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Interactive comment on "Influence of temperature fluctuations on equilibrium ice sheet volume" *by* Troels Bøgeholm Mikkelsen et al.

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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 nonlinear 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.

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

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Discussion paper

result should be published.

I have three main comments and criticisms.

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.

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.

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 ∼Tˆ2 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.)

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.

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.

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.

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.

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.

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!

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.

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

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.

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

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.

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.

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).

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.

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’.

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 ∼Tˆ3.

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).

p3 L12 “broader class of models.” broader than what?

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

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

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

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.

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

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 Tˆ2 because of the typically parabolic shape of ice sheets. So the total ablation scales as ∼Tˆ3. 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.

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.

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

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.

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.

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.

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.

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)

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.

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

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