Author Response to Reviewer #2

An assessment of basal melt parameterisations for Antarctic ice shelves

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RC: Reviewer Comment, AR: Author Response, *changed manuscript text*

RC: **Reviewer Summary:**
For this first time, this paper performs thorough evaluation of a large number of ice-shelf melt parameterizations in a pan-Antarctic context. This is a topic of significant importance to the community, as melt parameterizations allow for Antarctic ice-sheet modeling on time scales that are not currently possible using ice-sheet models coupled to ocean or Earth system models. A number of aspects of this work are novel. First and most importantly, the evaluation is performed on a large number (36) of Antarctic ice-shelves that are fully representative of the continent and which use realistic topography and climate forcing. Second, the rich NEMO model ensemble provides over 500 years of cumulative simulation results, a far more robust dataset than current observations are able to provide. Third, new variants of melt parameterizations (including various ways of including ice-shelf slope dependence and a new approach to plume parameterization) are introduced for inclusion in the evaluation. Fourth, the paper carefully evaluates each parameterization for continental-scale "biases" (relative to the NEMO model), "biases" for individual ice shelves, melt patterns (with an emphasis on grounding lines) and the behavior of parameterizations at high resolution. Finally, the paper includes invaluable recommendations on which parameterizations show the most promise (in a few different categories) and the caveats and best practices for using each, suggested by the evaluation.
The paper is well written and arranged. The figures are quite effective, well crafted, and easy to follow. They give the reader a good, intuitive sense of the relative strengths and weaknesses of the different parameterizations in a variety of ways. The numerous tables will help future users of these parameterizations to know the correct coefficient values to use for each, including recommended values of the tunable parameters that best fit the NEMO simulation results.

AR: Thank you very much for your constructive and very detailed comments on how to further improve our paper. We plan to address you comments as described in the following.

RC: ***Major comments***

RC: I do have a few major concerns about the methodology, and addressing these concerns may entail a significant effort by the authors. For that, I am sorry. But I also feel a duty to be particularly careful in evaluating the methodology and suggesting improvements because of the way that the paper is framed: it aims to provide recommended best practices, including preferred coefficients for use in pan-Antarctic modeling using the evaluated parameterizations.

AR: We appreciate your feedback on the methodology. We agree that this paper recommends best practices and needs to be robust. We thank you for raising concerns and plan to address them as follows.

RC: The first concern is one I share with the other reviewer. It would be preferable to tune the parameterizations with one subset of the available data and evaluate it with another, independent subset. Given the more than 500 years of simulation data, I agree that this would seem to be fairly easy to do by using a random subset of years for tuning and the remainder for evaluation. It may be worth adopting this approach in revising the paper.

AR: We agree that splitting the data into a "tuning" and "evaluation" dataset is worth adopting during revision. However, we want to clarify that we do not have 500 simulation years (as suggested by the reviewer), but "only" 127 years. These 127 years are a result of the accumulation of simulation years from four simulations spanning 30 to 40 years (see Table 1 in the manuscript).
For the revision, as suggested by both reviewers, we will conduct a cross-validation of the parameterisation. This means that we will conduct the tuning and evaluation several times on different periods to robustly estimate the generalisation performance of each parameterisation. To avoid autocorrelation influencing this cross-validation, we will divide the data into 10-year chunks, as the autocorrelation is typically 2 to 3 years in our input temperatures.

RC: The second major concern is that the metrics used in the paper for evaluating the root-mean-squared "error" (RMSE) relative to the NEMO results, Eqs. (32) and (33), do not seem robust to me, nor are these definitions the standard ones used in Earth system modeling. This is especially unfortunate because the vast majority of the results shown in the paper (from section 2.4 onward) are dependent on these metrics, including the process for identifying the best-fit parameters, the evaluation of parameterizations on the continental scale (both over each ice shelf and near grounding lines), and the methodology for evaluating parameter uncertainty. I am confident that the adoption of a more standard metric is unlikely to change the broad conclusions of the paper but it is hard to see how it would not

change specific results such as the recommended values of coefficients used in the parameterizations. For this reason, I ask the authors to strongly consider redoing their analysis with a more robust and widely accepted version of the RMSE metric.

