Response to Interactive Comments

Troels Bøgeholm Mikkelsen, Aslak Grinsted and Peter D. Ditlevsen

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1 Letter from the authors

Dear editor and reviewers,

First of all we would like thank all of you for your time and careful reading of our manuscript, as evidenced by your thorough and constructive comments.

We have now gone through your comments and answered them each individually; where a change has been made in the new version of the manuscript this is specified.

After reading your comments and re-reading the introduction it was clear that the abstract and introduction needed a substantial revision. This has resulted in the removal of much of the text (as detailed where appropriate below) and a much more focused and hopefully clearer introduction.

Yours sincerely,
Troels Mikkelsen, Aslak Grinsted and Peter Ditlevsen

2 Comment by X. Fettweis, May 18

This paper highlights the impact of taking into account the temperature variability in future projections of the GrIS SMB using a simple idealized model. As shown by Fettweis et al. (2013) and well mentioned in this paper, the temperature dependence of melt is not linear while precip increases linearly with temperature. Therefore, forcing a model with a mean climate or a climate resolving the interannual variability will be different. But, don't forget that such effect is explicitly taken into account in all of the simulations forced by GCMs. However, in the idealized models, this effect is often neglected and this paper evaluates the additional mass loss if the interannual variability is taken into account. This (technical) paper is well written, fits well with TC and deserves to be published.

However, we can not forget that the results presented here (13% of additional mass loss) are only valid for

- idealized models.
- for GrIS (where the future changes will be driven by melt increase) and not Antarctica (where the future changes will be driven by precip changes).

Comment #1: We agree that the magnitude of the effect is small in comparison to projections for the 21st century. However, the bias will accumulate over time, and can result in metre scale biases in ice sheet volume after a few millenia (as illustrated in figure 2). The effect can therefore not be neglected in longer experiments such as spin-ups tuned to match present day ice sheet volume. This could bias the final volume in paleoclimatic spin-ups (e.g. in ISMIP6-initMIP, and SeaRISE) because the input forcing series may not include realistic high-frequency variability. We disagree that our main result (equation 7) only applies to GrIS and idealized models. You can apply Equation 7 to total mass balance (including discharge) of the Antarctic ice sheet. You can also apply it to individual catchments, mountain glaciers, and even individual grid-cells. The magnitude of the effect will depend on the level of variability, and how non-linear the response is. There is a growing concern that Antarctic mass loss could be very non-linear in the forcing (e.g. DeConto and Pollard). This potential for a large non-linearity tells us that it is important to take forcing variability into account when modelling Antarctica. We feel that this is an important point to highlight, even if we do not quantify it in this paper.

> Therefore, any references to Antarctica in Abstract and Conclusion should be avoid at maximum (e.g. lines 23-27, pg 11) as the Antarctica SMB is, in a 1st order, linearly temperature dependent and the fact that this effect is already explicitly taken into account in all of the more complex/realistic GCMs forced simulations should be more clearly stated into Abstract and Conclusion (e.g. lines 15-19, pg 11).

Comment #2: Clausius-Clayperon is non-linear, but it is a fair point that present day Antarctic accumulation is near-linear (6-9 %/K) for temperature deviations of a few degrees. Our equation 7 therefore predicts a small impact of forcing variability on accumulation. However, there is concern that Antarctic ice discharge may have a much more non-linear response (see above). Equation 7 tells us that variability has the potential to be important. We do not quantify it in this paper, but we feel it is important to highlight the potential issue in this paper.

There is nothing to change in abstract as there we have no results or discussion of Antarctica.

Change #1: We have added the following to the introduction: "This mechanism is important for the mass balance of present-day Greenland, but less so for present-day Antarctica where mass loss is dominated solid ice discharge (Church et al. (2013, p. 1170)). There is, however, some concern that Antarctic ice discharge and total mass balance may be highly non-linear. The potential for a large non-linear response of Antarctic mass balance is particularly evident in the simulations from Pollard et al. (2015)."

> Finally, the fact that the interannual variability of temperature could likely be not the dominant missing process (vs positive feedbacks) in idealized models should clearly be mentioned into the conclusion.

Comment #3: Equation 7 applies to all models if they do not explicitly allow for forcing variability. It does not only apply to idealized ice sheet models.

Change #2: We state that the effect is small compared to other effects after the analysis of the results by Robinson et al. (2012).

3 Comment by G. Roe, May 28

From the accompanying materials, it is suggested that the paper has undergone several rounds of review already. The editor has already supplied an impressive round of comments to which the authors have responded.

The principle result is one that is important to recognize in all non-linear dynamic models: when models are calibrated to time-mean inputs, there will be bias in the model coefficients because nonlinearities act to rectify variability in the forcing. In my experience this basic point is not as widely appreciated as it should be in glaciology.

Comment #4: Thank you. It has been very hard to get this point across. This issue is important in all non-linear models; not just our simple model, and not just GrIS.

The results suggest that the effect is significant enough for ice sheets the size of Greenland and Antarctica, that the issue must be accounted when making future model projections. I think it is worth making the point in the context of ice sheets, and that the result should be published.

I have three main comments and criticisms.

3.1 1.

1. I question how important this effect is relative to other uncertainties. While the point is worth making, the size of the effect the authors find is hardly the rate-limiting uncertainty in ice-sheet projections, or in establishing the likelihood of, or proximity to, tipping points. The authors own calculations suggest the effect of variance is the same as changing the mean temperature by 0.12K. This is obviously very small compared to the spread of uncertainty in model projections of future climate change, polar amplification, and the parameterization of ablation. I think a revised manuscript should discuss the results in relation to other uncertainties; and the asserted importance in the abstract and introduction might be dialed down a bit.

Comment #5: We agree that 0.12K is not the limiting uncertainty, and we have added text to highlight this (see also our answer to X. Fettweis above). We now compare the temperature adjustment to the uncertainty estimate from Robinson et al. (2012), where they estimate a 90% credible interval of 0.8°C – 3.2°C of the GrIS threshold. Our temperature correction is small compared to this range.

3.2 2.

2. The physical reason for the nonlinearity should be clearly described. As of now there is almost no explanation, it is presented as a model fact, and only recent papers are cited. A reader will likely crave having a physical reason provided.

Comment #6: We have rewritten the introduction to better align with the focus of the paper. In this new introduction we explain the non-linearity. Further, (as you suggest below) we now cite Roe and Lindzen (2001) several places where appropriate. **Change #3:** The following has been added to the introduction: "Ice sheets are characterized by a large interior plateau flanked by comparatively steeper margins. A warming will shift the equilibrium line altitude (ELA) to higher elevations increasing the area exposed to melt. The area exposed to melt will increase non-linearly with ELA because of the top-heavy hypsometry (van As et al., 2017)."

> That the mass balance should be nonlinear has long been known. It is implicit in the ELA sensitivities derived sixty years ago in Wertman (J Glac, 1960, 1963, Science 1976). The reason is also explicitly derived in Roe and Lindzen (Clim Dyn, 2001), and likely earlier and elsewhere. The ablation rate scales as temperature, and the ablation area scale as $\sim T^2$ because of the characteristic parabolic profile of ice sheets; giving a roughly cubic dependency for total ablation. There is also a smaller, but nontrivial effect, that the length of the melt season changes with T. Note the degree of nonlinearity will be different for the plastic-rheology profiles the authors use in the Oer03, from the dynamic ice model used in Robinson et al., so there is an internal inconsistency in the results presented here. (The degree of nonlinearity is larger for a shallow-ice rheology than it is for plastic-rheology ice sheet, by an increase of approximately one in the exponent.)

Comment #7: Thank you for the pointers to existing literature. We were aware of these, but did not find them necessary originally.

Clarification: The calculations we do on Robinson et al. (2012) are done using the curvature we measure from Robinssons results. There is no inconsistency.

> I don't understand why the authors did not use the temperature nonlinearity that is directly represented in the Oer03 model, and instead calibrated to a completely different model set-up from Robinson et al. (the former is an axisymmetric ice sheet and climate, the latter is a realistic Greenland). In Oer03 the ELA is directly specified in terms of temperature, and the geometric nonlinearity in the ablation-temperature relation certainly exists in Oer03. It would be a more self-consistent estimate of the effect, and certainly worth comparing with the extrapolations from Robinson et al.

Comment #8: The Oer03 model is only illustrative, and not useful in a quantitative sense. So, we cannot apply a correction based on the second derivative of Oer03 to Robinsson.

Here's our thinking behind the order of presentation in the paper: We derive equation 7, and we want to explain it. This equation applies to all models, and so we choose to use a simple model to illustrate how it works. Perfectly plastic models such as Oer03 are attractive here because the mass balance can be written as a function of V and T alone. This enables us to make the Fig. 2 which we think is useful. We do not argue that Oer03 is realistic, and we only

use it for illustrative purposes. We use it to verify that equation 7 holds for this simple case.

Once we have shown the reader that it works on the simple model, then we proceed to

- highlight that the non-linear relationship between T and mass-balance is not just an artifact of the simple Oer03 model. It is also present in RCM results from Fettweis.

- Estimate the magnitude of the effect on the results from a real ice sheet model Robinson et al. (2012).

If the authors have other mechanisms they have diagnosed or have speculations about, those should be given too. Otherwise it can be frustrating to read about an effect whose cause is not explained.

Comment #9: If this comment refers to the nonlinear relationship between temperature and SMB, we think manuscript has been improved by the edited introduction (see Change #3) and added references to Roe and Lindzen (2001).

3.3   3.

3. The application of stochastic climate variability.

The authors represent stochastic variability by applying AR(1) red noise in annual-mean temperatures.

Applying stochastic variability to the annual mean temperature is likely wrong. Annual-mean anomalies are the result of much larger stochastic variation in seasonal temperatures (seasonal fluctuations are ∼ sqrt(4) larger than annual mean. A model will fail to emulate realistic mass balance anomalies without accounting for theses larger seasonal fluctuations that actually drive the ablation budget.

Comment #10: It is clear that we need to use the full variance in equation 6. However, sub-annual frequency variability may already have been accounted for in the model. E.g. Robinson et al. (2012) already model the seasonal cycle, and the Oer03 PDD factors have been selected to be representative over an entire year. Our interpretation of these models mean we only need to consider variability on annual and longer timescales.

p5L9 "(AR2 AR10) = (0.67, 0.85)" What is the persistence timescale implied by this coefficient? For AR(1) tau = 1yr/(1-a) giving tau ∼ 3yrs. The uninitiated reader has no idea what the point of AR(1) is, and why it is important to use, so more explanation is needed. The persistence in the annual-mean anomalies are not going to be the same as the melt-season anomalies, which is important to account for. The ablation anomaly is due to the melt-season temperatures, and the melt-season persistence timescale is typically less than annual-mean.

Furthermore, for this to be rigorous, some kind of criterion should be used to evaluate whether AR(1) is a sufficient, self-consistent, and parsimonious description of the data.

There are various methods for establishing this (e.g.., Akaike information criteria, etc.), but none are referred to, so a reader has no idea of the necessity of this fit, or if any such estimate was performed. The data should be detrended before fitting, and the residuals should be tested for any remaining autocorrelation. I did not find either of these mentioned in the paper or supplementary.

Comment #11: Visually it is clear that there is some autocorrelation. We use a minimal model of that, but it is really not that important. Equation 6 does not depend on the autocovariance structure. Hence, we feel that it would be a distraction to discuss alternative models. One could score the models using Akaike information criteria, or similar to justify any more complex model.

