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Interactive comment on "Assessment of the Greenland ice sheet – atmosphere feedbacks for the next century with a regional atmospheric model fully coupled to an ice sheet model" *by* Sebastien Le clec'h et al.

Anonymous Referee #3

Received and published: 12 December 2017

Review of "Assessment of the Greenland ice sheet - atmosphere feedbacks for the next century with a regional atmospheric model fully coupled to an ice sheet model" by Sebastien Le clec'h and others.

Summary ——-

The two-way coupling between a regional climate model and an ice sheet model is an important development that marks a clear step forward to improve the projections of the future contribution of the Greenland ice sheet to sea-level change. The manuscript

[Printer-friendly version]

[Discussion paper]

compares results of the two-way coupling to former methods of representing the interactions between ice sheet and atmosphere and comes to important conclusions concerning the errors implicit to those simpler approaches. The manuscript is of clear interest to the readers of The Cryosphere but still needs to be substantially improved before being acceptable for publication. I recommend major revisions along the comments outlined below.

General comments —————-

The language of the manuscript needs substantial improvement, because many formulations give rise to misinterpretations of the scientific content. While many mistakes could clearly be avoided with a better command of the English language, a large number of typographical errors and mistakes in the referencing suggest that the authors could have made a better effort to deliver a readable manuscript for the review process.

The text itself reveals that the models are not actually fully coupled (use of anomaly method) and also gives indications why a full coupling is so much more difficult to achieve. I suggest to adjust the title and modify other occurrences of "fully coupled" in the text to "two-way" coupled to take this into account. A discussion item on this point and next steps that need to follow to work towards truly full coupling between RCMs and ISMs should be included.

The ice sheet initialisation procedure is somewhat non-standard and therefore requires a much better explanation. As it is heavily based on former work that is in part not well documented, an additional effort is required to describe the method in a way reproducible for other modellers. Finally, the evaluation of the method appears to be based on an experiment that is not closely related to the model state actually used for the projections, which may be possible to resolve with an additional control experiment.

The thermodynamic aspect of the model is not well represented, arguably because it plays a minor role for the present work. Nevertheless, substantial computing time

is spent during initialisation to equilibrate the temperature and the role of bottom and surface boundary conditions is mentioned. Therefore, the model description in 2.2 requires at least a short description of this model component.

The experiment names are not specific enough and should be improved. For my understanding, what is presented as the method "no coupling" is in fact a one-way coupling, where the ice sheet is responding to changes computed by the RCM (with "no feedback"). 2-W is correctly described, but 1-W is somewhere between one-way and two-way coupling because it parameterises the feedback. Maybe you could use "no feedback", "parameterised feedback" and "two-way" instead.

The most important question in the comparison of results after 100 years and after 150 years is left open: why does the behaviour of 2-W suddenly change around 2010. For this it may be instructive to also look more detailed at around 2060, where a similar shift is possibly visible. Otherwise, I find the comparison redundant because the bottom line in most cases is 'like after 100 years, only stronger'.

The integration of SMB anomalies already discussed in the manuscript could be added as an additional experiment, possible even two, if masking would be additionally taken into account. This would facilitate the comparison and place the discussion of the effect of masking on firmer ground.

Please also see corresponding specific comments for where these issues appear in the text.

Specific comments —————

Title I would argue that the models are not "fully", but rather "two-way" coupled because an intermediate down-scaling step is necessary and, more importantly, an anomaly method is used.

P1.L1 Better "the projected sea-level contribution from the *Greenland ice sheet*". Also mention a typical time scale here to make clear this is about the centennial time-scale.

P1.L2 Be more precise about the mechanisms and feedback(s). The next sentence ("these feedback*s*") suggests that "temperature and surface mass balance – elevation feedback" refers to at least two feedbacks. What are these precisely? "surface mass balance – elevation feedback" is clear, but what is the role of temperature? Note also that melting is clearly related to temperature increase, but the SMB is ultimately controlled by the energy balance.

