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Interactive comment on "Numerical simulations of the Cordilleran ice sheet through the last glacial cycle" *by* J. Seguinot et al.

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To A. H. Jarosch,

We thank you for this detailed review of our manuscript.

Following your comments, we have decided to amend the manuscript with a new section that explores the sensitivity of our preferred run to some of the parameters governing ice deformation and sliding. Although the discussion of model results against geological evidence is indeed extensive and partly speculative, we prefer not to shorten it for the reasons we detail below. We hope that the changes described below address your concerns.

1 General comments

Seguinot et al. present in this well written and structured manuscript a numerical modelling study of the Cordilleran ice sheet through the last glacial cycle. The model is driven by several temperature reconstructions based on proxy data and model output is subsequently compared in detail to the existing geological evidence in the region. The study is of significant relevance as it focuses on the Cordilleran ice sheet evolution in the past, which is still poorly understood.

Thank you for this positive summary of our work.

Nevertheless, the manuscript is quite unbalanced in its presentation as it focuses strongly on section 4 (Comparison with geological record) and by doing so neglects crucial details in section 2 (Model setup). This poses a fundamental challenge for understanding the science presented. If it is not quite clear what the model does and how it performs to start with, it becomes difficult to discuss the results of the modelling study and why there are mismatches with geological evidence.

The aim of this manuscript, as we see it, is to create a bridge between two scientific communities, which have long remained largely disconnected, by presenting our work in such a way that both communities will understand it. We refer here to the community of numerical ice sheet modellers on the one side, and that of glacial geologists on the other. Thus from our viewpoint as authors, the fundamental challenge you refer to is to communicate to each community using their own terminology and methods to describe the results.

Because our manuscript is aimed at both communities, it is important for us, as a team of co-authors with different backgrounds, that a balance is kept between the description of the physics embedded in the numerical model, documented in detail in many publications by PISM developers and users elsewhere (e.g., Bueler and Brown, 2009; Winkelmann et al., 2011; Martin et al., 2011; Aschwanden et al., 2013; Seguinot, 2014; Bueler and van Pelt, 2015), and the level of regional detail in the discussion of geological evidence, documented elsewhere as well though in a more fragmented way. We would be grateful if this point could be taken into account when reading on.

Yet, while trying to keep this balance, we may have indeed omitted crucial details related to the model set-up. We have made every attempt to correct this in the new version of the manuscript.

An overall sensitivity study of the parameters used in the model is completely lacking, thereby making it almost impossible to understand different responses of the ice sheet model. After reading the manuscript, one is left with the impression that the authors assume the PISM ice sheet model to be a black box which just requires one initial "correct" setup with literature values. This notion is reflected in the current manuscript, where almost all mismatches of model output with geological evidence (as discussed in section 4) are attributed to climate variations lacking in the proxy data, or climate-ice sheet feedback mechanisms not represented in the model chain. Similarly in a previous study Seguinot et al. (2014) have focused only on the driving climate sensitivities and have omitted influences of the ice sheet model as well as mass balance model parameters even though they note in that study that these sensitivities require attention as well.

As announced earlier, we have decided to add a new section to the manuscript aiming to assess the sensitivity of the model to some of the most influential ice flow parameters. Thus the new outline now includes a "Sensitivity to ice flow parameters" section and becomes:

1. Introduction
2. Model setup
3. Sensitivity to climate forcing time-series

4. Sensitivity to ice flow parameters
5. Comparison with the geologic record

In accordance, Sect. 2 (Model setup) has been amended with a description of default and alternative parameters for ice rheology and basal sliding, including illustrations of their role in the model. Sect. 2.2 (Ice thermodynamics) has therefore been expanded and divided into:

- 2.2 Ice rheology
- 2.3 Basal sliding
- 2.4 Ice shelf calving

What I advocate at this point is not a complete, strict sensitivity study of all parameters involved in the model setup (that would be probably a work package large enough to fill a science career). However several key parameters can be investigated with not too much effort. Contrasting the influence of e.g. basal sliding and ice rheology parameters with the influence of driving climate on the model results would help to estimate the overall sensitivity of the model system as well as help guiding future efforts performing such modelling studies. Implicitly the authors assume that all other model sensitivities are negligibly small in comparison to the driving climate. However it is obvious from an ice sheet model perspective that at least chosen basal sliding parameters as well as ice rheology parameters will strongly influence the shape and volume of the modelled ice sheet. Thus it would be nice to see evidence supporting the claim that driving climate is the only input to worry about being presented in the current manuscript. Or should it turn out that basal sliding and ice rheology play an important role too, as one would expect, then the relative importance of each including error estimates on the chosen parameters should be presented as well.