We have done analyses similar to the ones presented in the paper (these additional analyses are not shown) using temperature time series T_t consisting of either 1) Gaussian random variates (white noise) and AR(1)-generated noise with a much longer persistence time scale τ (i.e., *not* a fit to observed Greenland temperatures). The persistence time scale of T_t naturally affects the persistence of V_t, but the change in equilibrium volume can still be determined using equation 6 in the manuscript and the *variance* of T_t alone, without referring to the persistence time scale.

We feel that an in-depth discussion of these issues would distract from our main point and add comparatively little to the quality of the paper.

Just by eye, the temperature time series shown in the supplementary looks a bit questionable between 1850 and 1900. I would recommend subsampling the data, and using other datasets (preferably instrumental records) to see how stable the estimates are.

Comment #12: As per above, correctly modeling the observed mean temperature over Greenland is not our main goal in the article. Such a study done properly would probably make a pretty good manuscript on its own.

Finally, AR(1) is a somewhat limited representation of climatic persistence. The spectrum flattens out at periods longer than 2*pi*tau, and so there is no persistence at multidecadal and centennial scales, in contrast to a power-law representation, for instance. The nature of climatic persistence at low frequencies is debated, but alternative representations would have important implications for these results and affect the answers quantitatively, so some discussion would be useful.

You might look at the discussion in Burke and Roe (Clim. Dyn., 2014), and Roe and Baker (J. Glac., 2016) for discussion of this in a glaciological context, and at the references therein for more general discussions. There are other references, but I'm most familiar with the ones I've written!

Comment #13: Long range persistence vs. AR(1) type models is an interesting issue and would probably warrant yet another paper in itself. Equation 6 is only sensitive to the variance; we allow for the simplest possible model of persistence.

3.4 Points:

p1 L4: "This bias could, if not taken into account, imply that the risk of collapse in a given climate change scenario is underestimated." This point is not developed in any way in the paper, and should be removed from the abstract.

Comment #14: AR5 put the collapse threshold for GrIS at +1.6 °C. The source for that is ultimately Robinsson et al. We estimate that the Robinsson results has to be shifted by -0.12°C. Do we really need to develop it further than that in the paper to justify that sentence?

It certainly seems important enough to highlight in the abstract, when you hold it up against the targets that have been put forward politically.

p1 L6 approximately 13%. Probably better to say 10 to 15%, given the model simplifications and uncertainties in its general application.

Comment #15: We agree that credible intervals for the ΔT and ΔSMB estimates should be added to the text. In the previous version of the manuscript this could only be determined from Fig. 4.

Change #4: The abstract now reads: "We estimate the bias to be 30 Gt/y (24 Gt/y – 59 Gt/y, 95% credibility) for a warming of 3 °C above preindustrial values, or 13% (10% – 25%, 95% credibility) of the present day rate of ice loss."

p1 L7: "Many predicted scenarios of the future climate show an increased variability in temperature over much of the Earth." This needs to be supported by citations or evidence. In most parts of the world observations are consistent with a linear trend acting on the same interannual variability. Unless supported strongly later in the paper, it is not clear if deserves to be in abstract. As of a final reading, there is no further discussion of this in the paper, and it should be removed from the abstract.

Change #5: We agree – this sentence has been removed.

p2 L16 "Greenland –, the West Antarctic –, and" weird dashes in my pdf.

Change #6: Sentence has been removed.

p1 L10. This whole introduction should be contracted. The proximity to a tipping point is not a main focus on the paper. The essential point of the paper is a simpler one about the nonlinearity of the mass balance subjected to climate fluctuations, and the effect is quite small. Uncertainty about tipping points is dominated by much larger effects than those postulated here.

Comment #16: It is correct that the main point of the paper is about the nonlinearity of the mass balance subjected to climate fluctuations. However, the mass balance non-linearity is more pronounced as you get closer to the threshold.

AR5 show the Greenland tipping point in figure 13.14c based on the model from Robinson et al. (2012). We estimate that this might be biased by -0.12°C. AR5 show a 90% credible interval from 0.8 to 2.2°C for the threshold location. This corresponds to a standard deviation of 0.42°C, assuming normality. So, yes, you are right that the full uncertainty is greater than the effect we discuss, but it is hardly insignificant. And further the effect we discuss is a bias with a clear sign.

> p2 L 23 "However, this approach disregards the effects of interannual variability." Perhaps more importantly it assumes the ice sheet is in equilibrium with the control climate (and implicitly the modern climate), which is unlikely to be true for large ice sheets.

Comment #17: This is a very good point. While editing the introduction we have striven to make it as compact and direct as possible; we feel that mentioning this point would add too much length.

> p2 L27. "We develop a general theoretical framework for how forcing variability impact the expected response in a model that exhibits a non-linear response." The nonlinearity of total ablation with respect to temperature is implicit in Weertman (1960, 1976) and explicit in, e.g., Roe and Lindzen (2001).

Comment #18: Thank you for this point – we have added these references to the introduction.   **Change #7:** The introduction now opens with "Ice sheet mass balance has a nonlinear dependence on temperature. This behavior is observed in simple ice sheet models (Weertman, 1961, 1964, 1976; Roe and Lindzen, 2001), in full regional climate modeling of Greenland surface mass balance (Fettweis et al., 2013), and the non-linear effect of temperature on melt has been observed in Greenland river discharge (van As et al., 2017)."

> p2 L34 "The results presented here show explicitly how to account for the effect of unresolved temperature variability." Well, it provides one estimate, it is far from a complete accounting and a replacement for its effects.

Comment #19: We agree, that sentence is incorrect.   **Change #8:** We have rephrased to "Our contribution is a quantification of this effect, and an estimate of the necessary bias correction needed to account for temperature fluctuations in long term ice sheet simulations."

> p3 L4. "That the SMB of an ice sheet model is nonlinear is well known." Statement depends on precise definition of nonlinear. Perhaps better phrased as 'nonlinear with respect to temperature fluctuations'.

Change #9: We have rephrased to "nonlinear with respect to temperature".

p3 L4. It would be nice if the fundamental physical reason were made clear. Although Roe and Lindzen (2001) could have been clearer, both ablation rate and ablation area increase with temperature. The characteristic parabolic shape of ice sheets means that total ablation rate scales as $\sim T^3$.

Change #10: We have added a the following after the sentence at p3 L4: "In a simplified model of continental ice sheets, Roe and Lindzen (2001) show that the total annual ablation scales with the cube of temperature at the ice sheet margin."

p3 L10. Sub annual temperature variability goes back much further. Early PDD formulations recognized the importance of stochastic fluctuations and included daily variability (e.g., Arnold and MacKay,1964; Reeh, 1991; Calov and Greve, 2005).

Change #11: Thank you for this point – we have added these references to the introduction.

p3 L12 "broader class of models." broader than what?

Change #12: We have removed that sentence.

p3 L16 "ice sheet initiated from a mountain glacier." On this scale, it is not relevant that it was initiated from a mountain glacier.

Change #13: Agree – we have removed that sentence.

p3, L28 "thus all components of the mass budget are uniquely determined by temperature and volume." This simply repeats the preceding clause.

Change #14: Good point, we have removed that sentence.

p3, L29 "This is a vast simplification" Not a scientific phrase.

Change #15: We have removed that sentence.

p3, L26 up to section 3: "Before proceeding with the simple model, we investigate the effect of interannual temperature fluctuations by considering the ice sheet as a simple dynamical system." What follows is much fancier than it needs to be. It is a simple point: total ablation is a nonlinear function of temperature, so +ve and -ve fluctuations do not average to zero. That's it. It does not need dressing up with this language, and it is thus less clear than it could be.

Comment #20: We do not agree that section 2.1 from "simple dynamical system" and onwards is written in an overly complicated way. It is clear that 'hot' and 'cold' do not average to zero, and that this is relatively straightforward to explain. The main contribution of this work, however, is equation 6 and to derive that we need the techniques from dynamical systems theory.

p5L11. Well, you've fit the AR(1) to observations, so it had better get the variance right.

Change #16: We agree that this sentence is superfluous – it has been removed.

p5L25 "The average mass budget of a colder year and a warmer year is less than the mass budget of a year with a temperature corresponding to the average of "cold" and "warm"; to put it another way: the increased SMB of a single anomalously cold year cannot balance the increased melt from an equally anomalously warm year." This and the equation that follows is a fancy way of saying the obvious. It is a shame that the basic geometric reason is not described simply and clearly, here and elsewhere: the ablation rate scales with temperature, the ablation area scales with T^2 because of the typically parabolic shape of ice sheets. So the total ablation scales as $\sim T^3$. An additional nonlinearity arises because the duration of the melt season also depends on temperature. So of course linear fluctuations do not average to zero. The paper's message would be stronger if a clear, simple physical description were provided.

Comment #21: Thank you for this comment – we have added a reference to Roe and Lindzen (2001), as stated above.

p7L1 "Fettweis et al. (2013) compare the output of RCMs forced with multiple future climate scenarios and show that the effect of rising temperature on the GrIS SMB is well described by a third degree polynomial" This is consistent with the cubic scaling of Roe and Lindzen (2001), derived from basic ice-sheet geometry.

Change #17: We have added the following: "consistent with the aforementioned findings of Roe and Lindzen (2001)."

p8 L8 "(see also supplementing information)." typo.

Change #18: Fixed.

p8L14 "Combing these numbers we arrive at a warming of 3.0C in the year 2100 relative to the preindustrial when considering the RCP45 scenario. For this value it is seen in Figure 3 that an additional 0.12C should be added to any constant warming term" First, combining, not combing. Second, some context would be useful here. Uncertainty in transient climate response is approximately a factor of 2 (at 2ish sigma). So the effect described here (0.12C) is pretty small in the scheme of things at 2100. A reader should be given a clear message about what the rate limiting uncertainty is for these problems.

p8L25 "The results above highlight that interannual temperature variability cannot be neglected in long term studies involving ice sheet models." Realistically, there are bigger uncertainties that swamp this effect. So these are strong words.

Comment #22: As stated above, the effect is small but nevertheless a bias with a clear sign.

> **Change #19:** We have added a 95% credible interval to the estimate.
>
> **Change #20:** We believe we now spell "combining" correctly!

> p10 L5 "Our result may be used to explicitly implement the contribution from the temperature fluctuations in the mass balance schemes before bias correcting due to other possible model deficiencies." Hmmmm. How exactly? The effect has been estimated only from one model calculation and only for the Greenland ice sheet. What confidence is there in the numbers so derived? One would need to know the the uncertainties before the correction could be applied even to Greenland, and what confidence is there is applying the effect to Icelandic, Alaskan, or Patagonian ice caps, or to Antarctica? It would be better to have a physical theory rather than to rely on a calibration based on one model and one location.

Comment #23: We agree that this wording is too strong. On the other hand we have shown that in a simple model this effect cannot be neglected, hopefully prompting further investigations using more comprehensive models.

> **Change #21:** We have rephrased to "Our results shows the importance of considering temperature fluctuations in the mass balance schemes before bias correcting for other possible model deficiencies."

> p10 L10 "The effect is explained by the curvature, or second derivative, of the mass balance as a function of temperature. A negative curvature gives rise to nonlinear effects meaning that the average mass accumulation resulting from a cold year and a warm year in succession is less than the mass accumulation of two consecutive years having the average temperature of the "warm" and "cold" years." This just states what nonlinear means. Again it is a shame not to have a clear physical description of why this is so, since previous studies long ago articulated the fundamental geometric reasons for this behavior.

