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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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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.

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.

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.

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

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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.

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.

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 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 (120ky to present) without further corrections (cf. section 2.4 equation 6).

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.

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.

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 (see Aschwanden 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.

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.

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.

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

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temperature fields on the 10 km computational grid of this study. The 5 km grid still needs to be argued for.

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.

3 Technical Corrections

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

P4166 L13: double "the" in the sentence.

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

Kind regards,
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