To keep this sensitivity study as simple as possible, we choose to present only four additional runs, two with varying rheological parameters and two with varying sliding parameters, using the simulation driven by the GRIP record as a control, at the horizontal resolution of 10 km.

In our simulations, ice deformation is governed by the constitutive law for ice (Glen, 1952; Nye, 1953),

$$\dot{\boldsymbol{\epsilon}} = A\,\tau_e^{n-1}\,\boldsymbol{\tau}\,. \tag{1}$$

where $\dot{\boldsymbol{\epsilon}}$ is the the strain-rate tensor, $\boldsymbol{\tau}$ the deviatoric stress tensor, and τ_e the equivalent stress defined by ${\tau_e}^2 = \frac{1}{2}\mathrm{tr}(\boldsymbol{\tau}^2)$. The ice softness coefficient, A, depends on ice temperature, T, pressure, p, and water content, ω, through a piece-wise Arrhenius-type law (Aschwanden et al., 2012, Eqs. 63–65),

$$A = E \cdot \begin{cases} A_c\, e^{\frac{-Q_c}{RT_{pa}}} & \text{if } T_{pa} < T_c\,, \\ A_w(1+f\omega)\, e^{\frac{-Q_w}{RT_{pa}}} & \text{if } T_{pa} \geq T_c\,, \end{cases} \tag{2}$$

where T_{pa} is the pressure-adjusted ice temperature calculated using the Clapeyron relation, $T_{pa} = T - \beta p$. $R = 8.31441\,\mathrm{J\,mol^{-1}\,K^{-1}}$ is the ideal gas constant, and A_c, A_w, Q_c and Q_w, are constant parameters corresponding to values measured below and above a critical temperature threshold $T_c = -10°\mathrm{C}$ (Paterson and Budd, 1982; Cuffey and Paterson, 2010, p. 72). The water fraction, ω, is capped at a maximum value of 0.01, above which no measurements are available (Lliboutry and Duval, 1985; Greve, 1997, Eq. 5.7). Finally, E is a non-dimensional enhancement factor which can take different values, E_{SIA}, in the Shallow Ice Approximation (SIA) and E_{SSA}, in the Shallow Shelf Approximation (SSA).

In our sensitivity study, we set constant the power-law exponent, $n = 3$, according to Cuffey and Paterson (2010, p. 55–57), the Clapeyron constant, $\beta = 7.9 \times 10^{-8}\,\mathrm{K\,Pa^{-1}}$, according to (Lüthi et al., 2002), the water fraction coefficient, $f = 181.25$, according to

Lliboutry and Duval (1985), and the SSA enhancement factor, $E_{SSA} = 1$, according to Cuffey and Paterson (2010, p. 77).

On the other hand, we test different values for the two creep parameters, A_c and A_w, the two activation energies, Q_c and Q_w, and the SIA enhancement factor, E_{SIA}, as follow.

- Our *default* configuration used in the control run and all other simulations in the manuscript include rheological parameters, A_c, A_w, Q_c and Q_w, derived from Paterson and Budd (1982) and given in Bueler and Brown (2009, Eqn. 5), and $E_{SIA} = 1$.
- Our *hard ice* configuration include rheological parameters, A_c, A_w, Q_c and Q_w, derived from Cuffey and Paterson (2010, p. 72 and 76), and $E_{SIA} = 1$, which correspond to a stiffer rheology than that used in the control run.
- Our *soft ice* configuration include rheological parameters from Cuffey and Paterson (2010), and $E_{SIA} = 5$, the recommended value for ice age polar ice (Cuffey and Paterson, 2010, p. 77).

An additional simulation using the ice rheology from Cuffey and Paterson (2010) and $E_{SIA} = 2$, the recommended value for Holocene polar ice (Cuffey and Paterson, 2010, p. 77) was performed, but its results were very similar to that of our default run, thus we decided to not present it here.