Comment #24: We agree that the two sentences basically state the same, but added the second sentence as a potentially useful clarification for the reader. Regarding the physical explanation, the reader may refer to the previously added references to Roe and Lindzen (2001). **Change #22:** We have removed the sentence starting with "A negative curvature . . . "

> p10, Line 14: "the results are transferable to other more realistic models" The authors really should be clearer about this. Transfer to what scales, and to models of what? Alpine glaciers, ice capes, other ice sheets? How can it be transferred? The basic point (mass balance is nonlinear) should be considered, but the quantitative application to other systems is highly uncertain and would need specific calibration to each setting.

Comment #25: We agree that this sentence could be clearer.

> **Change #23:** We have rephrased the sentence to: "We find that the steady state ice sheet volume in Oer03 is $0.5 - 1$ m$_{\mathrm{SLE}}$ smaller when the minimal model

is forced with fluctuating temperatures compared to constant temperature (Fig. 2). It is therefore necessary to consider the impact of temperature variability when designing long-term model experiments such as paleo spin-ups (eg. Bindschadler et al. (2013); Golledge et al. (2015); Nowicki et al. (2016)), especially when downsampling the paleo forcing series. Though differences between ice sheet models may be larger than the effect of temperature fluctuations estimated here, we expect the effect to be in the same direction and of similar magnitude for all models. Furthermore, models of sub-shelf melting, grounding line migration, and ice discharge have the potential to respond nonlinearly to changes in ocean temperatures (Favier et al., 2014; Joughin et al., 2014; Seroussi et al., 2014; Mengel and Levermann, 2014; Pollard et al., 2015; Fogwill et al., 2014), thus it is critical to take variability into account for quantitative assessments."

> p10, L24: "This is an interesting special case of an accumulation dominated mass balance" Accumulation variability dominates the mass balance variability of many maritime glaciers. See Medwedeff and Roe (Clim. Dyn., 2017)

Comment #26: This is a very interesting reference, thank you. However, we feel that the specific connection between temperature and precipitation regarding Antarctica is best illustrated by Frieler et al. (2015).

> p10 L 12: "meaning that the average mass accumulation resulting from a cold year and a warm year in succession is less than the mass accumulation of two consecutive years having the average temperature of the "warm" and "cold" years". Again, this clause just re-explains what nonlinear means. It would be much more satisfying for a reader to have the physical reasons for the nonlinearity explained. Unless the authors have a different answer and an analysis to support it, the leading reason is likely to be that the ablation rate and ablation area both change with temperature. And for approximately parabolic ice sheets, this renders ablation as approximately cubic with temperature.

Change #24: Thank you – we have removed that sentence.

> The use of left arrows in the supplementary to mean "=" is unconventional symbology. in this field, and I think is unnecessarily confusing.

Comment #27: This is a valid point, but we do think that left arrows for assignment helps distinguish between an algorithm (such as the Oer03 model) and the derivation of a result.

4   Comment by Anonymous Referee #3, June 8

Review of manuscript "Influence of temperature fluctuations on equilibrium ice sheet volume" by Troels Bøgeholm Mikkelsen, Aslak Grinsted and Peter Ditlevsen [The Cryosphere Discuss. doi:10.5194/tc-2017-47

General comments: This paper describes a study of a minimal ice sheet model of axially symmetric ice sheet resting on a bed that slopes linearly downwards from the center. Authors show that there is significant difference in the steady state volume if the model is forced with interannually variable T as compared with constant T. They further show that the effective temperature change resulting from considering fluctuating temperatures is dependent on the warming scenario. This is an interesting modelling study, but it is difficult to see the relevance of this simple model for more complex system, such as the Greenland Ice Sheet, as authors claim in lines 28-30 on page 11.

The leap from this simple model study to conclusion about equilibrium ice volume of the Greenland Ice sheet is not justified in my opinion.

Comment #28: It is likely that we did not in a sufficiently direct way explain that the output of Oer03 is *not* the main result of this paper. The study is not about the minimal ice sheet model, which is purely illustrative. The main result of the paper is equation 6, and the implications it has.

Equation 6 is valid for all models regardless of complexity. Whether it is a significant effect depend on

1. the variance of the forcing.

2. how non-linear the response is.

We first use Oer03 to illustrate how equation 6 works (and verify that it is correct). We then proceed to show that the mass-balance nonlinearity is also present in RCM results from Fettweis et al. (2013). Robinson et al. (2012) estimate a collapse threshold of Greenland at around $+1.6°C$. Using equation 7 we find that this needs to be adjusted by about $0 - -0.2°C$. This is not a huge bias considering all other uncertainties, but it is hardly insignificant considering how close we are to that threshold already.

Using Oer03 we show that the bias can result in metre scale volume differences. This can be important for 'real' ice sheet models in paleo spin-up simulations such as those from SeaRISE and initMIP/ISMIP6. The forcing used in the paleo spin up experiments is unlikely to have a realistic representation of the variance; e.g. the CISM SeaRISE setup for Greenland uses a forcing series with no data between 6400 and 0 BP, and 100 year resolution prior to that (`http://websrv.cs.umt.edu/isis/index.php/Configuration\_Files`). Similarly our results highlight that you will introduce a bias if you force the model with ensemble mean/median. This is apparently what was done for the CISM SeaRISE experiments (`http://websrv.cs.umt.edu/isis/index.php/Future\_Climate\_Data`).

The text needs much editing, and I suggest that authors rewrite most of the sections to improve the coherence of the paper. Same terms are called different names throughout the paper, which makes it very confusing to read, $\dot{V}$ defined with Equation 2 is called mass balance, dV/dt and SMB, I suggest that authors stick to one name.

Change #25: Agree – we have changed all occurrences of $\dot{V}$ to dV/dt.

The introduction section lacks coherence, it is not clear where authors are leading to, with their sentences and are jumping back and forth, for example WAIS instability is mentioned in line 20 page 1 and in again in different context in lines 13-15 page 2 and then jumping to threshold for GrIS.

Comment #29: We agree and have rewritten the intro with a stronger focus.

The result of the paper is announced in lines 14-16 on page 1, but not in any connection with the surrounding text. I suggest that complete rewriting of the introduction section be made where the current study is put in context, if that is possible (as said above it is not clear to me what the relevance of this modeling exercise is for any of the ice sheets on Earth).

Comment #30: We agree that this sentence is out of place – we have rewritten the intro.

The first figure that shows the dependence of the equation of the ELA height with specific balance is introduced but never revisited or used in the study, as the authors (line 10 page 4) "investigate the effect of interannual temperature fluctuations" before proceeding with the simple model.

Comment #31: In this section we aim to make a minimal description of the Oer03 model without going into any detail. Fig. 1 was moved from the supplementing information to the main manuscript due to a request from the editor.

The relationship between (if any) Eq. 1 and Eq. 2 is not explained and the two seem unrelated.

Comment #32: We have chosen to place the details of the Oer03 model in the supplementing information, where the relationship between equations 1 and 2 are specified. We agree that this was not clear from the manuscript.
Change #26: We specify in the manuscript that details – including the explicit connection between equations 1 and 2 – are found in the supplementing information.

Both K and °C are used, select either one.

Change #27: We have chosen °C as the temperature unit.

Specific comments – Page 1

Line 3, the time scale for weather is usually days or weeks, not interannual, it is therefore strange to call the interannual fluctuations "weather generated"

Change #28: We have changed the wording from "interannual weather generated fluctuations" to "interannual fluctuations".

> Line 4 How is the "risk of collapse" possibly underestimated when the steady state ice sheet is slightly smaller with interannual T fluctuations than constant T?

Comment #33: With no fluctuations the ice sheet is smaller (as you correctly say). A smaller ice sheet is closer to the threshold. Robinson et al. (2012) estimate the threshold at $0.8 - -3.2°C$. We calculate that if you apply our bias correction then this should be shifted by $-0.2 - -0.0°C$. I.e. we are closer to the threshold.

> Lines 5-7 This sentence is very confusing and needs clarifications.

Comment #34: We agree and have rewritten the abstract.

> Line 5, what temperature variability (spatial, temporal, interannual?) What is the relation between "recent ensemble forecasting" and "present day observed value"? How is the effect "adjusted downward", do you mean that the effect is smaller? This sentence needs rewriting.

Comment #35: We agree and have rewritten the abstract.

> Line 7 use either "predicted" or "scenario" not both

Change #29: We will stick with "scenario".

> Line 9 it is not clear what "further influence" means here, needs clarification

Change #30: We have removed the word "further"

> Line 11 clarify what "long term forecasting" means (100.000 years, 100 years?)

Comment #36: When we use the phrase "long term" to refer to the study by Robinson et al. (2012) we mean "on a time scale were a complete or substantial melt of the GrIS is feasible". Depending on the warming this will of course vary (see eg. Fig. 3b in Robinson et al. (2012)). With this in mind, we believe that the phrase "long term" is justified, even without stating a specific number of years.

> Line 14 as explained above the context of this presentation of the results is none and this sentence is strangely placed in the introduction.

Comment #37: Agree - the intro has been rewritten with a stronger focus.

> Line 20, something missing after "preindustrial" (time or value?)

Change #31: We have added "value".

> Lines 20-24 and 1-2 on page 2 strange sentences that need editing and clarification and some coherence

Change #32: We have removed this sentence when editing the introduction.

Page 2

Line 4, something is missing or badly placed () for reference, needs editing

Change #33: We removed this sentence during the editing of the introduction.

Line 5 suggest editing, the sentence is not clear ("is associated with" "likely long-term sea level rise" are strange choices - are those modeling results?

Change #34: That sentence has been removed.

Line 6 something missing after "interglacial" (period?) suggest to replace "points toward" with "suggest"

Change #35: Sentence removed.

Line 9-11 suggest editing, "corresponds to an ice free planed" seems strange here

Change #36: Sentence removed.

Lines 13-14 need editing (what is realistic future scenario?) missing reference for the statement of WAIS committed to collapse.

Change #37: Sentence removed.

Line 15 suggest to edit "within reach" do you mean "range"?

Change #38: Sentence removed.

Line 18-19 suggest editing, the context is not clear, as stated above there is lack of coherence in this whole section. Are you suggesting that your modelling study presented in the paper contributes to estimating whether ice sheet is close to a tipping point?

Change #39: Sentence removed.

Line 23 something missing, interannual variability of what?

Change #40: Changed to "interannual temperature variability".

Line 24 what is "classic" about the study by Pollard et al (1990)?

Change #41: Sentence removed.

Line 25 suggest to replace "in" with "of" delete one "constant" - what variability? (interannual? Of T?)

Change #42: Sentence removed.

Line 26 and 27 suggest to replace "expected" with "computed"

Change #43: Sentence removed.

> Line 27-29 the relationship between the simple minimal model of axially symmetric ice sheet resting on a bed that slopes linearly downwards from the centre and the Greenland surface mass balance, so this sentence is strange, needs clarification and editing

Comment #38: We have rewritten the introduction so that this sentence no longer appears. It is however clear that the Oer03 is an idealized model. To model the future of the GrIS with Oer03 would indeed be futile. We use the Oer03 as it has the practical property for the derivation of equation 6 that $dV/dt = f(T, V)$. Further, dV/dt in Oer03 has a qualitatively similar relationship to temperature (Fig. 3, left) as the model used in Robinson et al. (2012) (Fig. 3, right) and the relationship found by Fettweis et al. (2013) (Fig. 6h in that article) so we believe the use of Oer03 for the present purpose is justified.

> Line 30 do you mean spatial or temporal variability?

