

Responses to review 1 of the manuscript “Comparison of hybrid schemes for the combination of Shallow Approximations in numerical simulations of the Antarctic Ice Sheet ”:

General comments

This paper describes a study of 4 hybrid schemes, combining the Shallow Ice and Shelfy Ice approximations for Antarctic Ice Sheet simulations. The 4 schemes are implemented into the open source ice sheet model Sicopolis (Greve, 1997), and a number of simulations are made for the Antarctic Ice Sheet with each and the resulting basal sliding coefficients compared. Analysis of the calibrated sliding parameter is done and runs where average values from all 4 schemes is applied, as well as the results from other schemes (swapped).

We thank the reviewer for their very constructive comments that helped to improve the clarity of the manuscript overall. Our replies are provided in blue.

The paper is clear but the conclusions are not clearly specified and reader is left with wondering if authors have been able to conclude which scheme is preferred and how they will continue working with one, or all, of the schemes. A clearer message and conclusions from the study, as well as more concise analysis of the results (e.g. Figures 5 and 6) would improve the paper.

We agree. We have modified the text to include our conclusions regarding the relative performance and the potential for future applicability of each scheme. Our evaluation criteria do not only include the fit of the results to observations, but also the numerical stability of the schemes. Our experiments at higher grid resolutions show that 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. Following the suggestions of the reviewer, we have tried to make the results section more concise (see the related point below).

The structure of the paper could improve by adding a separate data section, before the

Methods section, the text in lines 4-22 on page 8 would be better located in a separate Data section and a more detailed information about the data used for this modeling approach would be beneficial for the paper. Also lines 19-24 on page 9 would be better located in a data section.

Done as suggested. The data sets are now described in a greater detail in a separate section, including their respective uncertainties. To keep the flow of the text, we have located this new section after the description of the model and before the description of the experimental setup.

The wording of the method used and the results is confusing, in this study a forward method is used to determine or rather calibrate C_0 , the basal sliding coefficient, and therefore it is confusing to call it inversion technique (e.g. line 23 page 8) or inversion (e.g. line 28, page 8), suggest to call it iterative technique or calibration, see further comments below (as in line 5 in the abstract: "the model is calibrated using an iterative technique . . .")

Done. All instances have been replaced as suggested.

It is not clearly explained why authors are swapping the determined C_0 values between the calibrated models, why is this useful? Is the following ice sheet adjustment indicating model consistency? As stated in the paper, each method has its own way of combining the approximated term and not clear why swapping of the determined (or calibrated) C_0 values would give any insight in the model behavior or result. It is discussed that the different methods, and the results of Pollard and DeConto (2012) give qualitative similar results, with high/low values in same regions, but the numerical value of C_0 is method/model dependent and not clear what swapping of the results is useful for.

These experiments are performed to show the effects of prescribing a distribution of sliding coefficients derived from one ice model (with a specific scheme/approximation for the stresses) into a different ice model. Thus, these results are indicative of the degree of inter-model consistency. This concerns not only differences between the basal sliding approaches implemented in each model, but also models using the same sliding law as part of different hybrid schemes. Given an increasing number of studies attempting to quantify the basal conditions under ice sheets through a variety of methods including ice flow models, our experiments show that one need to be careful when using these results as input data sets in glaciological models. The text of the Results and Discussion section has been modified to clarify this.

The text on page 15 is not clear and the two figures 5 and 6 are not discussed in satisfying manner, what information about the schemes and the results can we draw from this analysis?

The text has been modified to clarify this (see previous point). To provide a clearer and more concise analysis of our results, we have removed figures 5 and

6, since they do not provide the information that is not already included in the main text.

Specific comments:

The wording in abstract and introduction is confusing, the Shallow Ice approximation is a zeroth order approximation assuming the thickness of the ice is much smaller than the length scale and thereby horizontal stress gradients omitted. This approximation has no assumptions for the sliding law and a full system model would equally have to assume some sliding approximation, or shelfy ice solution to account for sliding. Line 2 in the abstract should be reworded (SIA is not applicable . . . where basal sliding operates) and line 6 (minimal sliding) on page 2. Consider to rewrite also lines 2-5 on page 2, (“neglecting terms” and “simplest and most commonly used”): the Shallow Ice Approximation is a zeroth order approximation of the momentum balance equation assuming the H/L is very small.

We have reworded all instances in the text as suggested.