Actual parameter values for A_c, A_w, Q_c, Q_w and E_{SIA} used in our simulations are given in Table 1, while the effect of the three different parametrisations on temperature-dependent ice softness, A, is illustrated in Fig. 1.

In our simulations, basal sliding is governed by a pseudo-plastic sliding law, already given in the manuscript but recalled here for the sake of completeness,

$$\boldsymbol{\tau}_b = -\tau_c \frac{\boldsymbol{v}_b}{{v_{th}}^q \, |\boldsymbol{v}_b|^{1-q}} \,, \tag{3}$$

which relates the bed-parallel shear stresses, $\boldsymbol{\tau}_b$, to the sliding velocity, $\boldsymbol{v}_b$. The yield stress, τ_c, is modelled using the Mohr–Coulomb criterion,

$$\tau_c = c_0 + N \, \tan\phi \,, \tag{4}$$

where cohesion, c_0, is assumed to be zero. Effective pressure, N, is related to the overburden pressure, $P_0 = \rho g h$, and the modelled amount of subglacial water, using a formula derived from laboratory experiments with till extracted from the base of Ice Stream B in West Antarctica (Tulaczyk et al., 2000; Bueler and van Pelt, 2015, Eqn. 23),

$$N = \delta P_0 \, 10^{(e_0/C_c)(1-(W/W_{max}))} \,, \tag{5}$$

where δ sets the minimum ratio between the effective and overburden pressures, e_0 is a measured reference void ratio and C_c is a measured compressibility coefficient. The amount of water at the base, W, varies from zero to W_{max}, a threshold above which additional melt water is assumed to drain off instantaneously.

In our sensitivity test, we set constant the pseudo-plastic sliding exponent, $q = 0.25$, and the threshold velocity, $v_{th} = 100\,\mathrm{m\,a^{-1}}$, according to values used by Aschwanden et al. (2013), the till cohesion, $c_0 = 0$, whose measured values are consistently negligible (Tulaczyk et al., 2000; Cuffey and Paterson, 2010, p. 268), the till reference void ratio, $e_0 = 0.69$, and the till compressibility coefficient, $C_c = 0.12$, according to the only measurements available to our knowledge, published by (Tulaczyk et al., 2000).

We also use a constant spatial distribution of the till friction angle, ϕ, whose values vary from 15 to 45° as a piecewise-linear function of modern bed elevation, with the lowest value occurring below modern sea level (0 m above sea level, m a.s.l.) and

the highest value occurring above the generalised elevation of the highest shorelines (200 m a.s.l., Clague, 1981, Fig. 5). This range of values span over the range of measured values for glacial till of 18 to 40° (Cuffey and Paterson, 2010, p. 268). It accounts for frictional basal conditions associated with discontinuous till cover at high elevations, and a weakening of till associated with the presence of marine sediments at low elevations (cf. Martin et al., 2011; Aschwanden et al., 2013, supplement; the PISM authors, 2015).

An additional simulation with a constant till friction angle, $\phi = 30^\circ$, corresponding to the average value in Cuffey and Paterson (2010, p. 268), was actually performed, but the induced variability was small as compared to that which will be presented here, and therefore we decided to not include this run in our sensitivity study.

On the other hand, we test different values for the minimum ratio between the effective and overburden pressures, δ, and the maximum water height in the till, W_{max}, as follow.

- Our *default* configuration used in the control run and all other simulations in the manuscript include $\delta = 0.02$ and $W_{max} = 2$ m as in Bueler and van Pelt (2015).
- Our *soft bed* configuration use $\delta = 0.01$ and $W_{max} = 1$ m.
- Our *hard bed* configuration use $\delta = 0.05$ and $W_{max} = 5$ m.

The effect of the three different parametrisations on the effective pressure on the till, N, in response to water content, W, is illustrated in Fig. 2. All parameter choices are listed in Table 1.

Finally, we adjusted the GRIP linear scaling factor for each run, so that they result in a similar glaciated area at the Last Glacial Maximum (LGM) to that modelled with the default configuration (Table 1). In other words, the sensitivity in modelled ice sheet extent at MIS 2 is expressed through the scaling factor required to obtain a fixed target area, since we consider this scaling factor as a free parameter in our study.