Change #44: We meant temporal variability – the sentence has been removed.

Page 3

> Lines 1-3 need editing, how is this related to the other text?

Comment #39: The mentioning of Fyke et al. (2014) and Roe and O'Neal (2005) were requested by a previous reviewer. As these studies concern the variability of GrIS SMB under a warming climate (Fyke et al., 2014) or the influence of fast temperature fluctuations on glacier extent (Roe and O'Neal, 2005), we believe they provide valuable context for the reader.

> Line 4, nonlinear with respect to what?

Change #45: With respect to temperature – this information has been added.

> What does "specifically avoid using monthly climatologies in order to include the effect of interannual variability" mean? Some editing is necessary.

Change #46: We have changed the wording to "Ridley et al. (2010) specifically avoid using average monthly temperature and precipitation climatologies and instead use time series from individual months in order to include the effect of interannual variability in their study."

> Line 5 and line 14 what does "climatology" mean here? Temperature and precipitation?

Comment #40: Yes, air temperature and precipitation in both cases.
    Change #47: This information has been added.

> Line 6 What kind of errors?

Change #48: We have rephrased to "At the scale of sub-annual climatology, there are large uncertainties as the estimates of the SMB differs significantly dependent on the simplifying assumptions used in the PDD formulation, highlighting the need to accurately model both spatial and temporal variability."

Line 9 Bias correction of what?

Change #49: We have changed the wording to "...necessary bias correction needed to account for temperature fluctuations in long term ice sheet simulations."

Lines 14-19 needs clarification and editing

Change #50: We have specified that the different "formulations" used by Seguinot (2013) refer to the simplifying assumptions employed in the PDD formulation.

Lines 23-24, needs editing and missing reference for the "long term ice sheet study"

Change #51: We have added a reference to Robinson et al. (2012) and specify that we investigate the effect of temperature fluctuations on their study.

Page 4

As discussed above the relationship between Eq. 1 and 2 is not explained and there seems to be none here, how is Eq. 1 applied?

Comment #41: We have chosen to place a detailed description of the model in the supplementing information, in order to not distract the reader from our main point. In the supplement, equation 1 from the manuscript concerning h_{Eq}(the altitude of the equilibrium line) is shown as equation 2; we then give a detailed description of how h_{Eq} enters the calculations.

Change #52: We have added a reference to the supplementing information between Equations 1 and 2.

Line 13, how do you know that the model is sufficient to illuminate the dynamic effect? It is not clear from the text of the paper.

Comment #42: We take the qualitative similarity of the SMB(T) curves of the Oer03, the results by Robinson et al. (2012) and the results by Fettweis et al. (2013) as an indicator that our method is justified. We do not attempt to model the GrIS with the Oer03, we use it to illustrate the effect of temperature variability.

Page 5

There is sometimes (line 1 and line3) swap of the variables in the equation f(T,V)

Change #53: We believe we have caught and eliminated all use of $f(V, T)$ so that only $f(T, V)$ remains.

Line 5 what meaning does the expectation value have?

Comment #43: The expectation value of the temperature $\overline{T} = \langle T_t \rangle$ is the time average of a fluctuating temperature time series, in this manuscript generated with equation 8. This is varied in the simulations shown in Fig. 2 where T_t can be read on the x-axis in the right panel. $\overline{V} = \langle V_t \rangle$ is the time average of ice sheet volume when the model has reached steady state. This corresponds to the y-axis in Fig. 2 (both panels).

    Change #54: We have added the following before the derivation equation 6: "In Fig. 2, $\overline{T}$ is shown on the horizontal axis in the right panel, and the corresponding $\overline{V}$ on the vertical axis (both panels)."

Line 20 replace "has" with "have" (variationS)

Change #55: Fixed.

Line 25 "tend to zero" for what condition?

Comment #44: Thank you, this was unclear in the manuscript.
    Change #56: We have added "tend to zero *as the ice sheet approaches equilibrium volume*".

Page 6

Line 10 what is Wt?

Change #57: We have added "where $W_t, t = 1, 2, \ldots$ are independent, random draws from a standard normal distribution."

Lines 12 some editing is necessary, what process?

Change #58: Thank you – we have clarified that we are referring to the *autoregressive* process defined by equation 8.

Line 14 missing reference for the value

Comment #45: This sentence has been removed in response to G. Roe's comment above. For reference, the value is the observed variance of the red curve in Fig. 2 (supplementing information).

Line 16 how do you determine that 1 year is sufficient?

Comment #46: We determine that a step size Δt used for integration of the Oer03 model to be sufficient by using the same simulated temperature time series for varying Δt (Fig. 1, supplementing information). We use Δt values of 1, 0.5, 0.1 and 0.01 years, respectively. In this way the same temperature is used for the whole year in the Oer03 model, and the model integrated with 1, 2, 10 or 100 time steps per year. As the results are practically identical based on a visual inspection of the resulting ice volume curves, we conclude that $\Delta t = 1$ year is sufficient. Please note the scale on the V-axis in Fig. 1 (supplement) compared to the Fig. 2 (article).
    Change #59: We have added a reference in the manuscript specifically to *Fig. 1* in the supplement.

Line 21 suggest to replace "lower" with "smaller"

Change #60: We have substituted "smaller".

Page 7

Figure caption missing "simulation" after "constant temperature" and "fluctuating temperature"

Change #61: Fixed.

Line 7 missing "," after specifically

Change #62: Fixed.

Line 8 suggest to replace "this" with "that"

Change #63: We agree and have made the change.

Page 8

Line 1 sentence is not clear, needs editing

Change #64: We have changed the wording to "As parameters in ice sheet models are often tuned to reproduce an observed ice sheet history from a time series of forcing observations (eg., Muresan et al. (2016)), ..."

Line 3 not clear what "this" means here

Change #65: Added "bias"

Line 6 "here we take same approach" does not make sense here, are you "com- paring the output of RCM. . ." (line 4)

Comment #47: We are referring to fitting third degree polynomials, not comparing different RCMs – this was unclear, thank you.
Change #66: Change to "We will follow Fettweis et al. (2013) and to the ensemble of simulations in Robinson et al. (2012) fit third degree polynomials ..."

Page 9

Line 11 red curveS .. show (delete s)

Change #67: Fixed, thank you.

Line 12 blue shaded

Change #68: Typo fixed, thank you.

Line 16 something missing after "preindustrial"

Change #69: Added "period" after "preindustrial".

Line 19-21 this is confusing, if the SMB of Greenland is -234 GT/a and 3°C warming will cause DeltaSMB to be 30 Gt/y, what does it mean for current mass loss?

Comment #48: We provide the SMB of Greenland to put the magnitude of the effect we describe into context. What temperature fluctuations mean for the current SMB is a very interesting question. We do not presume to answer this question in the present manuscript; to determine this would likely require model studies employing more sophisticated ice sheet models than the Oer03 used here, and we hope that such studies will be carried out in the future. We agree that the phrasing was unclear and have changed it accordingly.

Change #70: We have changed the wording from to "Fig. 4 (bottom) shows the most likely ΔSMB resulting from temperature fluctuations at a 3°C warming to be 30 Gt/y (24 Gt/y – 59 Gt/y, 95% credibility) or – for context – 30 Gt/y (24 Gt/y – 59 Gt/y, 95% credibility) of the average GrIS SMB of -234 ± 20 Gt/y reported for the period 2003–2011 (Barletta et al., 2013)."

Line 22 suggest to replace "saturate" with "reach a constant value"

Change #71: This change has been made.

Page 10

Figure caption, explain what deltaT and deltaSMB mean in this context

Change #72: We have added this description here.

Line 5-6 text needs editing, it is not clear what are meant here.

Change #73: We have edited to "Temperature fluctuations can be accounted for in ice sheet modeling studies, either explicitly (eg. Ridley et al. (2010); Seguinot (2013)) or implicitly, as happens when tuning the ice sheet model to reproduce an observed ice sheet history with observed forcing as input (eg. Muresan et al. (2016))."

Page 11

Page 11 line 2 Suggest to add "simulations" after ice sheet model

Change #74: We agree and have changed the text accordingly.

line 11 as explained above replace "mass balance" with , $\dot{V}$ or dV/dt

Comment #49: The use of "mass balance", $\dot{V}$ etc. has been cleaned up substantially as per your previous request – in this particular place we think it is better to be verbose.

lines 11-14 need editing, confusing sentence

Change #75: We have agree and have rewritten to Conclusion.

lines 20-30 needs editing, the relationship of the model result to reality is not clear or justified.

Comment #50: As previously stated, the qualitative similarity of the SMB(T) curves in Oer03, Robinson et al. (2012) and Fettweis et al. (2013) justifies our approach; the SMB $\sim T^3$ relationship is further supported by Roe and Lindzen (2001). However, the conclusion has been rewritten, as detailed above in the answer to G. Roe; we repeat the relevant part for convenience: "We find that the steady state ice sheet volume in Oer03 is $0.5 - 1$ m$_{\text{SLE}}$ smaller when the minimal model is forced with fluctuating temperatures compared to constant temperature (Fig. 2). It is therefore necessary to consider the impact of temperature variability when designing long-term model experiments such as paleo spin-ups (eg. Bindschadler et al. (2013); Golledge et al. (2015); Nowicki et al. (2016)), especially when downsampling the paleo forcing series. Furthermore, models of sub-shelf melting, grounding line migration, and ice discharge have the potential to respond non-linearly to changes in ocean temperatures (Favier et al., 2014; Joughin et al., 2014; Seroussi et al., 2014; Mengel and Levermann, 2014; Pollard et al., 2015; Fogwill et al., 2014), thus it is critical to take variability into account for quantitative assessments."

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Influence of temperature fluctuations on equilibrium ice sheet volume

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Abstract. Forecasting the future sea level relies on accurate modeling of the response of the Greenland and Antarctic ice sheets to changing temperatures. The surface mass balance of the Greenland ice sheet has a ~~non-linear~~ nonlinear response to warming. Cold and warm anomalies of equal size do not cancel out ~~. It~~ and it is therefore important to consider the effect of interannual fluctuations in temperature. ~~We show how the magnitude of the potential bias can be quantified statistically. We estimate the~~
5 ~~bias to be 30 Gt/y for a 3 °C above pre-industrial, or 13% of the present day rate of ice loss. The mass balance bias also has implications for the equilibrium size of the ice sheet.~~ We find that the steady state volume of an ice sheet is biased toward larger size if temporal temperature fluctuations are not taken into account in numerical modeling of the ice sheet. We illustrate this in a simple ice sheet model and find that the equilibrium ~~volume is~~ ice volume is approximately 1 ~~m~~ meter sea level equivalent smaller when forced with ~~fluctating versus~~ fluctuating temperatures as opposed to a stable climate. It is therefore important to
10 consider the effect of temporal temperature fluctuations when designing long experiments such as paleo spin-ups. We show how the magnitude of the potential bias can be quantified statistically. We estimate the bias to be 30 Gt/y (24 Gt/y – 59 Gt/y, 95% credibility) for a warming of 3 °C above preindustrial values, or 13% (10% – 25%, 95% credibility) of the present day rate of ice loss. Models of the Greenland ice sheet show a 'collapse' threshold beyond which the ice sheet becomes unsustainable. The proximity of the threshold will be underestimated if temperature fluctuations ~~have not been~~ are not taken into account. We
15 estimate the bias to be ~~0 – –0.2 °C~~ 0.12 °C (0.10 °C – 0.18 °C, 95% credibility) for a recent estimate of the threshold. Many ~~predicted~~ scenarios of the future climate show an increased variability in temperature over much of the Earth. In light of our findings it is important to gauge the extent to which this increased variability will ~~further~~ influence the mass balance of the ice sheets.