The wording in the abstract in lines 9 and 11 is not clear and should be rewritten for clarity: “averaged and swapped” cannot be understood until reading the main text of the paper and therefore needs some clarification in the abstract. “.. this requirement for internal consistency” - is not clear in this context and needs more explanation.

Done as suggested. The text as been modified to clarify this.

I find missing an overview figure indicating the location of the areas that are named in the paper, such as Dronning Maud Land, Coats Land, Siple Coast etc., this would be useful for readers not familiar with place names in Antarctica and makes the paper easier to read.

The location map has been added as suggested.

Note that “Blatter-Pattyn models” were developed much before the asymptotic analysis by Schoof and Hindmarsh (2010) and therefore it would be appropriate to reference the earlier papers with these model developments.

Done.

Lines 27-29 on page 2 are not clear, more information and detailed explanation of what authors mean is needed here, why would it be necessary to look for further explanations when two models yield similar result under similar forcing?

We mean that if the ice models are different (e.g. as in the hybrid schemes presented in this study), each model can independently calibrate its parameters to arrive at a similar solution (as shown in our results). Since the idea provided

by this line is discussed later in the main text, we have removed it from the introduction to avoid confusion.

Model resolution is very low, were any tests made to assess the sensitivity of the method to grid resolution?

Following the concerns of the reviewer, we have managed to rerun all experiments at a higher resolution of 20km. Additionally, we have performed some sensitivity tests at higher resolutions of 15 and 10km, which show that the modeled ice flow near the ice sheet margins benefits from a denser grid due to the added flexibility (i.e. more grid points) provided to the calibration procedure, further (but slightly compared to the change from 40 to 20km) decreasing the misfit between the modelled and observed fields, representing a quantitative improvement that does not impact our conclusions.

- or to potential errors/inaccuracies in the topographic data? How good is the observed topography?

The sensitivity of the method to various model and data uncertainties (including grid resolution) was assessed by Pollard and DeConto (2012). For the topographic data, they perturbed the bedrock elevations using Gaussian noise with different noise amplitudes, finding that widespread errors of ~400m or more are necessary to considerably affect their results. In BEDMAP2, this level of uncertainty is found primarily in two regions: Between the Recovery and Support Force glaciers, and in Princess Elizabeth land (Fretwell et al., 2013), which are characterized by unrealistically smooth topography. In the former region, we attribute to this smoothness the overestimation of ice thickness found in all the applied hybrid schemes, which are not able to reproduce the observed ice streams without the proper topographic routing.

The quality of English is generally good, but in many places the wording is strange and needs some editing, it would be beneficial for the paper to have a thorough editing of all the text. Below are a number of places indicated where rewriting/editing would improve the quality of the text.

The revised manuscript will undergo the suggested check. We have applied all the corrections suggested by the reviewer.

Technical corrections:

Page 1, line 2, suggest to replace “ice dynamics” with “stress within the ice”

Done.

Page 2, line 10, suggest to add “at the base” after “no friction”

Done.

Page 2, line 21, suggest to rewrite, models do not “detect”, replace with something like “the algorithm used to identify . . .”

Done.

Page 2, line 23, suggest to replace “versus” with “compared to”

Done.

Page 2, line 25, suggest to replace “superposition” with “combination”

Done.

Page 2, line 31, suggest to rewrite, replace “these result from ..” with “ these are . . .”

Done.

Page 2, line 32-33 reword sentence: “Mechanical properties . . . may serve as an example of . . . parameters” does not make sense.

Done.

These limitations include the scarcity of observational data needed to reduce the errors introduced by poorly constrained model parameters, e.g., the distribution of water-saturated sediments at the ice sheet base and their potential to enhance basal sliding.

Page 2, line 34, something missing “widespread misfit” of what? Elevation? - do all models show this type of misfit? A reference to a study showing this would be useful here.

The text has been modified to clarify this:

This is currently considered to be a major source of large, widespread misfits between the observed and modelled elevations of the AIS (e.g., de Boer et al., 2015) .

Page 3 Line 11, missing information and reference, what observational data sets are used?

The text has been modified to clarify this:

[...] using state-of-the-art observational data sets of ice thickness (Fretwell et al., 2013) and ice surface velocities (Rignot et al., 2011).

Line 14, add “the” between by and different

Done

Line 19, see comment above, replace “inverse” with “iterative”

Done.

Line 29, the model does not “consider” or is “keeping track” of temperature, the algorithm or post processing does, suggest to rewrite. Suggest to add “computed” after “ice is”

The text has been modified to clarify this:

SICOPOLIS is able to model polythermal ice sheets, i.e., explicitly identifies potential temperate regions in which the modeled ice temperature is at the pressure-melting point (Greve, 1997).