By analogy to our manuscript's Table 3 (ice volume and extent extrema) and Fig. 3 (sea-level relevant ice volume time series), the results of our sensitivity study are presented here using a similar layout in Table 2 and Fig. 3.

As a result of the different scaling factors applied, the resulting simulations show very little difference in the modelled glaciated areas corresponding to maximum ice volumes during MIS 2, but also during MIS 4 (Table 2). However, such can not be said of the modelled glaciated area corresponding to minimum ice volume during MIS 3. In fact, the extent of the remnant ice cap which persists over the Skeena Mountains during this stage shows a significant sensitivity to ice rheology of 31%, and an even more important sensitivity to basal sliding parameters of 62% (Table 2).

The modelled sea-level relevant ice volumes show more variability than the modelled glaciated areas (Table 2, Fig. 3). As one could expect, softer ice and weaker till both result in a thinner ice sheet, while harder ice and stronger till result in a thicker ice sheet. For instance, peak ice volume during the MIS 2 (LGM) varies by 30% between the two parametrisations of ice rheology used, and by 21% between the two parametrisations of basal sliding used. The differences in sea-level relevant ice volume are greatest during the MIS 3 (Table 2, Fig. 3) where both the areal and thickness contributions add up.

All the information detailed above, including Eqns 1 and 2, default parameter values, Tables 1 and 2, and Figs. 1–3 have been included in Sect. 2 (Model setup) and in the new section (Sensitivity to ice flow parameters). Relevant discussion points in Sect. 5 (Comparison with the geologic record) have been revised to account for these new results.

Generally section 4 appears to be quite long and seems to re-summarize known geological evidence for the region. At times the language is quite speculative, for example P4162 L1 and 7, P4164 L18, P4171 L7, L9, L17, L19 and L20 and so forth. I would recommend to shorten that section to focus only on the geological evidence which can or

can not clearly be reproduced by the presented model and avoid extensive speculation on what the reasons for mismatch are, especially in the present form of the manuscript, where a sensitivity study of the model itself is completely missing. However I leave the choice of how much geological evidence is discussed in the manuscript entirely up to the authors.

Our choice of language on pages P4162 L1 and 7 (discussion of a longitudinal shift between the modelled and geological reconstructed eastern margin and, in turn, the position of the main ice divide), P4164 L18 (discussion of the confined modelled ice extent on the Alaska Range north slope in agreement with the regional reconstructions), and P4171 L7, L9, L17, L19 and L20 (discussion of the modelled flow patterns and the observed lineation pattern on the Interior Plateau of British Columbia) is intentional. So is our choice to discuss some aspects of the model results that are not unequivocal, as a result of uncertain model parameters or, more often, due to processes not included in the model.

Our rationale for including this “extensive speculation” takes into account the fact that glacial geology is no exact science. In fact, it often has to be speculative and to include a large part of interpretation in order to reach conclusions that are often uncertain but without which the field could not move forward. Because we expect that part of our readership will consist of glacial geologists who have sometimes spent an entire scientific career on studying some aspects of the Cordilleran ice sheet (cf. comment by A. Stumpf in this discussion), we chose to address some of the long-standing debates on Cordilleran glacial geology, such as the location of ice divides or the enigmatic lineation pattern on the Interior Plateau.

This is not only to prevent an over-interpretation of our model results by readers with little or no background in ice sheet modelling, but also to hint at potential model improvements that may be needed to approach these debates through numerical modelling in the future. We believe that such discussion, though speculative, is pertinent, and, consequently, have not shortened this section.

2 Specific Comments

I refer to text locations in the discussion paper by page number (P) and line number range (L) for the specific comments.

