

Response to review by anonymous referee #1

Review comments in italic, responses in regular font.

Berends et al. present a thorough and well-presented study considering the relationship between the 'quality' of an ice sheet model's initial state and its projection over centennial timescales. This is an important question as ISMIP exercises have begun to use the capability of a model to reproduce present conditions as an indicator of its ability to produce tractable projections. Using an ensemble intended to mimic the diversity of ISMIP submissions, but retaining a more controlled overview of the differences by keeping the underlying model the same, the authors find that there is in fact no clear relationship between the quality of the present day snapshot and future projections. This is an important, well-founded, and largely well-described finding, and it is certainly well-deserving of publication following minor revisions. I was impressed by the comprehensive nature of the study, and I have no further suggestions for additional simulations or experiments.

We're happy to hear it!

General comments:

*Section 2 is a good and comprehensive overview of how the model is forced. What is missing however is a good description of how errors and instance quality are calculated *before* we get to the results. I would maybe put this as an additional section before the present section 3. This should include the full RMSE calculation (only introduced now at l244) and the Q_init terms (so basin, pan). I think the future projection methodology should also be introduced before, rather than alongside, the results pertaining to it (at the moment, l315 onwards is a methods paragraph in a results section). It would also be useful to introduce the idea that Q_init terms can be compared to sea level contribution within the methodology, rather than in the results, so that the reader understands why the methods are structured as they are.*

We agree that this suggestion is an improvement to the structure of the manuscript. We will move the description of our experimental set-up and of our quantitative analysis of the results to a separate section.

In addition, all of your simulations show a very small sea level contribution for the first 100 years or even more, which I believe falls below many other projections (and implies a deceleration from currently observed melt rates). This is not central to your study as one would assume that the disconnect between initialisation skill and projection could be proven even for an idealised ice sheet case, but I think it does warrant a bit more discussion than it currently receives.

We will add the following text to our manuscript where we present the future simulations:

“The general pattern of relatively little mass loss until 2100, a marked acceleration over the course of the 22nd century, and an approximately linear trend after 2200, is seen both in our results and in the majority of simulations in the ISMIP6 ensemble by Seroussi et al. (2024). That ensemble ranges between approximately -25 and +25 cm of sea-level contribution in 2100, completely enveloping the range of our results. The deceleration of the present-day sea-level trend in our results is likewise produced by a sizeable minority of the model realisations in the ISMIP6 ensemble.”

Sliding terms are a bit mixed up, going between 'roughness', 'till friction angle', 'slipperiness'. Suggest choosing one and sticking (pun intended) to it.

We will ensure the manuscript uses ‘bed roughness’ when referring to the resistance of the substrate to sliding in general, and ‘till friction angle’ only when referring specifically to the bed roughness as defined in the Zoet-Iverson sliding law.

I'm not totally clear how your 'nudging' schemes relate to what are more generally called inversion schemes (such as Morlighem et al., 2013, <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/jgrf.20125>). I would suggest making your definition clearer, and simply using inversion if your methods are suitably comparable to the likes of Morlighem et al. 2013. Usually when I hear nudging I think of the approach taken by paleo simulations where a fully automated inversion procedure is not possible. Speaking of which, it would be nice to get some description of how your findings might relate to the palaeo simulations that also feature in ISMIP ensembles.

We will add a few lines to the first paragraph of Section 2.3 describing the differences between the (generally more formal, mathematically rigorous) adjoint-based inversion methods used in e.g. Elmer/ice, ISSM, and Úa, and the (generally more physically intuitive) ‘nudging’ methods used in e.g. CISM, GRISLI, and UFEMISM. We will also briefly acknowledge the diversity of opinions regarding the level of similarity between these methods, and the (non-)applicability of the term ‘inversion’ to a method like ours.

Specific comments:

Abstract:

This is a cool paper and it does a lot of things, but I don't believe it warrants a long double-paragraph abstract

We will remove the paragraph break in the abstract.

I think acronym usage should generally be minimised and I do not think sea level contribution needs an acronym (especially not in the abstract)

We will remove the acronym for ‘Antarctic ice sheet’. However, ‘SLC’ currently occurs 36 times in the manuscript, so it warrants an abbreviation. We will adhere to Copernicus’ policy of only spelling out the first occurrence of the phrase (even if that is in the abstract), and only using the abbreviation thereafter.

I note that there are no studies available (to my knowledge) that actually link friction to 'novel observations of the ice sheet's sub-surface', and that this is only cursorily addressed in the discussion. I would be happy to see further affirmative coverage in the discussion, or saying that developing such a method is important, but otherwise I think this suggestion operates a bit like a research trope as it appears to perennially be just out of reach.

