Responses to review 2 of the manuscript “Comparison of hybrid schemes for the combination of Shallow Approximations in numerical simulations of the Antarctic Ice Sheet ”:

The manuscript by Bernales et al uses an ice-sheet model for Antarctica to test different schemes for the calculating of the ice velocities. The main focus is on combining the shallow ice and shelfy stream approximations and use different hybrid schemes to combine the two. Four different schemes are implemented in the model and used to inversely determine the basal sliding coefficient C0 by looking at the difference between modelled and observed ice thickness. Differences between the four schemes are explained clearly, through how the spatially varying basal sliding coefficient differ for each scheme, how grounded ice volume evolves and how ice thicknesses vary relative to the observations. Model performance is also checked against observations of ice surface velocities. Methods and results are explained clearly. The two main concerns I have are that the manuscript does miss a real strong final conclusion and that uncertainties in the methodology should be discussed in more detail.

We thank the reviewer for their very constructive comments that helped to strengthen the manuscript. Our replies can be found below.

Main comments

1. Uncertainties in methodology

Paragraph starting on Page 2, Line 30 to Page 3, Line 8:

You should here also mention that there are more parameters and/or processes involved that are poorly constrained (or less known) such as bedrock uplift due to post- glacial rebound (GIA), response of the bed itself to current changes in ice, heat flux from the bed, properties of ice flow (flow parameter) or the bedrock height uncertainty (see Bedmap2 paper). Following on the uncertainties, an important point to make using this method is that by solving for the basal stress you actually compensate/correct for other (some of them mentioned above) uncertainties in the boundary conditions and uncertainties/errors in the model itself. This is I think a key point to keep in mind when using the methods described in the manuscript. This should be mentioned here and discussed thoroughly in the final discussion/conclusion as well!

Following the suggestion of the reviewer, we have added a discussion of potential effects of these poorly constrained processes and boundary conditions on the modeling results. We have also added a separate Data section where the uncertainty in the bedrock topography data is spelled out. The possibility of canceling errors is a clear limitation of the method, since at present there is no easy way to differentiate what could be an artificial compensation of, e.g., an insufficient model representation of physical processes or deficiencies in the input data sets from the actual conditions at the base of the ice sheet. This shortcoming will hopefully be overcome in the future once the necessary observational data sets and more sophisticated ice models become available. For now -and for the purposes of this study-, the risk of error cancellation is over-passed by the dramatic reduction in the misfit between the modeled and observed ice thickness and surface velocities. This limitation is now discussed in the method description and in the final discussion sections.

2. Rewording of basal velocities and SIA

Starting on Page 4, Line 16:

Throughout the manuscript it is mentioned that SIA has a basal velocity. This is mostly a matter of rewording some of the text, not so much an error but rather how this is actually computed. I think the SIA itself does not have a basal velocity, but rather that the basal velocity is a basal boundary condition to calculating the SIA, which could also be set to zero if you do not include sliding at all. This is actually nicely illustrated in Fig. 1 of Bueler and Brown (JGR, 2009). I think this could be rewritten in some parts of the manuscript. In the general comments below some of these lines are mentioned.

We agree. All instances have been rewritten accordingly.

3. Final conclusions

On page 19:

The sentence “This suggests .. Stokes equations.” seems quite obvious but good to mention. However in the final conclusions I do miss a discussion on two things particularly: - Is a distribution of basal parameter C0 derived using present-day forcing and observations also applicable for long-term paleoclimate simulations, can it be used for other time periods and climate regimes?

This is an interesting and challenging question, since the current availability and the accuracy of the “observational” data for past periods is not sufficient for testing the performance of the reconstructed coefficients under paleoclimate scenarios. Basal thermal regimes of continental-scale ice sheets and thus the areas where model calibration procedures can be applied do not only depend on the external thermal forcing that remains nearly constant in time (e.g., geothermal heat flux) but also on climate conditions and ice sheet geometrical settings that had undergone significant changes in the past (Bentley et al., 2014). There are regions where basal sliding is not identified by the method under the present-day conditions, where paleo-ice streams could have developed under a different climate regime, and vice versa. Thus, the derived distribution of C_0 is not necessarily applicable everywhere. However, we believe that it is a good first-order approximation, and is definitely a better guess than commonly used single, homogeneous values of C_0 over the entire domain. The method used to derive C_0 could be potentially extended to the use for other time periods/climate regimes (or even adapted for the transitions between largely dissimilar regimes), providing that the necessary topographic and climate data are available. Looking at the recent efforts to reconstruct the past geometries of the Antarctic Ice Sheet (Mackintosh et al., 2014, Bentley et al., 2014), this may become well possible in the upcoming decades. We have added this point in the final discussion.

