|$. Therefore, the "wave-like" features of $|\boldsymbol{\tau}_b|/|\boldsymbol{\tau}_d|$ observed on Fig. 4d-f of the manuscript in this region come from similar features in $|\boldsymbol{\tau}_d|$, which are not present in $|\boldsymbol{\tau}_b|$. These features in $|\boldsymbol{\tau}_d|$ are likely due to the irregularity of $|\text{grad}(z_s)|$ in this region, as it can be seen in Fig. 2 of the present document.

- P. 25, Fig. 5: I can't see any difference between a,b and c. The tiny little areas in between the green and grey areas appear all just red. The zoom in area should be marked in one of the figures for the whole ASE.

The first purpose of Fig. 5a-c is to show that the nodes at which $|\boldsymbol{\tau}_b|$ recalculated with the Schoof law (with $C_{max} = 0.4$) following the identification of C_S differs significantly from the $|\hat{\boldsymbol{\tau}}_b|$ calculated with the linear Weertman law and used for the identification step, are very few (i.e. 8% at most). The second purpose of this figure is to show that these nodes are mostly located close to the ice shelves where ice is almost at floatation, except for some regions located far inland (bottom right corner of Fig. 5a-c) where N is low because of locally very low ice thicknesses, as shown in Fig. 3 of the present document. We think that this two goals are fulfilled with the version of Fig. 5 as it was in the first version of the manuscript. It is true that differences between a,b and c are difficult to distinguish (also because these differences are actually slight), but the reader can still see some small differences between panels (e.g. west part of Thwaites Ice Shelf or upper part of PIG Ice Shelf). We could have focused on a particular region to have a better zoom in, e.g. around Thwaites Ice Shelf, but this would have been to the detriment of other regions of interrest, which would have hampered the first purpose of the figure.
The zoom in area of Figs. 5a-c has been added on Fig. 5d

- P. 27, Fig. 8: The coloured lines should be slightly thicker.

This has been done.

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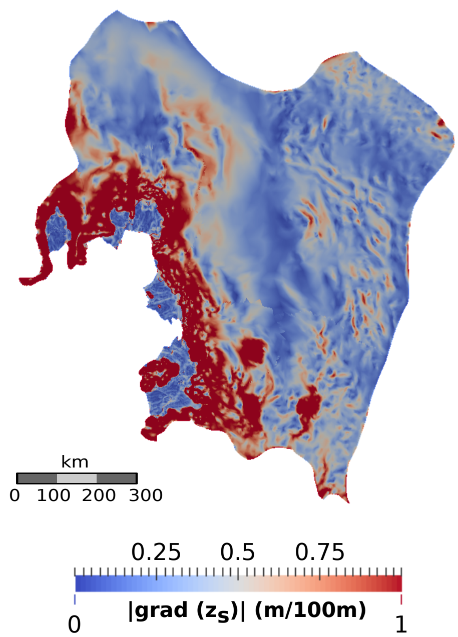

Figure 2: $|\text{grad}(z_s)|$ (m/100m) after initialisation.

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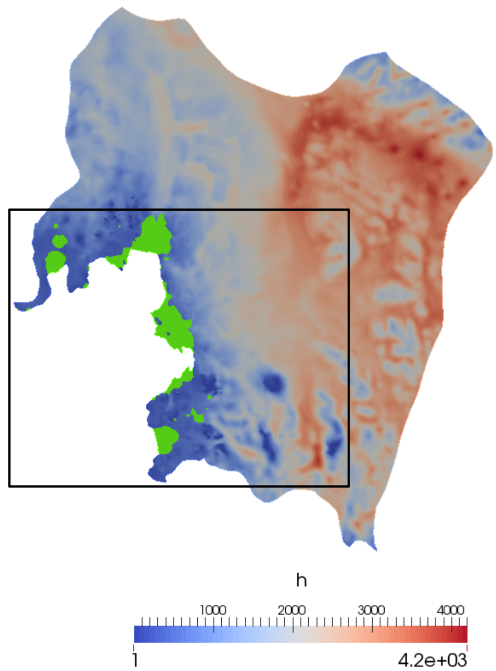

Figure 3: Ice thickness (m) used for initialisation. The black square is the zoom in area of Fig. 5a-c. The ice shelves are in green for consistency with Fig. 5 of the manuscript.

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