We agree with this notion and will discuss this in more detail in the discussion section. The new line now reads:

“Only if and when more, and more reliable observations of the ice-sheet’s sub-surface and temporal evolution become available (possibly from ground-based radar observations, e.g. Kingslake et al., 2025), ...”

l17 These to Our if they are yours

We will fix this.

l21 subglacial bed to glacier bed

This pertains to the bed underneath the ice sheet; while one could argue the difference is only in the scale, we prefer to avoid mixing the two terms. We will change this phrase to ‘substrate’.

l24 differently than is a bit imprecise, 'force the system away from...' or similar?

Rather than attempting to explain this complicated concept in a single line, we will remove it and rely on the comprehensive explanation in the introduction.

Introduction:

l33 I think worth very briefly mentioning the nature of these instabilities if you are going to introduce instabilities at all

We will mention and reference the marine ice-sheet instability and marine ice-cliff instabilities here.

l60 (bulk) to bulk or [].

Will change to square brackets.

l64 non-existence to not existing at all

Will change this.

l64 I would introduce that these can be derived from inversions or 'nudging/adjustment' procedures in the case of simulations running from a palaeo state

We will mention the approach of constraining a model's parameter space from a palaeo ensemble; however, we are not aware of any studies that attempted to constrain the 2-D distribution of basal friction/hydrology from palaeo simulations.

l83 You may want to consider starting one of these sentences with a word other than however. However, this is just a stylistic choice

We will do so.

l99 the surficial to surface

The Merriam-Webster dictionary defines 'surficial' as an adjective meaning 'of or relating to a surface'. That is precisely what is meant here, so we will not change it.

l105 I don't know if 5 km can be correctly described as high resolution, and it would be good to give a maximum resolution. I see this is in Table 6, but good to put the info here, too.

We will explain the spatially varying resolution of UFEMISM's unstructured grid in a bit more detail here already.

l117 i.e. to as, or consider writing out in fully vectorised form

We will rephrase this.

l127 See also the general comment, but a bit more description about this procedure is needed. How is the nudging achieved, is an inversion algorithm used?

The next three paragraphs explain the different nudging procedures.

l130 See an earlier comment, but I think good to make this introduction a bit clearer, and perhaps to include a bit more information about how other ISMIP models handle this (and why you use the methods you do)

We will expand the explanation of the nudging method in relation to other inversion methods.

l137 Suggest calling this height-velocity nudging scheme as H is not defined for a while (or to introduce the nomenclature in the first sentence of the subsection)

To maintain consistency with earlier publications, and with the source code and documentation of UFEMISM, we will stick to the abbreviated nomenclature.

l143 Can we get a bit more information on the f_u and f_d functions?

We will clarify what these functions mean conceptually, and refer to the original publication (Berends et al., 2023b) for the equations (as particularly the equations for deriving the flowline are a bit cumbersome, and not needed in the current manuscript).

l144 As above, I think replacing i.e. with a word is appropriate

We will fix this.

l150 I'm not sure personal comms falls under EGU citing guidelines. Suggest just removing the parenthetical statement

We will remove this statement, and instead add a similar one to the Acknowledgements section.

l169 As with section 2.3, suggest putting in a preface to the sub-shelf melt options. WOA as an acronym is not introduced until later in this subsection. I suggest just calling it World Ocean Atlas throughout (or at the very least in the subsection heading)

We will expand the acronyms in the subsection titles.

l171 Suggest stating which equation from Favier, and perhaps also why that choice

We will clarify that this corresponds to Eq. 4 in Favier et al. (2019), and that it is currently the most widely-used zero-dimensional parameterisation.

l188 Further clarification about nudging vs. inversion schemes

This will be covered by the extra lines in the introduction section discussing the differences and similarities between different inversion approaches.

191 I don't see a definition of BMB (basal mass balance?) and it's only mentioned four times -- remove the acronym?

We will rename this subsection to “Inverted melt rates”

l216 I think sections 3 and 4 could be folded into a larger results section that features a brief introduction to guide the reader

Agreed, also following the earlier general comment.

l223 This information could go higher up when resolution is introduced

These details are specific to the simulations presented here, not to the model in general. We will keep them in the description of the simulations.

l226 This is a bit unclear. Multiple resolutions to get to the start point of one simulation, or separate simulations. If so, why the difference in years? Needs a bit more explanation.