- What is the best scheme to be used here? Is there a scheme that simulates the observations best, also considering the lowest over- and underestimations, i.e. the lowest absolute difference in ice thickness.

We have evaluated the performance of each scheme based on both its fit to observations and numerical stability for a range of model setups. We have found that at higher grid resolutions the HS-1 and HS-2b schemes become numerically unstable, due to large gradients in basal sliding coefficients arising from the use of basal velocities as boundary conditions for the SIA solutions in conjunction with the calibration of sliding coefficients. The HS-2a and HS-3, which utilize the SStA as a sliding law, are numerically more stable to variations in model parameters and changes in grid resolution (especially HS-2a, which rarely produces simulation crashes). The drawback of the HS-2a is in its limited ability to influence the fit between the modeled and observed ice thickness in ice sheet sectors where the SStA velocities are low (<100 m/yr), which is the case over large tracts of the Antarctic interior. The HS-3 overcomes this limitation by accounting for the SStA contribution everywhere, but so it does with the SIA velocities, which in certain areas such as, e.g., the steep ice sheet margins are excessively high, as shown in Figure 3 of the original version of the manuscript for the SoS. To improve the performance of both schemes, our future work will reconcile the drawbacks of HS-2a with the advantages of HS-3, providing a very stable and flexible hybrid scheme. We have added this point in the final discussion.

General comments

Page 1
Line 1: Replace introduced with used or implemented

Done.

L 5: What do you mean with realistic scenario? Do you mean like present day climate forcing? Please be specific.

We mean a non-synthetic/simplified ice sheet geometry. We have modified the text to avoid confusion:

Here, we implement four different hybrid schemes into a model of the Antarctic Ice Sheet in order to compare their performance under present-day conditions.

L 8: Remove comma after Despite this

Done.

L 8: Robust agreement with what, or of which variable? Again please be specific.

We have modified the text to clarify this:

[…] we observe a robust agreement in the reconstructed patterns of basal sliding parameters.

L 17: Change to: However, the time scales over which an ice sheet builds up and disintegrates

Done.

Page 2
L 9: Remove commas before membrane and after important

Done.

L 7-14: You could first introduce the SSA (L10-14) and then discuss the ice streams (L7-10).

Done as suggested.

L 15: Instead of mentioning highly dynamic regions, actually state here something like: The limitations of SIA models for calculating the highly dynamics ice streams have ..

We have modified the text as follows:

The limitations of SIA models for reproducing the dynamics of ice streams have prompted the development of [...]

L30: Rephrase realistic scenarios, same as in the abstract, present-day climate forcing?

We have modified the text as suggested:

Despite the above differences among models, all of them are subject to common limitations when applied to the present-day Antarctic Ice Sheet

Page 3
L 17: No new paragraph, start sentence as: First, the ice-sheet model

Done.

Page 4
L 16: Please rewrite, the SIA itself does not have a basal velocity, but rather U-b is here the basal boundary condition to calculating the SIA.

We have modified the text as suggested:

At the base of the grounded ice sectors, bedrock stress conditions and the associated potential for sliding are linked to the basal velocity, u_b, used as a boundary condition for the computation of the SIA velocities, through an empirical Weertman-type sliding

L 18: Eexplain here how the effective basal pressure Nb is calculated.

Done. The description reads:

N_b is the effective basal pressure, computed as N_b = rho_{ice} g H - rho_{sw} g H_{sw}, where rho_{ice} and rho_{sw} are the density of ice and sea water, respectively, g is the gravitational acceleration, H is the modelled ice thickness, and H_{sw} is the difference between the mean sea level and the ice base topography.

L 22: Is Tm an actual temperature or a temperature difference. Explain this in the text.

We have modified the text in page 4, line 5 to clarify this:

[…] T_m is the temperature difference relative to the pressure-melting point, [...]

Page 5
L 19: Rewrite to something like: where ub is the Weertman sliding velocity (Eq. (2)) and us the surface SIA velocity, respectively.

We have modified the text as suggested:

where u_b is the Weertman sliding velocity (Eq. 2) and u_s is the surface SIA velocity.

Page 6

L 11-16: Do not mention sliding-SIA but rather SIA including (Weertman) sliding. Here it could also be discussed the carefulness of using Weertman sliding, as discussed by Bueler and Brow (JGR, 2009; see also their Appendix B) it leads to a discontinuities in the velocity field.

We have modified the text as suggested:

As described in Section 2.1, the SIA solution in SICOPOLIS is computed using the Weertman sliding component coming from Eq. 2 as a boundary condition. To assess the influence of a SIA solution including Weertman sliding, we have divided this hybrid scheme into two: A sub-scheme (HS-2a) that replicates the idea of Bueler and Brown (2009) and prescribes no basal velocities in the computation of the SIA velocities, and a sub-scheme (HS-2b) that keeps the Weertman sliding component and uses it to compute a slightly modified weight [...]