1   Introduction

20 ~~Using coupled climate and~~ Ice sheet mass balance has a nonlinear dependence on temperature. This behavior is observed in simple ice sheet models ~~, long time forecasting is often made computationally feasible by running a climate model for one or more years and then repeatedly applying the climate (or the surface mass balance computed from it) to an ice sheet model~~ (Weertman, 1961, 1964, 1976; Roe and Lindzen, 2001), in full regional climate modeling of Greenland surface mass balance (Fettweis et al., 2013), and the nonlinear effect of temperature on melt has been observed in Greenland river discharge
25 (van As et al., 2017).

Ice sheets are characterized by a large interior plateau flanked by comparatively steeper margins. A warming will shift the equilibrium line altitude (ELA) to higher elevations, increasing the area exposed to melt. The area exposed to melt will increase nonlinearly with ELA because of the top-heavy hypsometry (van As et al., 2017). This mechanism explains the nonlinear dependence of mass balance on temperature for ice sheets where run-off is a significant fraction of the total mass balance. This

5 mechanism is important for the mass balance of present-day Greenland, but less so for present-day Antarctica where mass loss is dominated solid ice discharge (Church et al., 2013, p. 1170). There is, however, some concern that Antarctic ice discharge and total mass balance may be highly nonlinear. The potential for a large nonlinear response of Antarctic mass balance is particularly evident in the simulations from Pollard et al. (2015).

The nonlinear relationship between mass balance and warming means that there is an asymmetry in the response from cold

10 versus warm anomalies. The average mass balance will be different when forcing the model with a variable climate compared to a constant average climate. Simulations using constant climate will therefore be biased unless they make statistical corrections to allow for variance. Constant climate forcing is sometimes used to trace the long term equilibrium response of ice sheet models as a function of temperature (e.g. Robinson et al. (2012)).

~~(Kageyama et al., 2004; Gregory et al., 2012; Ziemen et al., 2014). Some studies (e. g. Kageyama et al. (2004)) compute the~~

15 ~~surface mass balance from a climatology. The present analysis shows that computing the surface mass balance from a climatology can result in a bias towards a larger ice sheet size, if the surface mass balance is estimated assuming a yearly averaged temperature .~~

Ice sheet modeling and evidence from paleoclimatic records indicate that ice sheets display a hysteresis response to climate forcing (Abe-Ouchi et al., 2013; Robinson et al., 2012). There is a critical threshold in temperature, a tipping point, beyond

20 which an ice sheet becomes unsustainable. This is a generic saddle-node bifurcation point, estimated by Robinson et al. (2012) to be reached for the Greenland Ice Sheet (GrIS) at a global warming of $+1.6°C$ ($0.8°C - 3.2°C$) above ~~preindustrial~~the preindustrial value.

~~Several recent studies suggest that parts of the West Antarctic Ice Sheet (WAIS) may already have been destabilized (Favier et al., 2014; Joughin et al., 2014; Rignot et al., 2014; Mouginot et al., 2014; Seroussi et al., 2014). Other studies find that~~

25 ~~East Antarctica may be more vulnerable to warming than previously thought (Mengel and Levermann, 2014; Greenbaum et al., 2015; Sun e . There is a growing concern for a considerable risk of a marine ice-sheet instability of the WAIS may lead to a substantial sea level rise contribution already this century (Bamber and Aspinall, 2013).~~

~~Paleoclimatic records show a nonlinear relationship between temperature increase and sea level rise consistent with the threshold behavior of ice sheets, predicted by modeling studies. Gasson et al. (2012); Foster and Rohling (2012) find that even~~

30 ~~a moderate global warming of $+2°C$ or CO_2 levels of 400 ppm is associated with a likely long-term sea level rise of more than 9 m. This is consistent with evidence from the last interglacial which points toward a collapse of the WAIS (Kopp et al., 2009; Dahl-Jensen et al., 2013; Strugnell et al., 2012). Likewise there is evidence for at least one substantial deglaciation period in Greenland having occurred during the past 1.1 million years (Blard et al., 2016; Bierman et al., 2016; Schaefer et al., 2016) .~~

The greenhouse gas concentrations and intense warming in high-end scenarios such as ECP8.5 (Extended Concentration Pathways, extension of Representative Concentration Pathways beyond 2100) (Meinshausen et al., 2011) correspond to an ice-free planet in the paleoclimatic record (Gasson et al., 2012; Foster and Rohling, 2012) which evidence suggests was the case until approximately 35 million years ago (Ruddiman, 2014).

5 Observations, paleoclimatic records and model studies indicate a real risk of ice sheet collapse for realistic future scenarios global warming. A substantial part of WAIS may already be committed to collapse. The threshold for GrIS is estimated to be passed in ECP4.5 and ECP6, and even total deglaciation is within reach of the ECP8.5 scenario. The complete loss of the Greenland –, the West Antarctic –, and the East Antarctic ice sheets would raise global sea levels by 7.4 m, 4.3 m, and 53 m respectively, excluding any solid earth rebound effects that would take place during ice sheet decay (Bamber et al., 2013; Fretwell et al., 201

10 . The risk that global warming might exceed the tipping points of ice sheet stability pose an existential threat to low lying coastal nations. Estimating how close each ice sheet is to a tipping point is thus critically important.

The stability of ice sheets is typically investigated by imposing a constant climate forcing and then letting the ice sheet model reach equilibrium (Robinson et al., 2012; Solgaard and Langen, 2012; Huybrechts and de Wolde, 1999)(Huybrechts and de Wolde, 1999; l . The hysteresis curve , and collapse thresholds – are then traced out by repeating these experiments for a range of tempera-

15 tures and starting from ice free conditions. However, this approach disregards the effects of interannual temperature variability.

In the classical study of the effect of asynchronous coupling by Pollard et al. (1990) it was noticed that a stochastic forcing in an ice sheet model results in a smaller ice sheet in comparison to a constant constant forcing. Here we show how variability in forcing changes the expected mass balance of an ice sheet. We develop a general theoretical framework for how forcing variability impact the expected response in a model that exhibits a non-linear response. We illustrate the importance using a

20 minimal model of how Greenland surface mass balance responds to temperature fluctuations. The simple model is also used to assess the bias adjustments needed in model studies when constant forcing is applied.

Though some studies implement full GCM coupling to the ice sheet model, or have some mixed approaches (Ridley et al., 2005) (Gregory and Huybrechts, 2006), the computational demand of the GCM could come at an expense for the resolution of the ice sheet flow model. The results presented here show explicitly how to account for the effect of unresolved temperature variability.

25

Previous studies of natural variability in the context of ice sheets include Fyke et al. (2014), who find that the variability of the GrIS surface mass balance will increase in a warmer climate due to increased ablation area, and Roe and O'Neal (2005) who find that large fluctuations in glacier extent can be driven by natural, fast fluctuations in climate.

That the SMB of an ice sheet model is nonlinear with respect to temperature is well known. In a simplified model of

30 continental ice sheets, Roe and Lindzen (2001) show that the total annual ablation scales with the cube of temperature at the ice sheet margin. Ridley et al. (2010) specifically avoid using monthly climatologies average monthly temperature and precipitation climatologies and instead use time series from individual months in order to include the effect of interannual variability in their study. Seguinot (2013) shows how simplifying assumptions (in general leading to lower temperature variability) in a positive degree day (PDD) scheme leads to errors. Fettweis et al. (2013, see Fig. 6h) investigate the GrIS SMB simulated by regional

35 climate models (RCM) as a function of mean surface temperature from general circulation models (GCM). Our contribution is

a quantification of this effect, and an estimate of the necessary bias correction needed to account for temperature fluctuations in long term ice sheet simulations.

Sub-annual temperature variability in the context of positive degree-day (PDD) models is investigated in eg. ~~Hock (2003); Seguinot (2013~~ Arnold and MacKay (1964); Reeh (1991); Hock (2003); Calov and Greve (2005); Seguinot (2013); Wake and Marshall (2015)

5 . PDD models connect surface melting and air temperature, and are used extensively due to their simplicity and wide availability of air temperature data (Hock, 2003). Seguinot (2013) compares Greenland SMB calculated from four different annual PDD formulations with a reference SMB calculated from a PDD scheme using a monthly air temperature and precipitation climatology and deviations from an long-term interannual mean. At the scale of sub-annual climatology, there are large uncertainties as the estimates of the SMB differs significantly ~~among the formulations,~~ dependent on the simplifying assumptions used in

10 the PDD formulation, highlighting the need to accurately model both spatial and temporal variability. These findings are built upon by Wake and Marshall (2015) who find that the standard deviation of monthly average temperature may be represented as a quadratic function of monthly average temperature. In the present study we are concerned with interannual variability and expect our results to apply independently of the chosen SMB model.

We investigate how climate variability influence the mass balance of ice sheets with a nonlinear response to climate forcing.

15 We derive a simple statistical relationship which can be used to quantify the effect, and illustrate why it matters on a minimal ice sheet model. We then proceed to show how this may applied to published results from a coupled ice sheet model. In Section 2 we derive an analytical relationship between the magnitude of temperature fluctuations assuming a simple relationship between the mass balance, temperature and ice sheet volume. This relationship is shown to hold using a simple ice sheet model (~~including~~ that includes a surface mass balance model) in Section 3, and in Section 4 we estimate the consequences of temper-

20 ature fluctuations on a recent long term ice sheet study (Robinson et al., 2012), assuming the effect ~~we present~~ of temperature fluctuation presented here is not already accounted for. The limitations of this approach, as well as further possible applications, ~~is~~ are discussed in Section 5.

2 The Mass Balance of an Ice Sheet

2.1 A Minimal Ice Sheet Model

25 ~~We~~ In order to investigate the influence of temperature fluctuations on the mass balance we consider a simple ice sheet model introduced by Oerlemans (2003) hereafter denoted *Oer03*. This model describes the essential dynamics of an ice sheet ~~initiated from a mountain glacier. It assumes an axially symmetric ice sheet~~ assumed to be axially symmetric and resting on a bed that slopes linearly downwards from the center. The ice is modeled as a perfectly plastic material, and the ice sheet is coupled to the surrounding climate by adjusting the height of the equilibrium line h_{Eq} (Oerlemans, 2008):

30 $$h_{Eq} = h_{E,0} + (T - \bar{T}) \cdot 1000/6.5. \tag{1}$$

Equation 1 represents an increase of the equilibrium line altitude of roughly 154 m $^\circ$C^{-1}. The influence of h_{Eq} on the specific balance B is illustrated in Fig. (1). It should be noted that ~~this simple relationship~~ the simple relationship described