P1.L5 A bit confusing to mention start date as 2020. It is understood later that before 2020 elevation changes are considered too small to make a difference. But at this place it may be better to give the period of the entire simulation (2006 - 2150). Note also that the RCP is not defined beyond 2100, so it is better to mention "prolonged RCP 8.5 scenario".

P1.L5 It seems confusing to call this simple method "no coupling", since it represents a one-way coupling. See also general comment on naming the experiments.

P1.L6 Could mention that this one-way coupling methods attempts to incorporate or parametrise two-way interaction. It represents an intermediate method between one-way and two-way coupling. See also general comment on naming the experiments.

P1.L7 I suggest to omit "offline". The correction may be offline to MAR, but it is online to the ice sheet model, as the correction is updated every time step and dependent on the current ice sheet elevation. Could add what is happening with the extent, since it has been explicitly mentioned for the former method.

P1.L9 Clearer to replace "ice sheet elevation feedback" by "surface mass balance – elevation feedback".

P1.L9 Maybe ", the one-way and two-way coupling methods ..." since the amplification occurs in both cases.

P1.L11 Some ice sheet margins are not in the coastal region. Replace by "ice sheet margins" or similar. This should be followed throughout the document for other occur-

rences.

P1.L15 "52 400 kmˆ2 smaller"

P1.L16 "fixed ice sheet mask"

P1.L20 "always" is only true for the end of the simulation. In the first decades or so the volume loss difference cannot be significant. Maybe give an estimate for a time scale where this is true similar to the comparison one-way vs. two-way.

P2.L6 "the ablation" (singular) <–> "are processes" (plural). Reformulate

P2.L6 Some risk for confusion here. It is a bit simplistic for a paper discussing an RCM as an important component to reduce the interaction to changes in SMB and temperature. It is understood that these are the two variables used to force the ice sheet model, but a bit more detail is required. How does the change of ice extent change the albedo and therefore the energy balance? Does temperature enter the correction method and how? OK, accumulation and ablation are sensitive to ST, but why and how? Also, what is the role of ST other than its influence on SMB, as boundary condition to ice thermodynamics? Does it have an impact on the simulations at all (I don't expect it, but would be good to say something about why not and being able to exclude it).

P2.L6 Maybe already intended, but make really clear that the changes in ST have no direct effect on thickness volume and extent. Reformulate.

P2.L9 Replace "disrupt" by "modify"

P2.L19 More detail needed. Amplification of mass loss by what process under what forcing and compared to what other (control) experiment?

P2.L22 The beginning of this sentence suggests (and I agree) that increased resolution would help to improve the modelling compared to observations, while "more detailed physics" is at least for the ice sheet model typically associated with 'less approximation',

C5

i.e higher order physics. Could you add some detail to distinguish these.

P2.L26 Should introduce RCMs and add references to MAR, RACMO, HIRHAM ... already here, as that is the obvious choice to increased resolution. Introducing the Franco and Edwards methods is already a step further as it is based on RCM output.

P3.L3 Sentence misses references for examples of RCMs.

P3.L9 Specify again for what it is a requirement.

P3.L10 Reformulate "usually used" to "typically used" or similar.

P3.L11 Add reference to Goelzer et al. 2017 here, since it is specifically on GrIS models.

P3.L18 Remove "high resolution" or specify explicitly at what resolution GRISLI is run.

P3.L21 "two-way"

P3.L25 I would consider the three methods part of the experimental setup and therefore name initialisation and experimental setup first.

P4.L4 "developed". Correct also throughout the manuscript.

P4.L4 "SISVAT" requires a reference and description of the acronym.

P4.L16 "ice albedo that has been improved by parametrising the impact of melt ponds on the albedo."

P4.L19 Replace "provided by" by "taken from"

P4.L20 "forced with 6-hourly atmospheric fields". See also P8.L6

P4.L24 Remove "forcing"

P4.L27 Suggest reformulation to "... because it has been shown by Fettweis et al. (2013), to be the best choice from the CMIP5 data-base to reproduce the present-day climate compared to results of MAR forced by reanalyses."