Page 5 Line 1, see comment above, “detect” seems a strange selection of word, suggest to replace with “determine” or “identify”

Done.

Line 14, not clear wording “a consistent use of inverted distributions of Co” - (replace inverted with determined, or calibrated)

Sentence deleted (this info is a duplicate of the beginning of paragraph).

Line 21, not clear wording: “enters the computation of SStA velocities” - suggest some- thing like “and SStA velocities are computed for this point”

Done. The text has been modified as suggested:

If r is larger than the threshold, that grid point is flagged as streaming ice and SStA velocities are computed for that point.

Line 25, suggest to delete “which is assigned to the SStA” this is explained in next sentence (and add velocity after SstA)

Done.

Page 6, Line 2, suggest to add “SIA and SstA” before “Velocities are . . .”

Done.

Line 16-17, unclear wording, suggest something like “It is rather used to determine. . . . the computed SStA contribution should partly or completely replace sliding”

Reworded as suggested:

It is rather used to determine how much the computed SStA contribution should replace the basal velocities used to compute the SIA solution.

Line 24, sentence is not clear, suggest to rewrite, something like “In the continental interior the modelled ice flow is dominated by the SIA solution

Modified as suggested:

This approach is based on the assumption that on ice shelves the SIA contribution is negligible due to low surface gradients, and therefore the modelled ice flow is dominated by the SStA solution, whereas in the continental interior the modeled ice flow is dominated by the SIA solution.

Line 29 see comment above, replace “inversion” with “determination or calibration”

Done.

Line 30, replace “infer” with “determine”

Done.

Page 7, Line 11, see comment above, “easier activation of the inversion procedure” suggest to rewrite to something like “more frequent computation of sliding velocity” - what does “slightly” mean here?

The text has been modified to clarify this:

In contrast to previous studies using SICOPOLIS where $\gamma=1K$, this parameter is set to $3K$, which allows for a more frequent calibration of the sliding coefficients (Pollard and DeConto, 2012).

Line 14, suggest to delete “similarly, and”

Done.

Line 16, suggest to replace “speed” with “velocity”, not clear what “local adjustment” means here, elevation, or Co? suggest to replace “keeps the inversion” with “prevents the method”

Done. The text has been modified to clarify this:

Additionally, we implemented the following condition: When the computed surface ice velocity reaches an ancillary speed limit at a certain grid point, the adjustment of C_0 for that point is halted.

Line 17 suggest to replace “over-adjustment of” with “over-adjusting”, replace “speed” with “velocity”

Done.

Line 18, suggest to delete “the” before “numerical”

Done.

Line 20, replace “This” with “These” (plural of values)

Done.

Section in lines 22-29 is not clear, and needs rewriting for clarification what is the time in the iterative method or calibration?

Done. We have modified the paragraph to clarify this. The new version reads:

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.

Line 24, “time derivative of the ice thickness”, do you mean observed or modelled?

We mean modeled. We have modified the paragraph to clarify this (see previous point)

why suspend adjustment if previous time step reduced the difference?

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).

(line 24): What process?

We mean the calibration. We have modified the paragraph to clarify this:

The calibration is reactivated when [...]

Line 26 replace “overshoot prevention” with “over-adjustment of Co”

Done.

line 27, suggest to replace “lets” with “allows” and delete “to” before “influence”

Done.

line 31, add “the” before “fringing”

Done.

Page 8 See comment above, move lines 6-18 to a Data description section

Done.

Line 6, re- place “which” with “that”

Done.

Line 19 suggest to add “is used to” before “account”, replace “changes” with “discrepancies” , replace “by” with “with”

Done.

Line 23, see comment above, replace “inversion” with “iterative” or calibration

Done.

Line 24, this is forward method, suggest to replace “inversion” with “iterative”

Done.

Line 28, replace “inversion” with “calibration” see above

Done.

Line 30, suggest to delete “one-to-one”

Done.

Line 33, suggest to replace “not accounted for” with “is not included in the simulations”

Done.

Page 9 Line 1, see above, replace “inversion” with “calibration”

Done.

Line 12, replace “simulations” with “calibration run”

Done.

Line 13, see above, replace “inverse technique” with “iterative” or calibration

Done.

Line 27 what do you mean by “glacier flux gate” do you mean an outlet glacier?