AR: We deliberately chose non standard metrics instead of the standard melt rate RMSE, but your bad appreciation of our metrics shows that we need to improve the justification for these metrics and provide evidence for their robustness. In short, we want to tune and evaluate the parameterisation in a way that gives the most interesting results for an application in either an ice sheet or an ocean model (or possibly at the interface between both). In the following, we address your points and comments and hope that the more detailed explanation of our choices will convince you.

RC: In my own work and I think in climate modeling broadly, we always use the area-weighted variance to compute the RMSE as follows:

$$RMSE = \sqrt{\frac{\sum_{i=1}^{N}(m_i - m_{o,i})^2 A_i}{\sum_{i=1}^{N} A_i}} \tag{1}$$

where, in the context of this paper, m$_i$ is the melt rate (in whatever units you choose) from the parameterization for sample i (which might be one location and year); m$_{o,i}$ is the "observed" melt rate from NEMO (at the same location and year); A$_i$ is the area associated with the ith sample (this could be a whole ice shelf or an individual NEMO grid cell, depending on how the evaluation is performed); and N is the number of sample points. This metric is robust to different ways of sampling the evaluation points. It works just as well to evaluate the "error" over a single ice shelf as for any number of ice shelves aggregated together into a region. The RMSE values for individual ice shelves can be computed by sampling the pattern at locations across the ice shelf, and then these RMSE values can be combined to get regional- and continental-scale values of the RMSE. This approach is also robust in the sense that including or excluding any number of small ice shelves would have a small impact on the RMSE.

Unfortunately, the same is not true for either of the RMSE definitions, Eqs. (32) and (33), proposed in the paper. Instead of weighting by area, these metrics treat each ice shelf (independent of its area) as having equal weight. This approach has at least three problems. First, the metrics are significantly more sensitive to the number of ice shelves under consideration. Considering Eq. (32) first, repeated here:

$$RMSE_{\text{int}} = \sqrt{\frac{\sum_{i}^{N_{\text{isf}}} \sum_{t}^{N_{\text{years}}} (M_{\text{param}}[i,t] - M_{\text{ref}}[i,t])^2}{N_{\text{isf}} N_{\text{years}}}} \tag{2}$$

if we were to add several ice shelves with both negligible bias in melt flux and negligible area, the RMSE$_{\mathbf{int}}$ would decrease (because we aren't adding significantly to the numerator but we are adding new ice shelves to the denominator). Now, considering Eq. (33):

$$RMSE_{\text{GL}} = \sqrt{\frac{\sum_{i}^{N_{\text{isf}}} \sum_{n}^{N_{\text{simu}}} (m_{\text{GL,param}}[i,n] - m_{\text{GL, ref}}[i,n])^2}{N_{\text{isf}} N_{\text{simu}}}} \tag{3}$$

In this case, if we were to add several tiny ice shelves with negligible area but significant biases in melt rate, they could significantly increase the RMSE$_{GL}$. In this way, these metrics are likely fairly sensitive to the number of ice shelves (36) chosen for the study.

AR: We plan to address the two points raised as follows:

Tuning and evaluating at the grid cell level

We are aware that the most common way to compute the RMSE in climate modeling is to use the area-weighted variance taking into account each grid cell (as you showed in Eq.(1)). However, very early in our study, we decided that tuning and evaluating at the grid-cell level would not yield the best compromise for the tuning result and for the conclusion drawn from the evaluation. There are several reasons that motivated this choice:

1. The standard RMSE would be highly biased towards Filchner-Ronne and Ross ice shelves, which cover a much larger area (many more grid cells) than the others. As a comparison here are the areas of Ross and Filchner-Ronne compared to the third largest ice shelf Amery or to an important ice shelf like Pine Island: Ross = 4.7×10^5 km^2, Filchner-Ronne = 4.2×10^5 km^2, Amery = 5.9×10^4 km^2, Pine Island = 5.7×10^3 km^2. We do not think that tuning on the grid cell level is the most interesting way to go, because Filchner-Ronne and Ross are not necessarily the ice shelves that (1) affect most near-future ice dynamics (Seroussi et al., 2020) or (2) contribute the most to the present-day meltwater release into the ocean (Rignot et al., 2013).