P4151 L11-16: *In this sentence the authors refer back to their previous work (Seguinot et al., 2014) and highlight that the NARR temperature and precipitation fields are the most suitable present day climate datasets to be used. Especially since the NARR precipitation fields include steep precipitation gradients which are required as identified by et Seguinot et al. (2014). NARR is delivered on a 32 km Lambert grid, and thus it is questionable how "steep" these gradients can be, given the rather smooth representation of the existing topography on a 32 km grid. Seguinot et al. (2014) have partly discussed that however. NARR precipitation and temperature fields have been evaluated in detail based on available station data for large parts of the study domain dealt with in this manuscript. This evaluation (Jarosch et al., 2012) demonstrated that NARR has difficulties simulating orographic processes in the Coast Mountains which in turn results in unrealistic atmospheric conditions over the Rocky Mountains. Jarosch et al. (2012) further concluded that physics based downscaling is required to adequately drive glacier models in that region. The authors should argue in more detail here why they think that NARR precipitation fields at 32 km are adequate to drive their model and reflect their arguments with the findings of Jarosch et al. (2012). A solid argument here is of special importance as the authors assume the present day precipitation fields to be valid throughout their model time period (120 ky to present) without further corrections (cf. section 2.4 equation 6).*

We admit that the NARR climate forcing used in our simulations has its limitations.

In our previous study (Seguinot et al., 2014), we have evaluated the performance of NARR in forcing constant-climate simulations of the Cordilleran ice sheet against that of an observation-based data set (Hijmans et al., 2005, WorldClim). Indeed, the use of

NARR in these simulations produced slightly different patterns of glaciation relative to WorldClim, including more extensive ice cover on the Columbia and Rocky mountains (Seguinot et al., 2014, Figs. 6–7). Our simulations have shown that these differences are mainly caused by disparities in the precipitation fields of the two data sets (Seguinot et al., 2014, Figs.13–14), which may, indeed, be related to the fact that the NARR has difficulties with simulating orographic processes in some areas of steep topography as discussed by Jarosch et al. (2012).

In the light of your comments, we have introduced a discussion of the NARR and its limitations in Sect. 2.6 (Climate forcing) and in Sect. 5.1.2 (Ice configuration during MIS 2). We acknowledge that the implementation of the downscaling method presented by Jarosch et al. (2012) may address these limitations. However, extending this downscaling method to the entire the model domain used in our study could be challenging, because the northern part of the model domain is characterized by stronger precipitation gradients and includes much fewer weather stations than the computational domain of Jarosch et al. (2012).

P4152 L11: *Basal topography is "derived" from ETOPO1 data. What does this mean? Do the authors just re-sample the DEM data to their 10 km and 5 km model grids (P4152 L21-22) or is there more processing done? The ETOPO1 data contains the present day ice volumes within the study region. Clarke et al (2013) have estimated the ice volume in parts of that region to be $2530 \pm 220\,km^2$, with maximum ice thicknesses up to 200 m. It can be argued that the volume is negligible in this study and the authors should do so if they think this is appropriate, but I wonder about the ice thicknesses. Assuming that the authors did not remove the present day ice cover, basal topography could be up to 200 m higher that it actually is in reality. Given their used temperature lapse rate of $6\,K\,km^{-1}$ (P4157 L1), parts of the Cordilleran ice sheet growing in those regions with 200 m too high topography would experience a 1.2 K colder atmosphere than it actually should in reality. This favours unrealistic ice growth and thus the omission of present day ice cover removal should be clearly argued for in the* *manuscript.*

By "derived" we meant that the ETOPO1 data is simply re-sampled with linear interpolation from the original to the projected grid. We have clarified this in the methods.

Thank you for pointing out the study by Clarke et al. (2013). Indeed, the ETOPO1 data contains present day ice volumes. The most problematic part of the model domain in this respect is by far that of the Wrangell and St.-Elias mountains where ice thicknesses up to 1200 m have recently been measured by low-frequency radar (Rignot et al., 2013). In this area, located over the USA Canada border and just north of 60°N and thus not included in the study by Clarke et al. (2013), temperate ice, surge dynamics and deep subglacial depressions in the icefield interior pose a fundamental challenge to reconstructing basal topography for the entire ice cap. Although it is clear that our model overestimates ice thickness in this region, we are not aware of bed topography data or reconstructions that could be used to force the ice sheet model. This said, with the exception of the Wrangell and St.-Elias mountains ice field, present-day ice volumes are small relative to the ice volumes concerned in our study.

We have added a description of the limitations of ETOPO1 data in the methods, and a comment on overestimated ice thicknesses in the Wrangell and St.-Elias mountains in Sect. 5.2.2 (Major ice-dispersal centres).