We have mentioned the discussion on Weertman-type sliding in the description of the model with the respective reference to Bueler and Brown (2009).

L 16: change to: where ub is the basal sliding velocity as in ..

Done.

L 28: At the end of section 2.2 a table could be added that summarises the 4 schemes with the column show something like : 1) name, 2) sliding 3) reference to equations, 4) reference to studies

Done. We have added the table as suggested.

Page 7

L 23-24: Can you explain here and perhaps clarify in the text why you stop the adjustment when the difference between modelled and observed ice thickness is reduced?? A reduction of this difference is actually what you want right?

Because the previous adjustment can still affect the evolution of the ice sheet during the next time steps. In other words, it could contain the exact amount of adjustment needed for the best fit, requiring just a few extra steps to minimize the difference. Without this, the algorithm would only check for the magnitude of the difference, adding a potentially unnecessary extra adjustment that could result in an overshoot. If the previous adjustment is not enough, the small time step used (50 years, which is not possible without our algorithm) will ensure a prompt correction. We have modified the paragraph to clarify this (see previous points).

The text has been modified to clarify this:

The iterative technique involves an additional limiting condition that prevents over-adjustments of C_0. At each time step and individually for each grid point, if the adjustment implemented at the previous *time step reduces the difference between the modelled and observed ice thickness, the adjustment is skipped for the current time step. This allows previous adjustments to fully develop their effects over the following time steps and prevents the technique from adding unnecessary extra adjustments that can result in overshoots. The calibration is reactivated when the time derivative of the modeled ice thickness becomes zero (i.e. the difference between modelled and observed is not reduced anymore) or the misfit starts increasing (e.g. due to increased influx from surrounding areas). Our experiments have shown that this additional feature enables the use of a smaller Δt_inv (50 years here compared to 500--10,000 years in Pollard and DeConto (2012)), because further adjustments will only be applied when and where strictly necessary. A further benefit is that it indirectly allows non-local adjustments of C_0 influence the local ice dynamics: If an adjustment applied in the vicinity of a grid point reduces its misfit, further adjustments at that grid point will still be halted.*

L 22-29: This paragraph could be replaced to Section 3.

We prefer to keep this paragraph here, since it is an important part of the calibration algorithm and follows the description of other constraints in the method.

Page 8
L 13: On spin up procedures for ice-sheet models see also this paper, you might want to refer to this. Fyke, J. G. and Sacks, W. J. and Lipscomb, W. H., A technique for generating consistent ice sheet initial conditions for coupled ice sheet/climate models. Geoscientific Model Development, 7, 1183 – 1195, 2014.

We have added the reference as suggested.

Page 9
L 15-16: Reword: a simulation time of 100.000 years (100 kyr)

Done.

L 21-22: Spanning 100 kyr (mention exact length of your experiments)

Done.

Page 10
L 1: Remove -and get insight into-

Done.

L 4: Be specific: observed ice-sheet thickness and surface velocities as a measure ..

Done.

L 19: .. the variations of the total grounded ..

Done.

L 26: Numerically more or equally stable, and also a similar computational time com- pared to the SoS?

As explained in the paragraph. All hybrid schemes take longer than the SoS due to the calculation of SStA velocities over a larger domain. However, the hybrid schemes require less of these (in this case, very long) iterations to get convergence in the solution, which makes them less prone to numerical instabilities compared to SoS. We have modified the paragraph to clarify this:

Compared to the SoS, the computation of SStA velocities in grounded ice sectors implies an extra computational effort for each iteration in the numerical solvers, with the computing time increasing by a factor of ~4 for the applied hybrid schemes. The computing time of the HS-1 is somewhat shorter relative to the other hybrid schemes (but still longer than for the SoS) due to the prognostic identification of ice streams that does not require a computation of SStA velocities over the entire ice sheet. However, we have observed that the iterative solvers in the model require a substantially smaller number of iterations when the hybrid schemes are used, making them numerically more stable compared to the SoS.

Page 11
L 1: Change to: that include basal sliding with the SIA solution

Done.

L 4: Remove -high frequency-

Done.

L 7: The inset of Figure 1

Done.

L 13: Change to: that include basal sliding with the SIA.

Done.

L 20-21: The large overestimation of ice thickness between Shakleton Range and the Pensacola Mountains as you state is seen in all panels of Fig. 2. Could you give a more clear explanation why this particular area is overestimated. Also note that this is a region of large uncertainties in ice thickness (see Bedmap2 paper).