2. The standard RMSE would give the same importance to all grid cells although we know that there are regions that are more or less important for buttressing and therefore for the influence of melt on the ice-sheet evolution (Reese et al., 2018). Many grid cells of Ross and Filchner-Ronne as well as other smaller ice shelves are "passive" and can therefore suffer from acceptable biases that will not significantly affect the ice dynamics. In terms of impacts on the ocean circulation, we believe that getting a correct freshwater budget around Antarctica (i.e. cavity integrated melt) is a priority before getting the correct depth distribution of the freshwater release at a given location.

3. We consider that the melt parameterisations we tune and evaluate are too simple to reproduce all the details of the spatial melt patterns. If they can reproduce the main pattern (e.g. more melt near the grounding line) but the pattern is shifted a little in space, this will result in a high RMSE, penalising a parameterisation that could reproduce some of the complexities of the melt patterns but not at the exact correct location.

4. We only consider relatively large ice shelves (those that are well resolved by NEMO) so there is no issue with the number of very small ice shelves that matter for neither ice nor ocean dynamics. This is an important aspect of our metrics that we will emphasise more in the revised manuscript.

In conclusion, we would like to stay with our decision of tuning toward an integrated metric, but we will improve the part of our manuscript explaining our choice.

Weighting the RMSE for tuning
Our RMSE based on integrated values gives the same weight for two ice shelves with the same integrated melt irrespectively of their size, buttressing effect on ice dynamics, or effect on ocean convection. This is an empirical choice, and we propose to add a discussion about this choice and the associated robustness of our model selection.
Weighting the RMSE with the ice-shelf area results in the same problem as raised for the tuning on the grid-cell level: the tuning will be particularly biased towards Filchner-Ronne and Ross ice shelves, which cover a much larger area than the others, although they are not necessarily the most "interesting" ones for the reasons mentioned above. Instead, the integrated melt implicitly contains the importance of the area for the melt and therefore influences the tuning when it is relevant for the integrated melt, for example if a high area is the main driver behind a high integrated melt.

RC: Second, the metric in Eq. (2) here gives significantly more weight to ice shelves with large areas (since $\mathbf{M}_i$ = $\mathbf{m}_i$ $\mathbf{A}_i$, the numerator in this equation is getting weighted by $\mathbf{A}_i^2$), while the metric in Eq. (3) here gives significantly less weight to large ice shelves (because $\mathbf{A}_i$ does not appear in the numerator at all).

AR: To assess the parameterisations, we chose to evaluate different aspects of the resulting melt. With the integrated melt, we evaluated the ability of the parameterisations to reproduce the time variability and the integrated value of the melt for a given cavity. With the mean melt rate near the grounding line, we aimed to assess if the parameterisation reproduced the mean pattern well, i.e. if the mean melt in the grid cells near the grounding line was on the order of the reference. By averaging the pattern over time and space, we aimed to avoid that this part of the evaluation was biased due to mismatch in time variability (already evaluated before) and slightly shifted patterns in the vicinity of the grounding line. We will explain the procedure and choices more thoroughly in the revised manuscript to clarify.

RC: **Third, the definition of an ice shelf is not particularly robust. The Filchner and Ronne Ice Shelves are sometimes considered distinct (as in this paper) and sometimes are combined together. Similarly, the Eastern and Western Ross are sometimes considered separately (though Ross is a single ice shelf here). As grounding lines move, ice shelves that were once distinct may merge, or a single ice shelf may be divided into two. The RMSE metrics from the paper are sensitive to the authors' definition of the 36 ice shelves used in the paper, whereas the metric in Eq. (1) here would not change if an ice shelf were divided into 2.**

AR: We agree that the choice of division of ice shelves is somewhat subjective. From an oceanographic point of view, we agree that combining Filchner and Ronne as one ice shelf makes more sense. This is what we will use in the new procedure.