P4153 L2-3: *That the "shallow shelf approximation" (SSA) is used as a "sliding law" for the "shallow ice approximation" (SIA) is a confusing statement in this context. Bueler has coined the term in his 2009 paper as cited in the manuscript. However the casual reader will be confused at this point, especially since the authors state the pseudoplastic sliding law the model actually uses in equation 1. I would recommend to leave out the statement on the SSA being the "sliding law" for the SIA.*

We agree and have reformulated this statement. Thank you for spotting this.

P4153 L5-6: *As stated here, ice rheology within the used ice sheet model is based on Aschwanden et al. (2012). This enthalpy based formulation has proofed itself to be very suitable for estimating ice rheology in ice sheet models, but it also depends on several parameters to translate enthalpy within the ice to ice viscosity (Aschwanden see et al., 2012, equations 62-65). The authors do not mention any of these parameters (e.g. any of the rate factors or nonlinear power n) within the manuscript or in Table 1. I have mentioned above in the general comments section that parameters used in ice rheology and basal sliding formulations are important model parameters which will influence the ice sheet model output and that a basic sensitivity study on those parameters is required to understand the model results. Here the authors could start with listing the parameters used in the ice rheology formulation, than continue with estimating uncertainties for those from literature and afterwards perform additional model simulations to identify the influence of the chosen parameter sets on the ice volume and ice margin position history the model creates. In the end the authors will be able to identify the relative importance of uncertainties in driving climate as well as model parameters, which will strengthen their discussion in section 4.*

Our changes described above partly address this comment. To make our parameter choices clear, we have included all default parameter values mentioned in this response in the manuscript's Table 1 (with the alternative values used in the sensitivity tests being given in a separate table).

P4153 L8: *It is not clear where the geothermal heat flux boundary is located. Does the "depth of 3 km" refer to a depth measured from the ice surface, which would not make much sense for a ice thickness evolving ice sheet model or is it measured from the ice-bedrock interface downward. In that case the term "computed subglacially" is confusing as it refers to the ice-bedrock interface. Please be more specific here.*

The "depth of 3 km" is measured the ice-bedrock interface downward. This is where we apply geothermal heat flux as the lower boundary condition to the bedrock thermal model. By "computed subglacially" we mean below the ice-bedrock interface, into the bedrock. We have clarified these two points in the text.

P4153 L16 - P4154 L0: *Here the authors describe the basal sliding setup in their model. However they do not explain how they came up with the parameters used in equations 1-3 that are listed in Table 1 (part on "basal sliding"). What motivates these parameter choices (references?) and how sensitive is the model and its results to these choices? Both question come instantly to mind and need to be addressed in detail. Here a basic sensitivity study on how basal sliding parameters in the model control the outcome discussed in section 4 is in order and I strongly recommend to include one in the manuscript. The authors can start by estimating the uncertainties in the chosen basal sliding parameters and run two extra simulation runs with their preferred climate forcing and the end member values of the uncertainties. This would create the most simple sensitivity study with respect to basal sliding, but would be extremely helpful for the argument made above in my general comments.*

Our changes described above partly address this comment. We have added a new column to Table 1 containing references motivating default parameter choices.

P4156 L3-5: *In addition to what I have stated above on the NARR precipitation fields and their suitability, it is important to state at this location in the manuscript how the 32 km NARR data is translated to the 10 and 5 km computational grids of the current study. I disagree with the notion that a 32 km precipitation field can be called "high-resolution" in the context of 10 and 5 km grid based ice sheet modelling. The input data is either 3 or 6 times coarser than the numerical grid, thus not at all high-resolution. Seguinot et al. (2014) state in their section 3.3 that the NARR data fields have been bilinearly interpolated to 10 km resolution in their work. Did the authors do the same here for their 10 and 5 km working grids? This is crucial information to be included in the manuscript. It has been demonstrated by spectral power analysis (Jarosch et al.,*

2012) that the NARR precipitation fields do not contain any significant spacial information below approximately 39 km resolution and that bilinear interpolation does not add any information whatsoever on smaller scales, which should come to no surprise. Physics-based downscaling techniques however are able to add spatial information to precipitation fields down to about 1 km grid sizes (Jarosch et al., 2012). Taking these findings into the current context of the manuscript at hand, the NARR precipitation fields can hardly be called "high resolution" with their effective precipitation grid size of 39 km. The authors should argue for their choice of not performing any downscaling whatsoever to their computational grids of 10 and 5 km for precipitation and temperature and discuss their choice in the light of the findings from Jarosch et al. (2012). Temperature however is better constrained in NARR (Jarosch et al., 2012) and contains spectral information down to 10 km resolution, which justifies the usage of NARR temperature fields on the 10 km computational grid of this study. The 5 km grid still needs to be argued for.