We mean the area where an ice stream reaches the grounding line, usually exhibiting very fast ice flow. We have changed the text to clarify this:

These mainly occur close to the ice sheet margins where an ice stream reaching the grounding line is often represented by only one grid cell at low resolution

Line 31 not clear text “which overlap at their interface” needs clarification here

We have changed the text to clarify this:

11 equidistant grid points for temperate ice and 81 grid points for “cold” ice densifying towards the base, sharing the grid point at their interface.

Page 10 Line 8, suggest to add “a” before “new”

Done.

Line 12-13, this sentence is not clear and needs rewriting (what is internal operation of the hybrid schemes?)

We have changed the text to clarify this:

In addition, the influence of variations in the parameters controlling how each scheme combines the SIA and SStA velocities is assessed for a wide range of parameter values.

Line 17, what does “quasi-equilibrium” mean here?

Replaced by “equilibrium”, used and defined below (see next point)

Line 19, what is “negligible” in this context? A percentage or some value would be useful here

We consider negligible a change over a prolonged time (>10000 years) smaller than 0.01 %. We have added this to the text to make it clear.

Line 24, suggest to replace “prevents” with “does not require”

Done.

Page 11 Line 9, the smallest error of 49.9m is according to the table using HS-1, is there an error in the table? If not then this section should be rewritten to reflect that.

We have updated the table to reflect the increase in grid resolution and modified the paragraph accordingly.

Line 21, replace “independently” with “independent

Done.

- here some discussion would be appropriate about if this is related to the common SMB forcing or geothermal heat flux?

Done. We also included the potential influence of the uncertainties in the topographic data, as described above (see related specific comment).

Line 22, add “simulated” before “frozen”

Done.

Line 24, suggest to edit, change to something like: “far below the pressure melting point” and delete “the” before “white coloured”

Done as suggested.

Line 25, edit the text, delete “on the other hand”, suggest to write “Areas where ice is underestimated . . .” - but what does “sparsely distributed” mean?

Done. We have changed the text to clarify this:

Areas where ice thickness is underestimated are mainly located at and around the ice margins [...]

Line 27 replace “inversion” with “determination” or “computation”

Done.

Line 31, replace “inversion” with “iterative” or calibration

Done.

Page 12 Line 2, replace “inverted” with “applied” or “computed”

Done.

Line 4-5, same comment, suggest to write “region where Co is not applied”

Done.

Line 6, replace “inversion” with “calibration”

Done.

Line 21, delete “also”

Done.

Line 26, what do you mean here? What significant modification of Pollard and DeConto (2012) scheme would result in similar values of Co?

We mean that if Pollard and DeConto (2012) used a different hybrid scheme, we would expect a similar degree of variation in their calibrated values of C_0 . This line duplicates what is implied in the first sentence of the paragraph, and therefore it has been deleted to avoid confusion.

suggest to replace “perturbation” with “distribution” or “pattern” and delete “inverted”

Done (see previous point).

Line 30 add “the” before hybrid

Done.

Line 33 replace “small” with “low” and add “the” after “near”

Done.

Line 35, what are “high velocity flanks”? suggest to add “sheet” before “margins” and at the end of line, after “ice”

We mean the fast flowing ice streams reaching the grounding line. We have modified the text to clarify this and added the corrections:

distinguishing between ice sheet areas with low velocities near the ice divides and fast flowing ice streams reaching the ice sheet margins

Page 13 Line 2, strange wording, suggest to replace “contaminated by” with “characterized with”

Done.

Line 6, add “the” before hybrid

Done.

Line 7, suggest to replace “in” with “at”

Done.

Line 8, “flux gates” - not clear, is this a specific location?

We mean the portion of the glacier closest to the grounding line. We have modified the text to clarify this:

Furthermore, modelled surface velocities are generally overestimated at the grounding zone of most outlet glaciers

Line 11, delete “inferred” and delete “On the other hand”

Done.

Line 14, delete “inferred”

Done.

Line 19, delete “a” before slow ice motion, replace “flow speed” with “velocity” and “predicted” by “simulated”

Done.

Line 21, delete “Arguably”

Done.

Line 22, replace “stagnated” with “stagnant”

Done.

Line 25, not clear wording “deviated to either side” - “pushed to merge” are you referring to modelled or observed velocity? - what side of what?, what is pushing what?, suggest to replace “deficiencies” with “errors”

Done. We have modified the text to clarify this:

In other cases, the modelled rapid ice flow follows a different route compared to observations, sometimes merging with adjacent ice streams. These shifts may originate from local errors in the bedrock topography data accentuated by its projection onto the coarse horizontal grid we use here.