4

by Eq. (1) does not capture situations where the SMB may *increase* with increasing temperature, as discussed in Section 5. Further details of the Oer03 allowing the formulation in Eq. 2 below are described in the supplementing information.
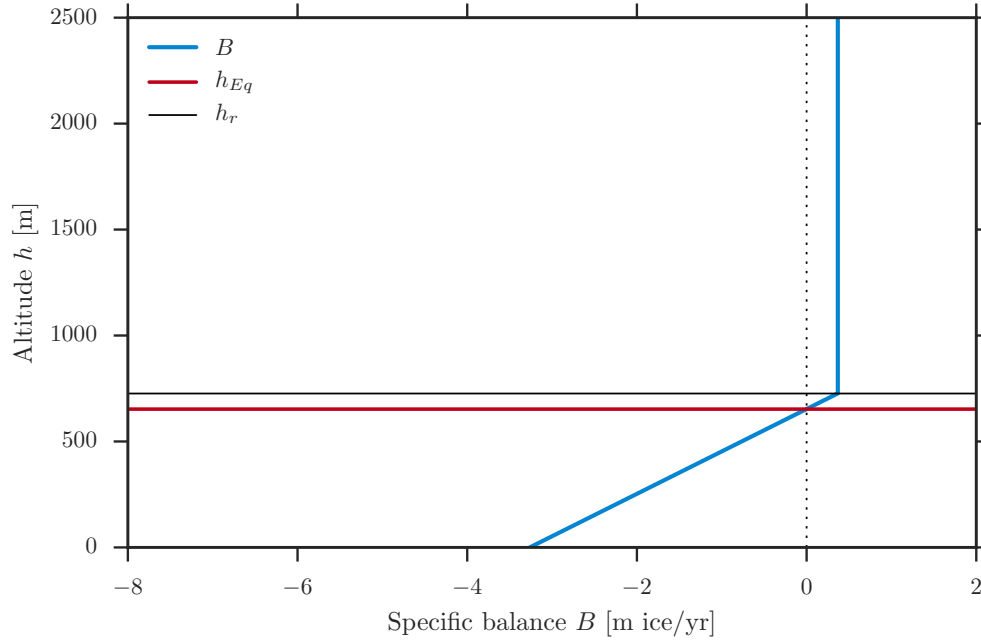

Figure 1. Specific balance B for $\overline{T} = 0$ from Eq. (1) using the parameters in Table 1 and Eqs. (3–4) of the supplement. h_{Eq} denotes the equilibrium line. The specific balance is constant above the runoff line h_r (Supplementing Information, Eq. (4)), and the balance gradient is constant below h_r (Oerlemans, 2003).

The model is chosen for its simplicity, thus it is not accurately modeling a specific ice sheet; the two main reasons for choosing it for our analysis are: 1) The simplicity of Oer03 allows the analytical approach detailed below and 2) the Oer03 model shows the same functional relationship between surface mass balance (SMB) and temperature as has been found for regional climate models (RCM) for a range of temperature scenarios (Fettweis et al., 2013). The change in volume or mass of the ice sheet depends on the balance between accumulation, ablation and ice sheet discharge which in turn depends on both the interplay between the fluctuating temperature and the state of the ice sheet itself.

Before proceeding with the simple model, we investigate the effect of interannual temperature fluctuations by considering the ice sheet as a simple dynamical system. Assume the mass balance of the ice sheet to depend only on the volume V itself and a single time-varying mean temperature over the ice sheet, T; ~~thus all components of the mass budget are uniquely determined by temperature and volume. This is a vast simplification but sufficient to illuminate the essential dynamical effect we consider in this paper.~~ Denoting the mass balance (change in ice sheet volume) as ~~$\dot{V}$,~~ dV/dt,

$$dV/dt = f(T, V), \tag{2}$$

5

where $f(V,T)$ is some non-linear where $f(T,V)$ is some nonlinear function. The (stable) fixed point, $f(T,V) = 0$ corresponds to a balance between loss and gain in the ice volume. This is in general an implicit equation to determine the steady state volume $V_0(T)$ as a function of temperature, such that $f(V_0(T),T) = 0$ $f(T, V_0(T)) = 0$.

However, the fixed point is not identical to the statistically steady state volume with a temporally fluctuating temperature
5    $T_t = T(t)$ with expectation value $\langle T_t \rangle = \overline{T}$. A numerical integration to equilibrium of an ice sheet model with and without interannual fluctuating temperature shows that in steady state the ice sheet volume V_t will fluctuate around $\langle V_t \rangle = \overline{V}$ where $\overline{V}$ is systematically smaller than the corresponding $V_0(T)$ (Fig. 2). In Fig. 2, $\overline{T}$ is shown on the horizontal axis in the right panel, and the corresponding $\overline{V}$ on the vertical axis (both panels).

Since the temperature T_t – and thus the ice sheet volume V_t – is a stochastic variable the following will characterize an
10   equilibrium state:

$$\langle f(T_t, V_t) \rangle = 0. \tag{3}$$

To calculate $\overline{V}$ we perform a Taylor expansion of Eq. (3) around the – presently unknown – steady state $(\overline{T}, \overline{V})$ and calculate the mean volume $\overline{V}$. We use the notation $f_T := \frac{\partial f}{\partial T}$, $f_{TV} := \frac{\partial^2 f}{\partial T \partial V}$, etc. Furthermore, $f^0 := f(\overline{T}, \overline{V})$, $f_T^0 := \frac{\partial f}{\partial T}(T,V)\big|_{(\overline{T}, \overline{V})}$ etc. We then get:

15    $\langle f(T_t, V_t) \rangle \quad = f^0 + \langle T_t - \overline{T} \rangle f_T^0 + \langle V_t - \overline{V} \rangle f_V^0 + \frac{1}{2} \langle (T_t - \overline{T})^2 \rangle f_{TT}^0$

$$+ \frac{1}{2} \langle (V_t - \overline{V})^2 \rangle f_{VV}^0 + \langle (T_t - \overline{T})(V_t - \overline{V}) \rangle f_{TV}^0 + \mathcal{O}(3), \tag{4}$$

where $\mathcal{O}(3)$ represents higher order terms.

We can simplify Eq. (4) considerably: First note that since $\overline{T}$ is the expectation value of T_t we have $\langle T_t - \overline{T} \rangle = \langle T_t \rangle - \overline{T} = \overline{T} - \overline{T} = 0$ and with the same argument $\langle V_t - \overline{V} \rangle = 0$. The quantity $\langle (T_t - \overline{T})^2 \rangle$ is the variance of the fluctuating temperature
20   – we will assume this is known in simulations and substitute $\langle (T_t - \overline{T})^2 \rangle = \sigma_T^2$. Since the temperature variations are small with respect to the mean and has have a symmetric distribution we may neglect higher order terms in Eq. (4) (Rodriguez and Tuckwell, 1996). We are left with:

$\langle f(T_t, V_t) \rangle \quad \approx f^0 + \frac{\sigma_T^2}{2} f_{TT}^0$

$$+ \frac{1}{2} \langle (V_t - \overline{V})^2 \rangle f_{VV}^0 + \langle (T_t - \overline{T})(V_t - \overline{V}) \rangle f_{TV}^0. \tag{5}$$

25   We have evaluated the last two terms in Eq. (5) numerically for the model presented in Section 3 and found that $\langle (V_t - \overline{V})^2 \rangle$ and $\langle (T_t - \overline{T})(V_t - \overline{V}) \rangle$ tend to zero (as the ice sheet approaches equilibrium volume (Fig. 3, supplementing information) – neglecting the last two terms, Eq. (5) reduces to

$$\langle f(T_t, V_t) \rangle \approx f^0 + \frac{\sigma_T^2}{2} f_{TT}^0. \tag{6}$$

Equation (6) is the main observation in this work. We shall in the following estimate the implications of this result on realistic
30   asynchronously coupled state-of-the-art ice sheet climate model simulations. As $\langle f(T_t, V_t) \rangle = 0$ at the steady state it can be

seen from Eq. (6) that

$$0 = \quad f^0 + \frac{\sigma_T^2}{2} f_{TT}^0 \Rightarrow$$

$$f^0 = \quad -\frac{\sigma_T^2}{2} f_{TT}^0 > 0 \tag{7}$$

since $f_{TT}^0 < 0$ – this negative curvature of f^0 is the nonlinear effect causing the bias. $V_0(T)$ is the stable fixed point; ~~$f(V_0(T),T)=0$, thus $f(V,T)>0$~~ $f(T,V_0(T))=0$, thus $f(T,V)>0$ for $V < V_0$ and ~~$f(V,T)<0$~~ $f(T,V)<0$ for $V > V_0$. This together with
Eq. (7) implies that $\overline{V} < V_0$; that is, a positive temperature anomaly increases the mass loss more than what can be compensated
by an equally large negative anomaly (van de Wal and Oerlemans, 1994).

3 Ice Sheet Simulations

3.1 Fluctuating Temperatures

To generate an ensemble of volume simulations we use time series T_t comparable to the observed temperatures over Greenland
between year 1851 and 2011. For this we use the AR(1)-process (Hasselmann, 1976; Frankignoul and Hasselmann, 1977; von
Storch and Zwiers, 2003; Mudelsee, 2010):

$$T_{t+1} = \overline{T} + a \times (T_t - \overline{T}) + \sigma_{AR} W_t, \tag{8}$$

where $W_t, t = 1, 2, \ldots$ are independent, random draws from a standard normal distribution. The exact form of the model used
for generating temperature time series T_t is of less importance than the variance of the resulting T_t as only the variance enters
into Eq. (6).

The parameters (a, σ_{AR}^2) were obtained by fitting Eq. (8) to the observed annual mean temperatures over Greenland between
~~year~~ years 1851 and 2011 (supplementing information). We obtain $(a, \sigma_{AR}^2) = (0.67, 0.85)$ thus the ~~process~~ AR(1)-process Eq.
(8) has variance (Box et al., 2008) $\sigma_T^2 = \sigma_{AR}^2/(1-a^2) = 1.54$ $\mathrm{K}°\mathrm{C}^2$. ~~comparable to the observed annual mean temperature variance over Greenland, $\sigma_{T,obs}^2 = 1.55\,\mathrm{K}^2$.~~

As we quantify the effect of interannual stochastic variability we use ~~yearly~~ annually averaged temperatures, consistent with
the formulation of the Oer03 model (cf. Table 1 of the Supplementing Information). We find time step size of one year to be
sufficient for integrating the Oer03-model (Fig. 1, supplementing information); thus T_{t+1} in Eq. (8) represents the temperature
one year after T_t.

To find the steady state volume we run the Oer03-model forward long enough for the ice sheet to reach equilibrium, with and
without fluctuating temperatures. The results of this procedure are shown in Fig. 2 (left) where it is clearly seen that the steady
state volume is ~~lower~~ smaller for simulations with fluctuating temperatures than with constant temperature. We emphasize that
the fluctuating temperature time series $\{T_t\}$ have as mean the constant temperature, $\langle T_t \rangle = \overline{T}$ so that the differences are due
only to the annual temperature fluctuation.