As in Seguinot et al. (2014), the NARR data fields have been bilinearly interpolated from the NARR 32 km Lambert grid to the computational domain grids. We apologize for omitting this information and have now added it to the manuscript.

We called the NARR data "high resolution" because its spatial resolution is higher than most other atmospheric reanalyses. But we agree that it is still too coarse for ice flow modelling and that temperature and precipitation downscaling techniques could potentially correct for the errors caused by the resolution gap. We have rephrased the sentence to highlight this.

P4157 L1: *How is a fixed temperature lapse rate justified for simulations over 120k years, when there is ample published evidence that temperature lapse rates vary significantly within space and time? I am sure that the choice of γ in this study has a significant influence on the model outcome and I leave it to the authors to explore this possibility.*

A fixed temperature lapse rate, $\gamma = 6\,^{\circ}$C, is certainly another coarse approximation in our model setup. Although it is clear that γ varies spatially, seasonally, and varied in the past with glacial fluctuations, including these variations in the model setup would require to introduce new degrees of freedom in the study, which we have been trying to avoid.

3 Technical Corrections

P4161 L10: *"further analysis further;" maybe change to "further analysis" or "further analysis here".*

P4166 L13: *double "the" in the sentence.*

Thank you for spotting these two typos. We have corrected them.

I hope the authors find my comments helpful in revising their manuscript and wish them success for their future endeavours.

Thank you very much. We think that the manuscript is now stronger indeed.

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Table 1. Parameter values used in the sensitivity test.

Config.	Rheology					Sliding		GRIP scaling	
	A_c (Pa^{-3} s^{-1})	A_w	Q_c (J mol^{-1})	Q_w	E_{SIA}	δ	W_{max} (m)	f	$T_{[32,22]}$
Default[1]	3.61×10^{-13}	1.73×10^{3}	60×10^{3}	139×10^{3}	1	0.02	2	0.38	6.2
Soft ice[2]	2.847×10^{-13}	2.356×10^{-2}	60×10^{3}	115×10^{3}	5	0.02	2	0.40	6.6
Hard ice[2]	2.847×10^{-13}	2.356×10^{-2}	60×10^{3}	115×10^{3}	1	0.02	2	0.37	6.0
Soft bed	3.61×10^{-13}	1.73×10^{3}	60×10^{3}	139×10^{3}	1	0.01	1	0.40	6.5
Hard bed	3.61×10^{-13}	1.73×10^{3}	60×10^{3}	139×10^{3}	1	0.05	5	0.36	5.9

After [1]Paterson and Budd (1982); Bueler and van Pelt (2015); and [2]Cuffey and Paterson (2010).

Table 2. Extremes in Cordilleran ice sheet volume and extent corresponding to MIS 4, 3 and 2 using the GRIP paleo-climate forcing with each parameter configuration (Fig. 3). Relative differences (R. diff.) give rough error estimates related to varying selected ice rheology and basal sliding parameters (Table 1).

	Age (ka)			Ice extent (10^6 km^2)			Ice volume (m s.l.e.)		
Config.	MIS 4	MIS 3	MIS 2	MIS 4	MIS 3	MIS 2	MIS 4	MIS 3	MIS 2
Default	57.59	42.91	19.14	1.93	0.67	2.09	7.43	1.54	8.62
Soft ice	58.89	49.97	21.57	1.96	0.54	2.08	6.58	1.03	6.88
Hard ice	57.32	42.90	19.14	1.90	0.75	2.12	7.83	1.91	9.46
R. diff.	3%	16%	13%	3%	31%	2%	17%	57%	30%
Soft bed	58.90	49.21	19.53	1.88	0.55	2.05	6.46	1.03	7.52
Hard bed	57.31	42.91	19.14	1.93	0.96	2.13	7.99	2.89	9.31
R. diff.	3%	15%	2%	3%	62%	4%	21%	120%	21%