C6

P5.L1 Heading "Climate model initialisation and experiment"

P5.L2 What is the difference between "spurious drifts" and "unwanted trends" or are they one and the same? Reformulate.

P5.L3 Replace "SISVAT requires more than 6 years", by "SISVAT requires less than 7 years" to make clear that the chosen 7-year period is long enough. Or otherwise explain why 7 years is considered OK.

P5.L5 Replace "provided by" by "taken from". Add explanation how the data was interpolated to the coarse MAR grid.

P5.L5 Be consistent in if SISVAT is written in italic or not.

P5.L6 Replace "following year 1976" by "from 1977 onward".

P5.L10 Need to explain in more detail why 2095 can be considered representative for the 2090s. Is it e.g. the year that is closest to the decadal mean? Are trends so linear that the middle of the decade are representative for the average? Typically one would use the decadal mean to represent the long-term average and not one individual year, unless it doesn't matter for some reason.

P5.L12 Better to omit "coupled" here, since it is not clear what is coupled to what and it is further detailed later.

P5.L15 "the northern hemisphere ice sheet*s* (NH references) and the Greenland ice sheet (GrIS references)". or "the northern hemisphere ice sheet*s* and the Greenland ice sheet (all references)".

P5.L16 "... covering Greenland with ...", since the coverage extends outside of the ice sheet mask. Add information about the vertical.

P5.L17 Need to specify what "hybrid" means.

P5.L19 Need to add explanation on the thermodynamic aspect of the model. See also

general comment.

P5.L27 SIA velocity is even stronger controlled by ice thickness.

P5.L28 "SSA component is mainly controlled by the ice flux" is confusing because ice flux is velocity x ice thickness. Clarify!

P5.L29 "rheologies" is the wrong term here. Maybe "deformation regimes".

P5.L30 Replace "ice melting point" by "pressure melting point"

P6.L3 Replace "floating criterion" by "floatation criterion"

P6.L4 What does "characteristics of the Greenland bedrock" mean? Explain

P6.L5 Does that mean the enhancement factor differs for different regions? Explain.

P6.L6 "ice loading changes"

P6.L7 Add a reference for the used isostatic model.

P6.L8 Add a reference describing the thermodynamic model.

P6.L10 This whole paragraph needs to be reworked. Be more specific. What is considered a boundary condition, what is input data and what is considered a forcing? What variables are concerned for ice flow, ice thermodynamics and isostasy?

P6.L11 Is there are a difference between "The annual mean near surface air temperature" and ST? If yes, explain, if not, use TS instead.

P6.L13 What data are these 'boundary conditions' and which variables are taken from which data set? Surface elevation, bedrock elevation and ice thickness are not boundary conditions to the equations that GRISLI solves in the proper sense. You could call this "input data" instead.

P6.L14 "The climatic forcings". Say what they are! TS and SMB?

P6.L15 If basal drag were a boundary condition, it could hardly be computed. Refor-

mulate to make this clearer.

P6.L18 Heading "Ice sheet model initialisation and experiments"

P6.L19 The motivation is not quite correct. I would argue that to equilibrate the model to a steady state is not a necessity given the approximations, but rather a choice. One could envision a transient spinup as initialisation with the exact same model.

P6.L20 Again, more precision needed. What the ice sheet model equilibrates to is rather the climate forcing held constant for this particular initial steady state experiment.

P6.L20 Replace "sensitivity" by "forced" or "forward".

P6.L20 Reference Le clec'h et al. (in prep) is not in the reference list. If you are referring to the present manuscript, say that instead of using an external reference.

P6.L22 Replace "avoid" by "reduce", since the method is not perfect. Also I'd suggest the formulation ".. reduce an initial adjustment of the model during the first years of the simulation due to factors not related to the climate forcing alone." or similar.