RC: **In summary, I do not think robust statistics can be performed when treating ice shelves as distinct entities given equal weight regardless of size. I do not see how a meaningful RMSE in the integrated melt flux can be computed between ice shelves: it would only be meaningful if totalled over all ice shelves in a region (but averaged over samples in time and across ensembles).**

AR: We hope that the responses to the previous comments have addressed this issue of robust statistics. We would like to emphasize that our metrics were not defined for a universal use, but are rather carefully designed for the specific purpose and datasets of our study. We nonetheless think that our results are robust in the sense that our recommendations are not overly sensitive to our empirical choices.

RC: **My preference would be that the results of the paper be re-evaluated with the metric in Eq. (1) here instead of Eqs. (32) and (33) in the paper. I realize this is a monumental task. If the authors do not feel that this is necessary, I am willing to hear you out. But I would need to see a strong case for why the existing metrics are more robust and defensible than I currently believe.**
I want to reiterate that I think this is amazing work! If one takes the metrics for evaluating "biases" as given, the rest of the results follow nicely: the evaluation of each parameterization is remarkably thorough and well executed. The service that this work does for the community is truly commendable, particularly because it gives future model developers not only the tools to make an informed choice among the existing parameterizations but also a good choice of coefficients to use.

AR: We thank the reviewer for the detailed description of their concerns. After consideration, we sincerely believe that our initial choice is more appropriate for the aforementioned reasons. As a consequence, we will re-run the tuning and evaluation, taking into account that we now combine Filchner-Ronne as one ice shelf and that we will conduct several iterations of the tuning and evaluation on different chunks of time, but we will keep the initial metrics.

RC: Below, I have a number of specific comments and suggestions for typographical or grammatical changes. Please do not be intimidated by the number of comments. I have been told by editors in the past that I seem to review papers as if I were a coauthor, which may be true.

AR: We thank you for the thoroughness of the comments. We deeply appreciate the time you have put into this review.

RC: ***Specific comments***

RC: l. 7-8: "The box, PICOP parameterization and quadratic parameterizations with slope dependency..." In the context of the abstract (in which these different parameterizations have not yet been introduced), I found it hard to understand what was being described here. The previous sentence does a nice job of introducing what the quadratic parameterization means and what the slope dependence is about. A similar introduction for the box parameterization and PICOP here would be helpful.

AR: We understand the concern and will clarify this part of the abstract.

RC: l. 19-20: "...interacting with land or shallow rocks...": I'm not sure I follow what you mean by interacting with shallow rocks. Maybe take this out?

AR: We will take this out.

RC: l. 38: "...and do not resolve ice-shelf cavities...": Should this be "include" instead of "resolve"? It seems like you are referring to model configurations that don't have the cavities at all, rather than that they are there but at too coarse a resolution.

AR: Thank you for pointing this out. We agree and will reformulate as suggested.

RC: l. 66-69: When you refer to a "step" here, does each of these correspond to a section of the paper? If so, it might be clearer to replace "step" with "section".

AR: The first step is one section but the second and third step refer to subsections of the results. We will change to "section" and "subsection".

RC: **l. 115 "The top boundary-layer thickness is set to 30 m." This likely needs a little more explanation: this is referring to the boundary-layer thickness over which temperature and salinity are averaged for use in the parameterization of sub-ice-shelf turbulent fluxes.**

AR: Thank you for pointing this out. We will reformulate to clarify that this is the boundary-layer thickness over which temperature, salinity, and ocean velocities u and v are averaged (and we corrected to 20 m, as 30 m was an error).

RC: **l. 147: "basal mass loss" might be better as "basal mass flux" or something like that because usually "mass loss" is used in our field to refer to the uncompensated loss of mass from the ice sheet that contributes to sea-level rise.**

AR: Thank you for pointing this out. We will use "integrated melt rate" to stay consistent with the rest of the manuscript.

RC: **l. 188-191: Here, a bunch of physical constants and variables are introduced for the second time (they were introduced on l. 104-115). I think the only new ones here are Γ_{TS}, T_i and U. Also, it seemed to me that U might be the same as u_{oc} introduced above. If so, maybe use the same symbol for both. If not, maybe clarify the distinction.**

AR: Thank you for pointing out that this was confusing. We re-introduced them because the first introduction of the variables happened well earlier. We will clarify the definitions and avoid defining the same variable twice.