The nudging scheme essentially solves a parabolic PDE to model the convergence of the bed roughness to a steady state (which is the distribution that results in the smallest error in ice geometry and/or velocity). As such, it requires an initial value for the bed roughness – and the closer that initial value is to the steady state, the fewer model years it requires to converge to within a certain tolerance. For the first, lowest-resolution simulation, we take an initial guess of $C(x,y) = C_0$ (where C_0 is typically a relatively high value, resulting in slow-flowing ice). From that initial guess, the nudging scheme requires about 40,000 model years to converge (actually, we find the solution to be acceptably stable after 20,000 years – we prefer to err on the side of caution). In principle, we could do exactly the same at the higher resolutions, and those simulations would require the same amount of model years (short-wavelength errors in the bed roughness are diffused faster, so the tail of the converge is dominated by the long-wavelength errors). However, running our model for 40,000 years with a 1 km grounding-line resolution would require over a week of wall time, which we find inconvenient. So instead, we take the converged solution of a lower-resolution simulation as the initial guess. In that solution, the long-wavelength terms in the bed roughness error have already diffused, and only short-wavelength terms remain (since the lower-resolution model could not resolve those terms). Those terms are diffused much faster, so that the high-resolution simulations require far fewer model years to converge, saving precious computational resources.

We will include this explanation in the revised manuscript.

l231 resolved that were already resolved, consider rewording

This sentence will be removed, in favour of the new explanation about the sequential initialisation in the opening paragraph of this chapter.

l234 also/instead to also or instead of. Also, specify the place for most currently observed mass loss (Antarctica, west Antarctica)

We will rephrase this. Note that this sentence already states that most of the observed mass loss occurs at Pine Island Glacier and Thwaites Glacier.

l279 Worth giving the simulation moniker, or referencing best rather than this?

We will state which simulation performs ‘best’ earlier on, in the description of Fig. 2.

l333 How is it that it stabilises for such a long time when we would really expect observed melt rates to continue (see also general comment)?

See our response to the general comment.

l329 Here and in at least one other instance, I think better to start the paragraph with something more descriptive than introducing a figure

We will rephrase these lines.

l403 This sentence is not super clear, and could probably just be removed if not tightened

We agree that the last line of this paragraph does not add to the clarity of the whole; we will remove it.

l405 This paragraph is not really necessary to the central thesis and reads more like a commentary, but I appreciate the points being made. I just think that as it isn't mentioned in the abstract only the real heads will get around to reading it, probably still worth including

We agree that it is probably still worth including this paragraph.

l416 Specify what both refers to

We will clarify that it might not be possible for an ice-sheet model to perfectly reproduce observations of both ice geometry and velocity simultaneously.

l420 I think it is complicated, but not necessarily impossible. See for example Maier et al. (2021, <https://tc.copernicus.org/articles/15/1435/2021/>) and Jager et al. 2026 (a and b, <https://essopenarchive.org/doi/full/10.22541/essoar.177099457.70593031/v1>).

Our approach assumes a sliding law, and consequently moves the uncertainty into the bed roughness. Maier et al. do the opposite, assuming that the bed roughness (and other physical properties/processes controlling basal sliding) does not vary within the basins where they performed their statistical analysis. As this is a non-reviewed preprint, we will not reference it.

l440 I think it would be great to foreshadow this conclusion in the abstract

This is not a conclusion from the work presented here, but one cited from earlier work by Berends et al. (2023) as a plausible mechanism explaining the results presented here. As such, repeating it in the abstract does not seem appropriate.

Figures:

Fig. 2 a minor point, but UFE on its own has not been defined. As with Fig. 5, I think the radial grid needs explaining in the caption and should probably co-exist with a normal perpendicular grid.

We will add the perpendicular grid lines, and explain the intent of the radial grid in the figure caption.

Fig. 5 I think the radial grid is so that Q_{init} is visible, but I think this is confusing at first glance. I would suggest using normal perpendicular grid lines and overlying the radial grid if you feel strongly about it, and also mentioning it in the caption. Also handy to put sliding law, nudging method, and so on alongside A, B, C, ...

We will add the perpendicular grid lines. We follow the Copernicus guideline in naming the figure panels only with letters, and explaining them in the caption.

Fig. 7 I would be personally interested to see how/if each group of model differences alters the final model geometry (appreciate that Fig. 7 is grounded cells not geometry), but feel free to ignore this

While this could indeed be interesting, we see no feasible way to visualise this without using a large number of figure panels; even something like ‘mean ice thickness in 2300 between simulations with setting X’ would then result in a panel for each setting, i.e. 16 panels.

Fig. 8 Could you make a figure with all simulations where they are coloured by there Q_init values? Might be a neat illustration, but just a suggestion

We have tried that, but it doesn't really show much. As we show in Fig. 9, there is no clear relation between Q_init and the projected sea-level contribution, so colouring the sea-level curves by Q_init basically just gives you a multi-coloured mess.

Fig. 9 This figure could be improved by colour-coding the dots to specific groups (for example, Weertman-type and otherwise)

The relations between model parameter choices on the one hand, and Q_init and projected sea-level rise on the other, are already depicted in Figures 5 and 8. Adding the model parameter choices to Figure 9 as well would not add any new information, while it would require 5 extra copies of that figure.

Appendix A, nowhere is the acronym for ABUMIP (or ABUK) expanded

We will provide the definitions of these acronyms in Appendix A.