P6.L25 Reformulate "just over the bedrock". Maybe "basal conditions".

P6.L26 If basal conditions are "likely to change in time" your method to define spatially variable but *temporally fixed* basal drag coefficient could never be successful. Should add here that your method assumes them to be constant over the 150 years of your experiment.

P6.L27 Suggest to remove sentence "As a result any error in the basal velocity computation can spread vertically in the ice and generate slowdown or acceleration of ice sheet motion." In its present form this sentence is generally true in any case and doesn't support your chose of assimilation method.

P6.L29 It is not clear to me at what point in the procedure observed velocities are actually used. Which observational data set is used? Reference needed.

P6.L30 "three main steps:" Make a numbered list (possible with lists of sub-steps) to facilitate navigation of the different steps.

P6.L31 Replace "not necessary consistent between them" by "not necessarily mutually consistent".

P6.L32 It looks to me like the first guess of basal drag mentioned here is a very good first guess and further adjustment of the basal drag coefficient is very much based on it. At any rate, a full description of the procedure used to arrive at that stage should be included, otherwise the method is not reproducible with another model (and not even with GRISLI itself). See also general comment on initialisation.

P6.L32 Edwards et al is a multi-model intercomparison and does not give specific details on the assimilation technique for GRISLI. The model reference there is given as Quiquet et al., 2012), which does not provide information on spatially variables tuning. Again, the method to produce the first guess basal drag needs to be made transparent for other modellers to be able to reproduce the results.

P6.L32 "surface and bottom". I think you mean surface elevation and bedrock topography. Be more specific!

P7.L1 "vertical fields" Be more specific!

P7.L2 If I understand correctly, you calculate something here in the first step to be used in the second step. Maybe you should say that. Confusing to mention already here "to have an ice flux as close as possible to observation" when diagnostically calculating something here will not have any influence on the match of the ice flux with observations in this step. This could be mentioned in the second step or as a general motivation for your method before.

P7.L3 Not clear to me how to derive a factor (a/b) from a difference (H1-H0). Please provide an equation or better explanation what the underlying idea is, what is done here, and how it is calculated?

P7.L4 You are mixing topography differences and ice thickness differences. Possibly similar or identical in absence of bedrock adjustment, but is it necessary to distinguish them?

P7.L4 Again "the factor allows to decrease (resp. increase) the surface ice velocity" is confusing, because this is not happening in this first step. Also "If *locally* the topography difference *is* positive ..."

P7.L5 How does deltaH translate into deltaV? How does the new velocity compare to observed surface velocities?

P7.L11 How is the new coefficient calculated? Explain in detail. This is reminiscent of the method of Pollard and DeConto 2012, could you describe the similarities and differences to their approach? Surprisingly your adjustment goes very fast (in total less than 2000 years). This makes me believe that the original basal drag was already a good guess and you only need minor adjustments. Is that correct? How different is the final basal drag field from the initial one? Can we see a figure for this comparison?

P7.L15 Replace "minimum gap" by "error".

P7.L15 You additionally need to convince the reader here that this method is optimal in the parameter choices (adjustment time 20 y, relaxation time 200 years) and to make clear in how far the results are (not) dependent on these choices.

P7.L20 After each step you have "a new set of initial conditions" for the next step. Maybe better to only name the final result of your initialisation your initial state as input for the forward experiments.

P7.L21 After 30 kyr, T is in equilibrium with the climate *and with the fixed geometry*, but not the other way around. In the next step of retuning basal drag, you further evolve the geometry and the ice temperature? Could you quantify, give an estimate how far from equilibrium you are now? Why could you not run (part of the initialisation) with freely evolving temperature? What is your stopping criterium at this point and the

reason for not iterating further?

P7.L27 It is not clear why evaluation of the initial state should be based on an experiment which includes further relaxation steps. The control experiment that offers itself naturally and should be used for that purpose is just running the model after step 3 forward with constant forcing. This would give a good indication of the match with observations (at t=0 or t=25) and the remaining model drift (after 150 years), since this is the model state actually used as initial state for the forward experiments. It anyhow seems strange to impose the observed geometry, when the model has been relaxed to a different geometry in step 3.