In Fig. 2 (right) the effect of temperature fluctuations is shown in the (T, V)-plane: the markers "+" are steady states of
numerical simulations with constant temperature, while the circles represent ensemble averages of simulations with fluctuating
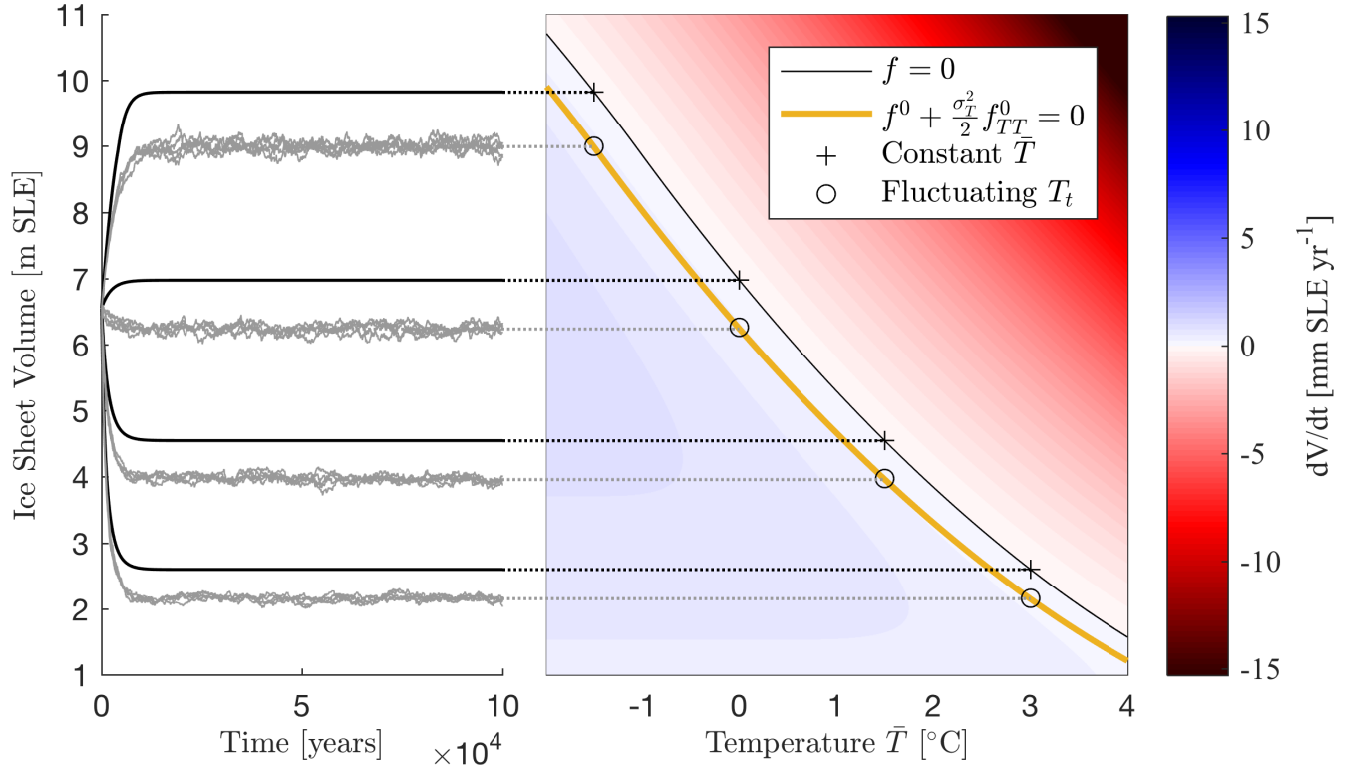
Figure 2. (Left) Simulations of the Oer03-model for $\overline{T} = -1.5, 0, 1.5$ and 3. The black curves denote a constant temperature and the grey curves fluctuating temperatures generated with Eq. (8). (Right) The mass balance Eq. (2) for the Oer03-model in the (T, V)-plane. The black contour is the steady state $f = dV/dt = 0$. The markers represent the average of the numerical simulation with constant $(+)$ and fluctuating $(\circ)$ temperature seen on the left. Finally the yellow contour shows the approximation derived in in Eq. (6).

temperatures. It is evident that temperature fluctuations decrease the steady state ice volume. The yellow curve in Fig. 2 (right) was calculated using Eq. (6) and gives a good agreement with the results from ensemble simulations.

In order to illustrate the physics behind Eq. (6), consider values of the mass budget function f for different ice sheet volumes V, shown in Fig. 3. The insert shows, for a particular value of V, how the steady state is influenced by fluctuating temperatures:

5   the average mass budget of a colder year and a warmer year is less than the mass budget of a year with a temperature corresponding to the average of "cold" and "warm"; to put it another way: the increased SMB of a single anomalously cold year cannot balance the increased melt from an equally anomalously warm year (van de Wal and Oerlemans, 1994). In particular let $T_c = \overline{T} - \sigma$ and $T_h = \overline{T} + \sigma$:

$$\frac{f(V, T_c) + f(V, T_h)}{2} \frac{f(T_c, V) + f(T_h, V)}{2} < f\left(V, \frac{T_c + T_h}{2}, V\right), \tag{9}$$

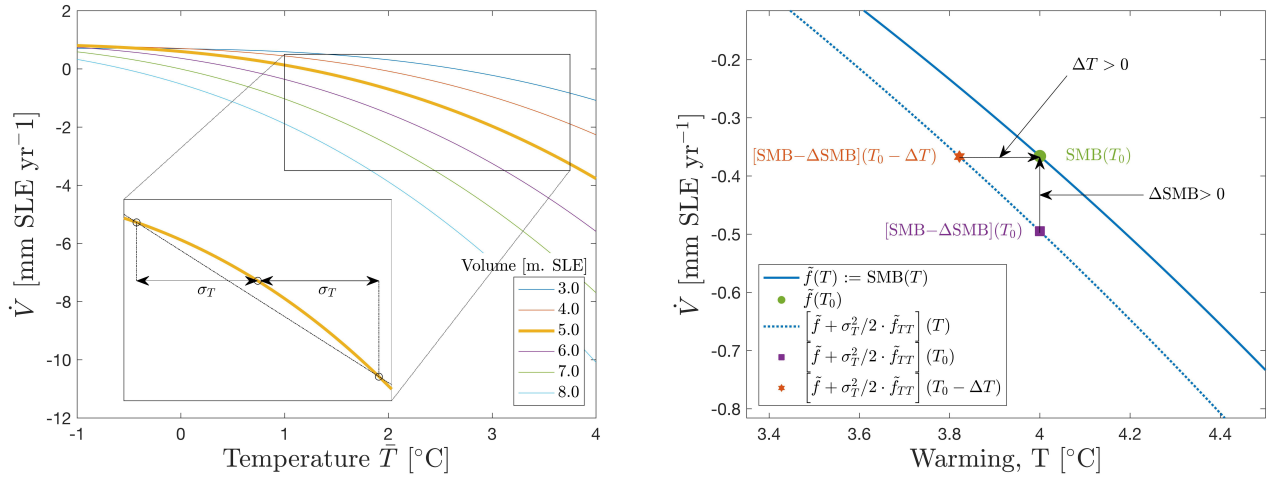10   which is consistent with $f^0_{TT} < 0$ as shown in Eq. (7).

Figure 3. Left: Mass balance $\dot{V}\ dV/dt$ of the ice sheet for different values of the total ice sheet ice volume V in the Oer03-model. Similar to Fig. 2 but here we show $\dot{V}\ dV/dt$ as a function of $\overline{T}$ for different total volumes V. **Insert, left:** The curvature of $\dot{V}(\overline{T})\ dV(\overline{T})/dt$ influences the steady state behavior – a cold year does not cancel out the effect of an equally warm year as shown in Eq. 9. The value of σ_T is used for illustration and is given as the square root of the temperature variance, $\sigma_T = \sqrt{1.54\text{K}^2} = 1.24\text{K}\ \sigma_T = \sqrt{1.54°\text{C}^2} = 1.24°\text{C}$. Note the similarity of the $\dot{V}(\overline{T})\ dV(\overline{T})/dt$ found here to Fig. 6h in Fettweis et al. (2013). **Right:** Estimating the effect of fluctuating temperatures on GrIS projections. The full curve is obtained by fitting a third degree polynomial $\tilde{f}(T)$ to an SMB(T) from Robinson et al. (2012). The dotted line show the effect of temperature fluctuations obtained by applying Eq. (6). For a warming of $4°$C the green circle shows the SMB. ΔSMB is obtained by applying Eq. (11) and represents the change in mass balance resulting from the temperature fluctuations. $-\Delta T$ is the temperature change required to negate this effect and is obtained implicitly from Eq. (12).

4   Consequences for Long Term Ice Sheet Simulations

Here we investigate the effect of accounting for fluctuating temperatures when running long time scale climate simulations. These can be either transient runs, scenarios with specified changing CO_2-forcing or equilibrium runs with specified constant forcing. Specifically, we analyze the results of Robinson et al. (2012) where the long term stability of the GrIS is investigated. In ~~this~~ that study, an ice sheet model is forced by the output of a regional climate model driven by the ERA40 climatology with a constant temperature anomaly applied, see Robinson et al. (2012) and Supplementary Information.

As parameters in ice sheet models are often tuned to ~~best match the problem under investigation~~ reproduce an observed ice sheet history from a time series of forcing observations (eg., Muresan et al. (2016)), the ice sheet volume bias we describe may already be implicitly compensated. To estimate the size of the temperature fluctuation bias, we assume that this ~~has not~~ bias has *not* already been accounted for by parameter tuning.

Fettweis et al. (2013) compare the output of RCMs forced with multiple future climate scenarios and show that the effect of rising temperature on the GrIS SMB is well described by a third degree polynomial(, consistent with the aforementioned findings of Roe and Lindzen (2001). The reader may note the qualitative similarities between Fig. 3 in the present article and

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Fig. 6h in Fettweis et al. (2013)~~)~~. ~~Here we take the same approach. To the~~ We will follow Fettweis et al. (2013) and to the ensemble of simulations in Robinson et al. (2012) ~~we~~ fit third degree polynomials to the SMB as a function of temperature at time $t = 200$ years (see also the supplementing information) and obtain third degree polynomials in T:

$$\left\{\tilde{f}_{ij}(T)\Big|\tilde{f}_{ij}(T) = A_{ij}T^3 + B_{ij}T^2 + C_{ij}T + D_{ij}\right\} \tag{10}$$

where the indices i and j run over two separate parameters in the model that take 9 – respectively 11 –values (Robinson et al., 2012) so in total we have 99 unique polynomial fits. These polynomials are then used as a simple description of the mass balance function as a function of temperature, $\text{SMB}_{ij}(T) = \tilde{f}_{ij}(T)$. Differentiating twice we obtain $\tilde{f}_{TT}(T) = 6AT + 2B$ (suppressing indices i, j for clarity).

For all parameter pairs (i, j) we evaluate $\tilde{f}(T)$ and $\tilde{f}(T) + (\sigma_T^2/2)\tilde{f}_{TT}(T)$ – this is shown in Fig. 3 (right) as the full and dotted lines, respectively.

To illustrate this approach we pick a specific temperature T_0. $\tilde{f}(T_0)$ is thus the SMB for a constant temperature and $\tilde{f}(T_0) + (\sigma_T^2/2)\tilde{f}_{TT}(T_0)$ represents the effect of letting the temperatures fluctuate. This procedure gives us an expression for ΔSMB

$$\begin{aligned}\Delta\text{SMB} = \quad & \tilde{f}(T_0) - \left[\tilde{f}(T_0) + \frac{\sigma_T^2}{2}\tilde{f}_{TT}(T_0)\right] \\ = \quad & -\frac{\sigma_T^2}{2}\tilde{f}_{TT}(T_0)\end{aligned} \tag{11}$$

where ΔSMB is positive in accordance with Eq. (7). Next we find the temperature difference ΔT such that

$$\tilde{f}(T_0 - \Delta T) + \frac{\sigma_T^2}{2}\tilde{f}_{TT}(T_0 - \Delta T) = \tilde{f}(T_0). \tag{12}$$

In this way ΔT is the *effective* temperature change resulting from considering fluctuating temperatures.

The results of applying the steps outlined above on the data from Robinson et al. (2012) are shown in Fig. 4~~(see also supplementing information)~~. The red curves in Fig. 4 ~~shows~~ show the most likely ΔT and ΔSMB; the grey curves are estimates for the 9×11 individual parameter values and the blue ~~shade~~ shaded area represents the 95% credibility region.