RC: **l. 213: "where the subscript oc denotes the far-field temperature and salinity that is advected into the cavity". This seems like a different definition than on l. 105: "T_{oc} and S_{oc} are the temperature and salinity averaged over a boundary layer below the ice shelf." It would be helpful if the oc subscript meant the same thing throughout the manuscript. Maybe another subscript like ff could be used for far-field quantities (or bl for boundary-layer quantities)?**

AR: Here also, thank you for this suggestion. We will re-read through the manuscript to make the definitions more homogeneous throughout the manuscript.

RC: **l. 217: The liquidus slope has already been defined on l. 108.**

AR: We defined it more losely in L108 but will make it more precise to avoid having to re-define it in L217.

RC: **l. 240-240: "However, through the resulting circulation, the temperature at one**

point of the ice shelf cavity is not necessarily independent from the temperature at the other points of the ice shelf cavity." This may be correct, but I understood the quadratic dependency a little differently. I thought the concept was that the overturning itself was driven by the aggregate effect of the thermal forcing (so that the strength of the overturning circulation would be proportional to the mean thermal forcing in the cavity). Thus, the local velocity is represented in terms of the amount of heat available for melting and there isn't necessarily any implication that the temperatures at different parts of the ice shelf are correlated to one another.

AR: We agree that this was not clearly formulated. We will reformulate as suggested.

RC: Eq. (17): It seems like some explanation of the bracket < > notation is needed.

AR: We thought this was a notation commonly used. We will add an explanation.

RC: Eqs. (20) and (21): I think these should be moved after Eq. (24) (with Eq. (20) after Eq. (21)) because they don't currently have any context where they are.

AR: We will move them as suggested.

RC: Eqs. (23) and (25) are identical, right? If so, I don't think it's necessary to have Eq. (25), you can just state that M_2 is still given by Eq. (23).

AR: They are identical but the characteristic length scale x used in both cases is different. We will re-arrange to avoid repeating Eq.(23) twice.

RC: l 311-312: "If one of the boxes has an area of zero...": It isn't clear to me how this could happen. Is this just for an ice shelf that is poorly resolved so that there aren't enough grid cells for each box to get some?

AR: Yes, this is how it can happen that a box has an area of zero. We will add a short explanation.

RC: l. 313: "are non-zero": Should this be "have a non-zero area?"

AR: Yes, we will reformulate as suggested.

RC: l. 349-350: "We use yearly mean profiles as the residence time of water in ice shelf cavities might be longer than a month for some cavities." You explain this more in the discussion but it might be worth giving it more context here, too. The

parameterizations do not include the finite advection time from the ice-shelf front to the ice-shelf base, meaning they are missing important delays that are present in the physical system. This means there is little hope of capturing seasonal melting in the proper phase.

AR: We will add a sentence to briefly mention this issue here as well.

RC: l. 431: "We therefore only show results for heterogeneous boxes hereafter." This is not true in the next subsection when you talk about PICOP, so maybe change "hereafter" to "in the remainder of this subsection."

AR: Thank you for pointing this out. We will reformulate as suggested.

RC: l. 458-460: "To put these RMSE in context, the integrated ice-shelf melt averaged over all ice shelves and simulation years in the reference is 38 Gt/yr, which means that even for the better-performing parameterisations, the RMSE remains very high compared to the reference melt as such."
I have concerns about this concept, related to my concerns about the metrics in Eqs. (22) and (23). I do not think it is meaningful that the total Antarctic melt flux (1500 Gt/yr) averaged over 36 ice shelves (an arbitrary number related to the resolution of the NEMO simulations used for reference) comes out to 38 Gt/yr. The different ice shelves have such different areas and melt rates that this average isn't informative. Given that I have problems with the metric from Eq. (22) that you're given context to, this might be a moot point but if you want to give context to what 40 Gt/yr is like, maybe just point out an ice shelf by name that has about that much melting.