P7.L32 It is a bit unusual to specify errors in ice thickness as median values, given that errors locally could be positive or negative. Why not specify the absolute error or root mean squared error augmented with the quantiles given already. A map of the mismatch with observations should be given (possibly in the appendix), but then for the model state after step 3, which is assumed as the initial state for the projections.

P8.L1 The model state that has been compared to other models in the initMIP exercise appears to be different from the state used in the forward experiments, because it includes re-imposing the observed geometry and additional relaxation for 2000 years. This should be made very clear, especially in light of the claim that the model is one of the best in the model comparison. This statement in particular requires further qualification and needs to specify what criteria to consider, since the Goelzer et al paper does not provide any explicit ranking of the models and goes into length about how different criteria for evaluating models are not independent. Please use such community efforts to improve your model, but don't misuse them to gain credibility for your model.

P8.L5 SLR contribution as the most abstract change could be named last.

P8.L16 Is the elevation difference used for the correction calculated between Bamber (at 5 km) and Bamber (at 25 km) bi-linearly interpolated to 5 km? Please describe.

P8.L21 This seems to imply that at least until 2020, NC is an appropriate approximation to the full problem. This should enter the discussion and the abstract, following an earlier comment. Is there any reason why the modification starts at 2020 and not at 2000? It would seem like a cleaner comparison to start the interaction from the moment it is possible (i.e. 2000).

P8.L23 Another "coupling method" that is already discussed in the text and could be formally listed here as well is the one where MAR SMB anomalies alone are used to generate a changing ice sheet geometry (in the absence of an ice sheet model). This experiment can be performed with or without taking into account the surface elevation - SMB feedback and with or without fixed ice sheet extent.

P9.L13 This section reveals that the models are not actually fully coupled and also gives indications why a full coupling is so much more difficult to achieve. See general comment.

P10.L2 The mean decrease in SMB explains the shift in the ELA not the other way around. The ELA is an abstract concept, the SMB change is 'real'.

P10.L5 I am not sure reporting the changes in ice thickness changes as mean and standard deviation makes much sense, given the bipolar nature of thickening in the centre and thinning at the margins. More useful would be for me to describe the changes for specific regions.

P10.L12 What exactly is the impact of ice temperature on ice dynamics? Are you implying that changes of the surface boundary conditions modify the temperature structure of the ice and its deformation?

P10.L13 Are you talking about velocity or velocity anomalies here? Figure 4A shows anomalies! Please clarify.

P10.L15 This statement calls for a figure comparing modelled and observed velocities! Add a panel to substantiate this point.

P10.L18 Add "in this area" after "ice velocities" and remove it in the sentence after.

P10.L30 "amplification of all the changes" is a bit too general here. Better "amplification of the changes".

P10.L31 A figure showing the absolute sea-level changes for the different experiments would be in place, possible as additional panel in figure 7.

P11.L3 ST is already defined

P11.L4 Replace "is strongly colder" by "sees a strong cooling" or similar.

P11.L10 "Thus, the *stronger* ST decrease in 2-W compared to NC ...", assuming there is decrease in both cases. To check also in other places that you discuss differences in changes, not changes itself.

P11.L10 Not sure where "the middle of the slope is". Clarify!

P11.L14 Costal regions don't exist inland from the ice edge.

P11.L28 Replace "SMB anomalies increases by a factor of 10" by "SMB anomalies decreases by 10 cm yr-1"

P12.L1 Again mixing discussion of surface elevation and ice thickness here. Revise.

P12.L1 Add "difference" after "surface elevation change" and reformulate to "follow the patterns of SMB anomaly differences (Fig. 6B)".