The warmings quoted in Robinson et al. (2012) are relative to the preindustrial period whereas the reported warming from the preindustrial period to the present day is estimated to 1°C (Stocker et al., 2013, p. 78). Following the RCP45 scenario it is *more likely than not* that Earth will experience a further warming of 2.0°C (IPCC, 2013, p. 21) from today to the year 2100. ~~Combing~~ Combining these numbers we arrive at a warming of 3.0°C in the year 2100 relative to the preindustrial period when considering the RCP45 scenario. For this value it is seen in Fig. 4 (top) that an additional ~~0.12°C~~ 0.12 °C (0.10 °C – 0.18 °C, 95% credibility) should be added to any constant warming term when considering simulations of the Greenland ice sheet, assuming the same temperature variance as in Section 3. We note that this bias correction is small compared to the spread in temperature projections. Nevertheless this is a known bias that should be accounted for. The threshold for GrIS ice loss has been estimated to be at $+1.6^\circ$C (0.8°C – 3.2°C) (Robinson et al., 2012). Our results indicate that the threshold for GrIS may be 0.12 °C (0.1 °C – 0.18 °C) colder (Fig. 4, top). This is not a large adjustment considering other uncertainties, but it reduces the window available to avoid passing this threshold and the corresponding multi-millennial sea level commitment. ~~Further,~~

Fig. 4 (bottom) shows the most likely ΔSMB resulting from temperature fluctuations at a 3°C warming to be 30 Gt/y ~~–(24~~ ~~Gt/y – 59 Gt/y, 95% credibility)~~ ~~To put this number in context , consider Barletta et al. (2013) who report an~~ or ~ for context ~ ~~30 Gt/y (24 Gt/y – 59 Gt/y, 95% credibility) of the~~ average GrIS SMB of -234 ± 20 Gt/y reported for the period ~~2003 to 2011.~~ 2003–2011 (Barletta et al., 2013).
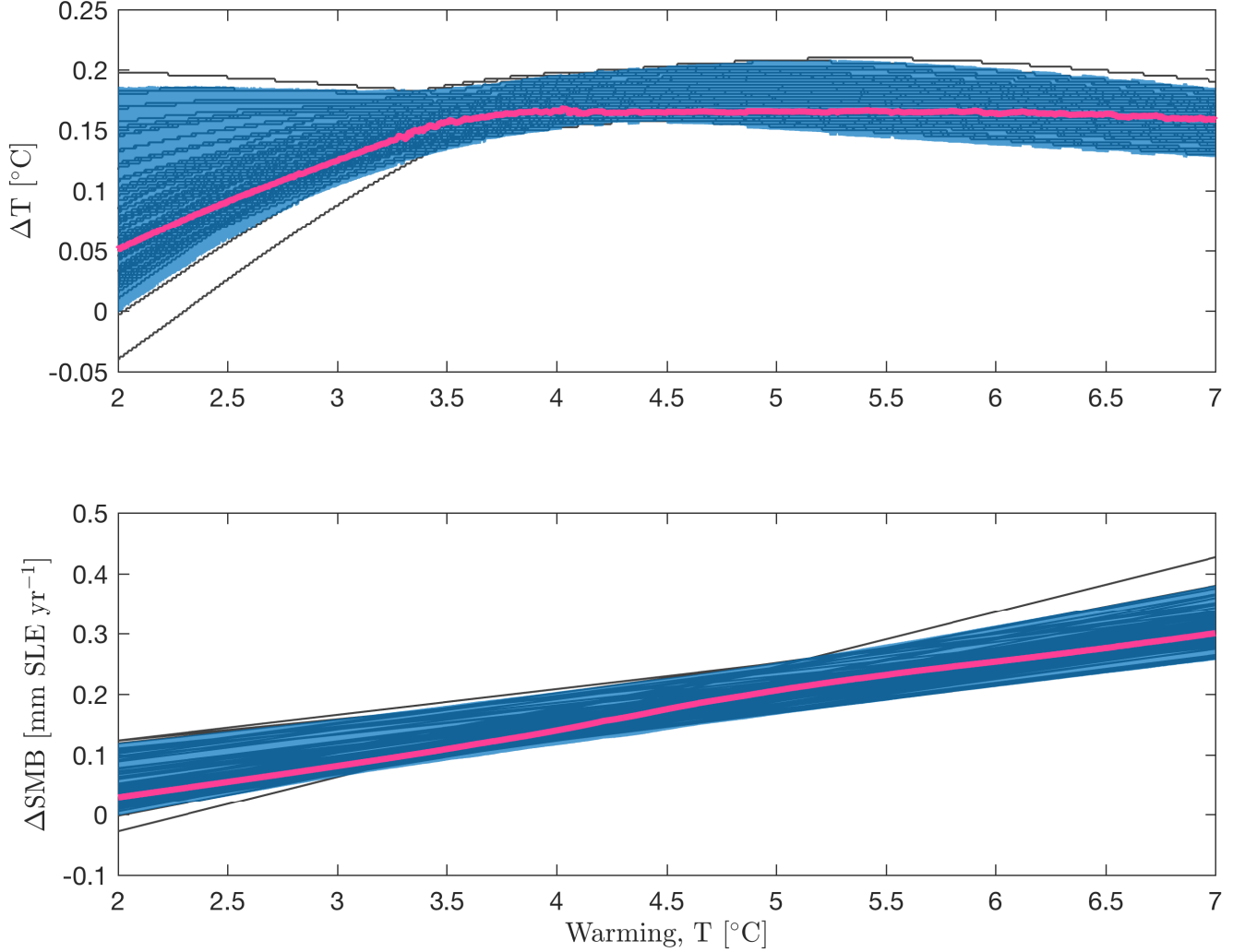

Figure 4. Maximum likelihood estimates of ΔT (effective temperature change) and ΔSMB (effective SMB change where positive values correspond to SMB loss, red curves) resulting from a given temperature increase. ΔT and ΔSMB defined as in Fig. 3, right. The grey curves are estimates from individual simulations and the blue shaded area denotes 95% credibility regions.

5   Observe in Fig. (4) that ΔT goes to zero for low temperature anomalies and appears to ~~saturate~~ reach a constant value for higher temperature anomalies. In the framework presented here this can be explained by considering the SMB(T)-curves shown

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in Fig. (3) (left). For low temperature anomalies the SMB(T) curve in Fig. 3 (left) is close to flat so the second derivative is small; this gives a small contribution to ΔSMB from Eq. (11). On the other hand, as the SMB(T) curve in Fig. 3 (left) becomes progressively steeper, a correspondingly smaller ΔT in Eq. (12) is required to compensate for ΔSMB.

The results above highlight that interannual temperature variability cannot be neglected in long term studies involving ice sheet models. The straightforward approach would be to simply include the expected temperature variability in a number of simulations followed by calculating the ensemble average. Conversely, one could calculate the effect of temperature variability for a range of climate scenarios as a starting point for a following bias adjustment.

5   Conclusions

5.1   ~~Limitations of this study~~

When calculating the $\tilde{f}$'s in Eq. (10) and Eq. (11) we assume a constant volume in the data from Robinson et al. (2012), but in reality the relative variations are as large as 9.5% when considering all the warming temperatures shown in Fig. 4 (Fig. 4, supplementing information). However, to draw the conclusion about the consequences of a $3°C$ warming it is adequate to consider warmings less than $4°C$, and here the volume variation was less than 3% of the average ~~.~~ (Fig. 5, supplementing information). Neglecting variations in volume does add uncertainty to our results, and it is not immediately clear to us how to quantify that uncertainty. Additionally, at time $t = 200$ years where we extracted the SMB data from the simulations in Robinson et al. (2012), the ice sheet ~~models~~ model simulations had not yet reached steady state; thus, expanding the analysis using a data set from ice sheet simulations in steady state would be desirable.

~~The temperature fluctuation is~~ Temperature fluctuations can be accounted for in ~~most studieseither explicitly (Ridley et al., 2010; Seguino~~ ice sheet modeling studies, either explicitly (eg. Ridley et al. (2010); Seguinot (2013)) or implicitly, as happens when tuning the ice sheet model to reproduce an observed ice sheet history with observed forcing as input (eg. Muresan et al. (2016)). ~~or implicitlyin the tuning of the surface mass balance scheme. Our result may be used to explicitly implement the contribution from the~~ Our results show the importance of considering temperature fluctuations in the mass balance schemes before bias correcting ~~due to~~ for other possible model deficiencies.

5.1   ~~Conclusion and outlook~~

From a theoretical argument and by considering a minimal ~~ice sheet model~~ model of the Greenland ice sheet we have shown that fluctuating temperatures forcing the ice sheet have an effect on the mass balance, and thus on the steady state volume of the ice sheet ~~.~~

(Eq. 6 and Fig. 2). The effect is explained by the curvature, or second derivative, of the mass balance as a function of temperature. ~~A negative curvature gives rise to nonlinear effects meaning that the average mass accumulation resulting from a cold year and a warm year in succession is less than the mass accumulation of two consecutive years having the average temperature of the "warm" and "cold" years.~~

~~Even though we considered a simple ice sheet model, the results are transferable to other more realistic models as long as the rather weak assumptions leading up to Eq. (6) hold; eg.~~ We find that the steady state ice sheet volume in Oer03 is $0.5-1$ m_{SLE} smaller when the minimal model is forced with fluctuating temperatures compared to constant temperature (Fig. 2). It is therefore necessary to consider the impact of temperature variability when designing long-term model experiments such as paleo spin-ups (eg. Bindschadler et al. (2013); Golledge et al. (2015); Nowicki et al. (2016)), especially when downsampling the paleo forcing series. Though differences between ice sheet models may be larger than the effect of temperature fluctuations estimated here, we expect the effect to be in the same direction and of similar magnitude for all models. Furthermore, models of sub-shelf melting, grounding line migration, and ice discharge ~~respond very non-linearly~~ have the potential to respond nonlinearly to changes in ocean temperatures (Favier et al., 2014; Joughin et al., 2014; Seroussi et al., 2014; Mengel and Levermann, 2014; Pollard et al., 2015; Fogwill et al., 2014), thus it is critical to take variability into account for quantitative assessments.

The response of a real ice sheet to temperature increase is naturally much more complex than what can be described in a simple study such as the present paper. In a model study, Born and Nisancioglu (2012) observe mass loss acceleration of the Northeastern GrIS as a response to warming. This part of the GrIS experiences comparatively little precipitation and thus increasing melt is not compensated by increasing accumulation. However, the opposite has been shown to be the case for Antarctica. Frieler et al. (2015) show that increasing temperatures will *increase* Antarctic SMB at continental scales due to increasing precipitation. This is ~~an interesting special case of an~~ a case of accumulation dominated mass balance ~~,~~ where the curvature term in Eq. (6) has the opposite sign~~,~~; thus an underestimated temperature fluctuation would lead to an underestimation of the growth of the ice sheet.

We have evaluated the consequences of the temperature fluctuation bias on long-term GrIS simulations and found that, if the full effects are taken into account with no further modifications, a significant *effective* temperature change would be required for an unbiased estimation of the equilibrium ice volume.

6 Code availability

The code for this study is available ~~upon request to the corresponding author~~ at https://bitbucket.org/bogeholm/ice-sheets-fluctuating-temp.

7 Data availability

Data used in this study was obtained from the authors of Robinson et al. (2012).

Author contributions. TBM, AG and PD designed the study. TBM performed the data analysis. TBM, AG and PD wrote the article.

Competing interests. The authors declare no conflict of interest.

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