AR: Thank you for expressing your concern, we will think about a new way to put the RMSE in context.

RC: l. 504-505: "...likely because they are all forced by yearly ocean temperatures." This seems to assume that the parameterizations would be able to get the Mode 3 melt if they were forced with seasonal or monthly temperatures. As you state later, they are unlikely to have the correct processes or the correct seasonal phasing to get these melt rates right even if temperature forcing were provided on a shorter time interval.

AR: We do not completely agree. If we used monthly temperature and salinity profiles as input, it would be possible to represent mode 3 melt. Due to the lag between inflow time and melt, the response in the melt would probably occur too early compared to the reference but the mechanisms might be grasped, and as mode 3 melt mostly occurs near the ice shelf front, the advection times are shorter than for grounding line melt. We will clarify this in one sentence.

RC: Figs. 4 and 7: I appreciated this visual representation of the RMSE – it was very intuitive.

AR: Thank you!

RC: Figs. 5 and 6: These are wonderfully done and very clear!

AR: Thank you!

RC: l. 575-576: "...and is composed of 36 random samples, with replacement, of the different ice shelves." Do I understand correctly that you double or triple (or whatever) count some ice shelves and omit other ice shelves in this process? That seems like an uncommon practice and that the approach (suggested by reviewer 1) of using only some of the years for training and holding back others for validation would be a more standard method for testing the robustness and parameter uncertainty.

AR: As explained for example in Wilks (2006), bootstrapping relies on resampling different samples with the *same* size. This is why it works "with replacement". Bootstrapping gives an estimate of the parameter uncertainty. The cross-validation, which is the procedure used in the iterations of tuning (holding back part of the sample to evaluate on this part), provides an estimate of the generalisation performance of the parameterisation. These are two different conclusions from a statistical point of view.
We agree that the method presented here only shows the uncertainty introduced by the choice of included ice shelves. We will include the time-chunk dimension in the resampling to cover the full uncertainty of the parameters.
We will explain the procedure and methods more thoroughly.

RC: Fig. 9a. On the light gray curve it says "fit on pairs with E0 < 50" but in the text describing Eq. (34), I didn't see anything stating that it was for E0 < 50. Is (34) not for the gray curve? Is this description simply missing from the text?

AR: Yes, this is missing, thank you for pointing it out. We will add it.

RC: l. 617-619: "Note that some of the higher values of $C_d^{1/2}\Gamma_{TS}$ and E_0 are several orders of magnitude higher than expected (see e.g. Table 12), which we cannot explain." This seems odd. Don't you have constraints on the parameters to prevent them from varying outside a physically likely range?

AR: We agree that this is odd and is not satisfying. Introducing lower constraints led to parameters "stuck" at the constraint, showing that the lowest RMSE was reached with unrealistically high parameter values. If after the new tuning and evaluation procedure this problem still exists, we see no other option than acknowledging it.

RC: l. 648: "The slope-dependent parameterisation is nonetheless relatively good for cavity melt rates..." Do you mean "...good for capturing melt patterns" or something like that?

AR: Yes, this is what we meant. We will reformulate to clarify.

RC: l. 744-745: "...with RMSEs on the same order as or even larger than the reference value." This is another reference to ice-shelf-by-ice-shelf statistics that I think needs to be rethought in terms of area-weighted statistics.

AR: We will think about an alternative formulation.

RC: Appendix D: Could you say how the RMSE for the heatmaps are defined? Are these the same as Eq. (32) but with Nisf = 1?

AR: Yes, thank you for pointing out that this is not clear. We will add a little explanation. It is the same as Eq (32) for Fig. D1 and Eq (33) for Fig. D2 but with N_{isf}=1.

RC: ***Typographical and Grammatical Suggestions***

RC: l. 11: "Additionally to..." should be "In addition to..."

AR: Thanks, will be changed.

RC: l. 28: "uncertainty source" should be "source of uncertainty"

AR: Thanks, will be changed.

RC: l. 140: Probably remove "For example", since this sentence didn't clearly follow (at least for me) as an example of the changed parameters not having a significant impact on the physical ocean.