P12.L5 Do you mean lower surface temperature in 2-W is the cause for higher SMB and therefore increasing ice thickness, or is the lower surface temperature directly impacting ice thickness (i.e. not through its effect on SMB)? In the first case, lower surface temperature and its effect on SMB should be mentioned first and higher SMB as a consequence. More precision needed here.

P12.L6 "in areas of lower ST"? In my eyes, ST and ST differences (Fig. 6A) are both high and positive in regions of negative thickness anomaly. Clarify that statement.

P12.L11 I thought you are trying to describe here the impact on the ice thickness evolution of two-way coupling as opposed to no coupling. In this part, you however come to the impact on the atmospheric circulation (katabatic winds) and land model changes (albedo). From line 15 on, you go back again to ice dynamic changes. Could this material be better organised to avoid jumping between the different aspects? Also, if I understand correctly, the anomalous katabatic winds created by 2-W have visible impact mainly on the narrow marginal areas of the ice sheet where anomalous cooling increases SMB. This should then be counteracted by the albedo changes described L12 and following. It is not really resolved for me how these different factors influence each other and which is the dominant mechanism in which region.

P12.L13 What is the difference between snow-free and snow free?

P12.L22 Melting itself does not necessarily contribute to SLR since melt water can be refrozen in the snow pack. Better replace "melting contribution" by "ice sheet contribution" or similar, also in the rest of the manuscript.

P12.L22 These numbers should be calculated against a control experiment to remove the contribution from remaining model drift. Has this been done?

P12.L23 I would suggest to add a panel to figure 7 with the total contributions for the three experiments and include the integrated SMB mentioned further below in this section.

P12.L25 Since you discuss 2-W against NC, the surface elevation - SMB feedback which operates all over the ice sheet should also be mentioned, not just the processes at the margin.

P12.L26 The difference of 52400 km2 is at the end of the experiment and then it increases with time? Reformulate.

P12.L27 I think all you are saying is that the high resolution ISM mask changes are translated to partial mask changes for MAR. Clarify that the ice sheet mask (Fig S2B)

is the one seen by MAR.

P12.L31 I think the point to make here is not about increase in uncertainty. You can show that when a fixed mask is used, you simply get the wrong result and overestimate the mass loss. Could you quantify the relative importance of this effect compared to the error that is made when not taking into account the surface elevation - SMB feedback?

P13.L3 Please specify the resulting SLR.

P13.L14 This is exactly the reason why median results are not very meaningful in this context. Mean absolute or root mean squared differences are easier to interpret.

P13.L15 After showing figure 8, figure 9 does not add substantial information in my view. I would remove it and continue discussion about differences between 2W and 1W based on figure 8. The only reason to show figure 9 would be if you wanted to attempt modifying the parameterisation used in 1W to incorporate the katabatic wind effect, which could be a logical next step.

P13.L27 These sentences are just stating the obvious. I'd suggest to remove them.

P14.L5 It is not clear to me why a higher resolution should lead to increase the SLR and not the opposite. Unless there are convincing arguments to support that claim, I would leave the sign of the change open. The same applies to the limitation of constant basal drag in the next sentence. With all the complexities surrounding the evolution of the basal conditions over time, I don't think there is any evidence that acceleration of ice flow has to be the dominant response. Again, putting forward some convincing arguments would be appreciated.

P14.L19 Additional limitations that should be discussed: - Ignoring the glacial-interglacial signature of past climate changes in this steady state spip-up of temperature typically makes the ice too warm. This needs to be compensated by other factors (likely a different set of basal drag parameters). - The steady state initialisation also ignores any influence of transients in the observed ice sheet evolution. - Mismatch

of the modelled ice sheet geometry and velocity structure with observations leads to uncertainties in the projected evolution.

P14.L28 Add "in this comparison" after "atmosphere-GrIS feedbacks". I hope you don't think this statement is universally true.

P14.L30 While this statement seems true for the given results, the conclusion hinges on the change in behaviour of 2W at 2110. Unless investigated in more detail, it can not be excluded that such change could happen at an earlier point in time, e.g. for a different model used as boundary condition to MAR.