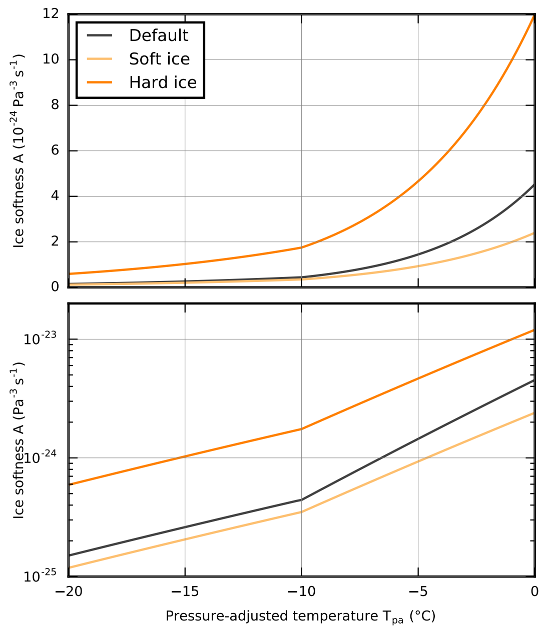

Fig. 1. Ice softness as a function of pressure-adjusted temperature for the default, hard ice, and soft ice rheologies, using a linear scale (top panel) and a logarithmic scale (bottom panel).

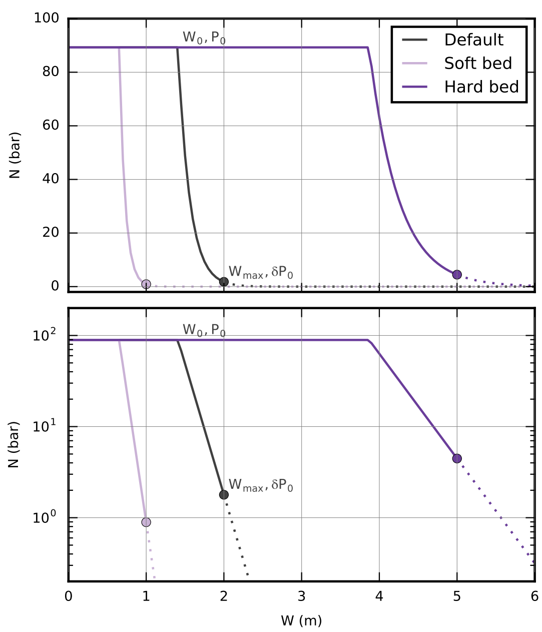

Fig. 2. Effective pressure as a function of water content in the till for the default, hard bed, and soft bed sliding parametrisations, for an ice thickness of 1000 m.

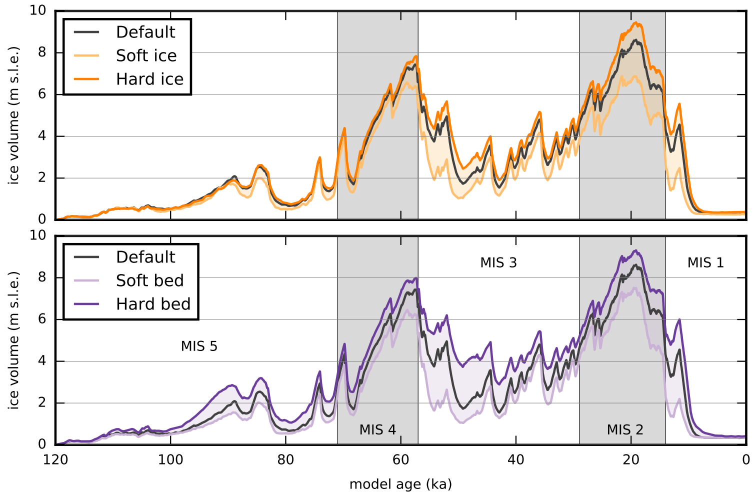

Fig. 3. Modelled sea-level relevant ice volume using default parameters (black curves), different ice rheology parameters (top panel), and different basal sliding parameters (bottom panel).