AR: Maybe this was not clear enough then. We mean that the changed parameters don't have a significant impact on the physical link between the ocean in front of the ice shelf and the basal melt rates. They have, however, an impact on the physical ocean *outside* of the cavity. So this "for example" actually should follow the sentence before. We will reformulate to clarify.

RC: l. 204: "from an ice shelf to another" should be "from one ice shelf to another".

AR: Thanks, will be changed.

RC: l. 252: "(based on Sec. 2.2.2)" might be more natural as "(as described in the next section)".

AR: Thanks, will be changed.

RC: l. 288-291: This is a lot in one sentence and gets a bit confusing. It might be clearer if you break the details of each subscript into its own sentence.

AR: Thank you for pointing it out. We will break this into several sentences as suggested.

RC: l. 308, 314, 315, 437, 444, 446, 575, 589, 666, 667, 747, Appendix C, Table C1: The word "amount" is used in several places where "number" is correct (because the object being referred to is countable, rather than indefinite): "number of boxes", "number of data points", "number of high-resolution ocean simulations", etc.

AR: Thank you for this very helpful comment. We will correct this mistake.

RC: l. 381-382: "two constraints are additionally needed" would be better as "two additional constraints are needed"

AR: Thanks, will be changed.

RC: l. 399, 434: "(2020)(Table 4, 1st and 3rd columns)" and similar: This is very picky on my part but if there's a way to avoid back-to-back parentheses like here, I would prefer it.

AR: Thanks, we will try to change.

RC: l. 404-405: "The new tuning only slightly achieves to reduce the RMSE further" would be better as "The new tuning achieves only a slight ruther reduction in RMSE"

AR: Thanks, will be changed.

RC: l. 414-415 and 449: "input properties are of a similar order of magnitude" and "all of a similar order of magnitude"

AR: Thanks, will be changed.

RC: l. 438: "...about twice as high as the..."

AR: Thanks, will be changed.

RC: l. 454: "Slightly above..." should be "At slightly above..."

AR: Thanks, will be changed.

RC: l. 460, 517, 565: "... to the reference melt as such." and "...the melt patterns as such". The phrase "as such" is used in a few places in the paper where I don't fully understand it. Maybe you mean "itself"? Maybe this can just be omitted?

AR: We will identify the occurrences and look for a better formulation.

RC: l. 506: "additionally" should be "in addition"

AR: Thanks, will be changed.

RC: l. 519: "This can be explained by punctual very strong melt". I do not know for sure what is meant here by "punctual". This usually means "on time".

AR: Thank you for pointing this out, this was a wrong translation from our French mind then. We meant "in a very small region" (like a point). Will be changed.

RC: l. 521: "should therefore be kept" would be a bit better as "is important to keep" and "On the opposite" should be "In contrast".

AR: Thanks, will be changed.

RC: l. 527: "...in a more consistent way and with larger...

AR: Thanks, will be changed.

RC: l. 543: "emulates well NEMO" should be "emulates NEMO well"

AR: Thanks, will be changed.

RC: l. 549-550: "...and therefore rather is a result of the circum-Antarctic tuning rather than of the resolution"

AR: Thanks, will be changed.

RC: l. 550 "...on the contrary" should be "in contrast"

AR: Thanks, will be changed.

RC: l. 568: "...require to account for..." should be "...require accounting for..."

AR: Thanks, will be changed.

RC: l. 616: "This relation is different from Eq. (34)..."

AR: Thanks, will be changed.

RC: l. 627: "On the opposite" should probably be "In contrast"

AR: Thanks, will be changed.

RC: l. 637: "Especially" should be something like "In particular"

AR: Thanks, will be changed.

RC: l. 648: "...used as end members" might be "...used to generate end members..."

AR: Thanks, will be changed.

RC: l. 660-661: "We therefore suggest, when possible, trying out..."

AR: Thanks, will be changed.

RC: l. 694: "...any improvement in either of the two..."

AR: Thanks, will be changed.

RC: l. 716: "...take into account, to some extent, the horizontal..."

AR: Thanks, will be changed.

RC: l. 719: missing a comma after "geometry"

AR: Thanks, will be changed.

References

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