P15.L5 This comparison is a bit awkward. Wouldn't it be more appropriate to compare the +0.5 cm to the total projected SLR as a relative error?

P15.L14 Remove repeated "respectively" after SLR.

P15.L19 It would be good to additionally put this number (21%) in perspective to the underestimation due to ignoring feedbacks, i.e. the difference between 2W and NC.

P15.L24 Again, I don't see any evidence for the interpretation that higher resolution and higher order physics increase the response.

P15.L29 Replace "disrupt" by "modify"

Tables: ——-

Table 1 Does "after 50 yrs" mean at year 2050? Maybe that would be a better indication. Or do you not want to assign an absolute date to your simulation? The historic and future RCP forcing is clearly linked to an absolute date, though.

Since the ablation area changes so much, it may be interesting to calculate additional diagnostics for a constant region, e.g. for the observed present day ablation zone, or backwards for the area of the ablation zone area after 150 years. This way, the convolution with a changing area could be avoided.

Table 2 Not sure how to interpret a velocity change of e.g. -3.0+-25.0. The noise being much larger than the signal, is the valid interpretation 'no significant' change?

Figures ——-

The labels in the figures are upper case (A,B,C), but the panel references in the captions are all in lower case (a,b,c). Make consistent.

Figure 2 Why are figures B and C so different? At least in the interior, one would expect a pattern very similar to the SMB anomalies in this experiment. My guess is that this is indicative of a remaining model drift. Results of a control experiment starting after step 3 with constant forcing should be shown here or in the appendix and the origin of this difference should be discussed.

Figure 3 The displayed field is ice thickness, not surface elevation as written in the caption. Since the discussion is about ELA and surface elevation - SMB feedback, it may be useful to show surface elevation instead.

Figure 4 The colour scale in A is not easy to read with small positive and small negative values sharing the exact same colour (green). This should be improved. Have you tried to plot velocity ratios instead of anomalies? Since velocity magnitudes cover several orders of magnitude, a large relative change is not visible because of the cutoff at 2 myr-1, while small relative changes at the margin appear exaggerated.

Figure 5 Why not use the same colour mapping here and in figure 4 for the velocity anomalies? That would make it easier to compare the two figures. Caption: "left panel"

Figure 6 There appears to be a slight instability in one or both of the experiments compared in figure 6A. Also Figure 8A shows signs of instability in form of a checker board pattern. While these instabilities are likely not critical for the interpretation of the large scale results presented here, they should at least be mentioned.

Figure 7 Add a panel with absolute contributions of the three experiments. Note that

results shown so far are double differences, i.e. differences in anomalous contributions since year 2000 between different experiments. Could also show sea-level contribution differences calculated from difference to a control experiment with constant forcing to remove the model drift. Same consideration holds for the absolute contributions.

There seems to be a step change around 2060 and again around 2110, where the behaviour of 2w-1w (yellow) changes dramatically. By comparison with 2w-nc it appears to be caused by the evolution of 2W. What is happening at these moments in 2w? Please investigate this further.

Caption: "Differences in Greenland ice sheet sea-level contribution between the different experiments." Then explain how it is calculated.

Figure 9 Figrue 9 is not needed in my estimation.

References:

Format of many references in the text are non-standard. A few examples are given here, but all should be re-checked.

P3.L13 add e.g. before Gagliardini

P3.L19 reformat list of reference and avoid double brackets

P4.L10 "(e.g. Fettweis et al., 2013)

P6.L12 Author is called Fox Maule. Check reference.

P6.L20 Reference Le clec'h et al. (in prep) is not in the reference list.

P8.L2 add Goelzer et al., 2017 to the reference list.

References:

Fox Maule, C., Purucker, M. E., Olsen, N., and Mosegaard, K.: Heat flux anomalies in Antarctica revealed by satellite magnetic data, Science, 309, 464-467, doi: 10.1126/science.1106888, 2005.

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