As you have pointed out, this area has one of the largest uncertainties in the BEDMAP2 data. The lack of observational data produced an unrealistically smooth bedrock topography (Fretwell et al., 2013). We attribute to this uncertainty the absence of topographically driven ice streams that fosters the accumulation of ice at the interior. We have added this point to the text.

L 28, 32: I suggest not to refer to Fig. 1a but to: the inset of Fig. 1.

Done.

Page 12
L 2: Change to: over the areas where there is no sliding and C0 is not inverted.

Done.

L 32: Change to: .. that are unresolved by the model due to its..

Done.

L 34: Remove comma after divides.

Done.

Page 13
L 7: Remove commas before and after for example

Done.

L 26: Change to: .. projection onto the course horizontal grid we use here.

Done.
L 29: Remove comma after simulation

Done.

L 33: Change to: Equation (10) uses the basal velocities from equation (2) to compute..

Done.

L 34-35: Where sliding velocities from equation (2) are high.

Done.

Page 14
L 1: Here also perhaps mention how H-2a compares to H-2b?

Done.

L 5: are implemented

Done.

L 5: Change to: .. hybrid schemes, Fig. 4 illustrates the

Done.

L 22: Remove comma after general

Done.

L 23-24: At a first glance it may

Done.

L 25: Change to full stop: the case.

Done.

Page 16
L 2: Change to: The misfit for Hs-1 increases

Done.

L 4: The Hs-3 experiment shows an ..

Done.

L 19: Change to: the basal velocity in the Hs-2b experiments

Done.

Page 17
L 24: Remove commas before and after continental-scale.

Done.

L 25: Remove entire

Done.

Page 18
L 1: Also mention the range of the ice thickness errors not only the means.

Done.

L 17: Change to: which adds up SIA (without sliding) and SstA.

Done.

L 29: Also mention the uncertainties in ice thickness and surface velocities (see their respective references).

Done.

Tables and Figures

Table 2:
Should the sum of the last two columns not be 100%? Please clarify and explain.

The columns show the percentage of area where the flow is predominantly dominated either by the SIA or the SStA, defined as the area where the weight w is <0.25 (SIA-dominated) or >0.75 (SstA dominated). Areas where w is between 0.25 and 0.75 are excluded from these percentages, since they present a closer mix of both velocity solutions. We have clarified this in the caption.

Fig. 1:
- Label a and b can be removed, rather refer to fig. 1 and inset of fig. 1.

Done.

- Instead of a coloured bar for the pre-initialization the names of the periods could also be placed above (pre-initialization period and automatic calibration).

Done.

- In the legend of the inset the difference between solid and dashed should be clearer.

Done.

Fig. 4:
It is not really clear which scheme represents the surface velocities the best. Could you also add a correlation/R-square value to the scatter plots? Also use the correlation in the discussion.

Thanks for the suggestion. We have added the root-mean-square error to the scatter plots, which provides a simple way to clarify this. This information is also used in the results and final discussion sections to aid the determination of the "best" scheme overall.

Fig 8: Hs-2a always seems to be have a misfit, whereas the other scheme should have a possible solution which closely fits the observations, for a particular (probably not the same) set of parameters. Please explain and discuss this in the conclusions.

HS-2a prescribes zero basal velocity before the computation of the SIA. Sliding comes from the SStA, but only in those regions where the SStA velocities are high (see the weights w in Figure 4). Thus, at the continental interior the contribution from the SStA is small and not enough to prevent the overestimations of ice thickness that produce the misfit observed in Figure 8. The HS-3 does not scale the SStA contribution with any weight, and thus is able to prevent the overestimations. This has been added to the final discussion (see also our reply to the third main comment).

Cited studies:

Bentley, M. J., Cofaigh, C. Ó., Anderson, J. B., Conway, H., Davies, B., Graham, A. G., ... &

Mackintosh, A. (2014). A community-based geological reconstruction of Antarctic Ice Sheet deglaciation since the Last Glacial Maximum. Quaternary Science Reviews, 100, 1-9.

Bueler, E., & Brown, J. (2009). Shallow shelf approximation as a “sliding law” in a thermomechanically coupled ice sheet model. Journal of Geophysical Research: Earth Surface, 114(F3).

Fretwell, P., Pritchard, H. D., Vaughan, D. G., Bamber, J. L., Barrand, N. E., Bell, R., ... & Catania, G. (2013). Bedmap2: improved ice bed, surface and thickness datasets for Antarctica. The Cryosphere, 7(1).

Mackintosh, A. N., Verleyen, E., O'Brien, P. E., White, D. A., Jones, R. S., McKay, R., ... & Miura, H. (2014). Retreat history of the East Antarctic Ice Sheet since the last glacial maximum. Quaternary Science Reviews, 100, 10-30.