Line 28, according to the table, it is HS-1 that has the minimum misfit for the ice thickness

Done. The text has been modified to fix this.

Line 30, see above, replace “inversion” with “calibration”

Done.

Line 31, suggest to delete “in the modelled surface velocities”

Done.

Line 33, what do you mean by “opposite ends”?

We mean that their degrees of fit to observations is very different (keeping in mind the limitations of the method, as explained in the manuscript), with SoS performing the poorest and HS-2b representing one of the best fits in terms of final ice sheet geometry. We have modified the text to clarify this:

Although the results of the HS-2b simulation presented in Section 5 are in many aspects similar to those from the SoS, their respective skills in reproducing observations are very different [...]

Line 34, suggest to replace “enabling” with “adding”

Done.

Page 14 Line 4, suggest to replace “imply” with “control”

Done.

Line 5, what does “internal operation of the hybrid scheme” mean?

We have modified the text to clarify this:

In order to provide a deeper insight into how each hybrid scheme combines the SIA and SStA velocities, [...]

Line 11, suggest to replace “the velocity field from the observational data set” with “the observed velocity” Line 12, suggest to replace “simple differentiation” with “transition”

Done.

Line 17, reword, “tends to prevent” to “can prevent” and delete “s” in causes (and cause underestimation..)

Done.

Line 24, what is a “cursory comparison” ? clarification is needed

We mean that is readily visible from the figures. We have modified the text to avoid confusion:

At a first glance it may seem that overestimations are caused by an excessive contribution of the SStA, but a comparison with the SoS scatter plot shows that this is not necessarily the case.

Line 29-30 suggest to rewrite (delete Here we attempt) and write: To isolate the influence . . . we plot averaged errors . . .

Done.

Line 35, replace “inferred” with “resulting” or delete “inferred”

Done.

Page 15 Line 1 add “the” before different

Done.

Lines 2-5 this sentence is not clear and does not explain the difference between the 3 figures in Fig.5 clarification is needed here

Figure 5 has been removed (see previous points).

Line 9, suggest to replace “looking” with “comparing”

Done.

Line 19 delete “the” before “period”

Done.

Line 26 – this line is not clear, what do you mean by “generalizing” – see comment above, it is not clear what information about the model can be drawn from Figures 5 and 6

We have modified the text and removed Figures 5 and 6 (see previous points).

Line 34, delete “On the other hand,”

Done.

Page 16 Line 2 suggest to add “distributions of Co from” before the HS-1 and HS-3

Done.

Lines 1-6, it is not clear from this text what this analysis gives for useful information about the different schemes used in the study

The text has been modified to reflect our answer to the related general comment.

Line 8, add “The” at beginning of line

Done.

Line 11, rewrite: “allows us” is a strange wording here

Done.

In order to explore the sensitivity of the results to parameter variations within this parameter space, we perform an additional series of experiments where we vary the somewhat arbitrary threshold and reference quantities used by some of the hybrid schemes.

Line 23, what is the criteria for selecting lower and upper limits for each scheme?

For the HS-1 values outside the [0,1] interval are invalid. For HS-2a and HS-2b, our tests showed that higher or lower values produce no noticeable differences compared to the representative limits we chose. We have modified the text to clarify this:

Here we test parameter values within a range that contain almost every possible scenario, either because values outside the range are unphysical (HS-1) or they

exhibit no noticeable differences compared to the range limits (HS-2a and HS-2b).

Line 31, suggest to replace “decrease” with “get worse”

Done.

Page 17 Line 4, delete “by” before 2.5%

Done.

Line 24, delete “,” after “enables”

Done.

Line 27-29 suggest to rewrite, something like “differ in the way ways the 1) relative contributions . . . are computed 2) areas where . . . is applied is determined, and 3) basal sliding is accounted for.

Done.

Line 30, see above, replace “inverse” with “iterative” or “calibration”

Done.

Line 31, delete “all” or add “the applied” after “all”

Done.

Line 32, add “the” before “schemes”, replace “below” with “less than” and add “the” before “total”

Done.

Page 18 Line 3, delete one “can”

Done.

Line 5, add “the hybrid” after “all”

Done.

Line 7, add “the” before hybrid

Done.

Line 8, suggest to replace “develops” with “exists”

Done.

Line 15 What particular scheme is discussed here? It is not clear

Done. We have modified the text to clarify this:

We have found that the HS-2a tends to increase basal sliding coefficients in an attempt to compensate for an insufficient sliding that would otherwise lead to a larger misfit.

Line 19 add “,” after Here

Done.

Line 27, what does “the neglect of paleoclimate signal” mean here – can applied geothermal heat flux, or applied SMB play a role here?

We mean the neglect of transient temperature effects from previous glaciations and the glacial isostatic adjustments in the bedrock. In the areas mentioned in the text, the geothermal heat flux mostly allows the schemes to calibrate the sliding coefficients, providing the good fit mentioned in the text. These processes were tested by Pollard and DeConto (2012), finding only a small on the results. A dryer climate forcing over these areas would in fact reduce the velocities, because less mass would need to be removed in order to obtain the same fit. We have modified the text to include these points:

Such misfits might originate from other factors such as, e.g., the lower model resolution, the assumption of an ice sheet in equilibrium, or biases in the model-based geothermal heat flux and/or climatic forcing data sets (Pollard and DeConto, 2012).

Line 33, does the scheme affect the way the temperature is computed?

Not explicitly. However, due to the coupling of the evolution equations the computed velocities do affect the ice temperature. From the four hybrid schemes, only the HS-1 shows a noticeable difference in the basal temperature field, due to its particular threshold-based selection of grid points where the combination of SIA and SStA is applied. We have added a discussion of this point in the conclusions, which contributes to our evaluation of the applicability of each hybrid scheme in future work (see first point)

Line 34, does the scheme allow determination of hard or soft bed? – it can only indicate high or low value for C_0 , or what?

We agree. The calibration of the sliding parameter is just an attempt to quantify the combined effect of several processes influencing whether and how the ice slides over the bed. We have modified the text to clarify this:

Furthermore, there is a qualitative agreement in the patterns of low vs. high values of C_0 obtained from each calibration run.

Page 19 Line 3, suggest to replace “inferred” with “calibrated”

Done.

- not clear how high variability can “provide an opportunity to quantify the effects of the uncertainty” – suggest to rewrite to clarify what is meant here.

We have modified the text to clarify this:

We assessed the effects of the high variability in the calibrated parameter distributions derived from different hybrid schemes by performing additional experiments in which averaged and swapped distributions of basal sliding coefficients are prescribed as external data sets.

Line 8, see comments above, replace “inverse method” with “iterative technique”

Done.

Line 9, what is meant with “internal consistency required to avoid misfit” suggest to rewrite to clarify.

Done (see answer to related general comment)

Suggest that the concluding sentence of the paper will state the main results of the study and how it can be useful for further modelling approaches.

Done (see answer to related general comment)

Figures and tables

Table 2, suggest that the caption include some explanation, referring to text, what the different lines stand for (HS-1 etc).

Done.

See comments above, the lines may have got mixed up, since the text states that the minimum difference for elevation is for HS-2, but 49.9 m is in line HS-1

Done, we have checked this (see answer to comments above)

Figure 2, is this at the end of the simulation?

Yes. We have modified the caption to clarify this:

Comparison of the equilibrium ice sheet states derived from different schemes at the end of the simulations. [...]

Figure 4, suggest to replace “grid cell” with “point” for the right hand column figures

Done.

Figure 5, the figure caption is not clear and needs editing. What does “quantify different ice flow regimes” mean here? The y-axis label is mean velocity ratio, but the text states Surface velocity error, clarify what is shown here. Suggest to replace “by” with “as” before ratios.

This figure as been removed (see previous points)

Figure 6, as discussed above it is not clear how this figure is useful. What information can be gained from this analysis? “retrieved distributions of basal sliding coefficients” is not clear, do you mean standard deviation of the determined values with each scheme in each grid point?

This figure as been removed (see previous points)

Figure 7, what does this figure tell us? What meaning does the median of the inferred distributions of C_0 have? Is this a useful quantity?

Please see answer to related general comment

Figure 8, what do the bumps in the lines of HS-1 with $r\text{-thr}=0.0$ and HS-2b for $v\text{-ref}=1000$ m/yr shortly after 50 kyr mean? Is this instability in the simulations?

Yes, it corresponds to instabilities in the simulations. We discuss this in the conclusions (see also answer to first general comment)

Figure 9 “throughout the simulations” – do you mean at the end of simulations?

Yes. We have modified the caption to clarify this:

Mean differences between the modelled and observed ice thickness at the end of the simulations, [...]

Cited studies:

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