To the reviewer, thank you for your thoughtful comments. We have provided an initial comment-by-comment response below. A completed revision is nearly complete (less some in progress simulations, detailed below) and will be submitted upon solicitation by the editor. Below please find original review comments in blue and our responses in black.

Reviewer 1

The importance of noise and its ability to change the mean state of an ice sheet model is a relatively novel research direction and this manuscript makes important contributions to further our understanding of its role. The methods used in this manuscript are sound, but I think the conclusions in this manuscript would be strengthened with more modeling results. As it is, I do not agree that all conclusions are supported by results (see more detailed comments below). I also think that the paper would benefit from some restructuring and editing, as it appears a bit disjointed at the moment. In particular, I would move sections 3-3.3 before section 2, rather than after, and extend the abstract to be more informative.

Thank you to this reviewer for their helpful suggestions. As the reviewer can see we have made revisions, are in the process of adding more simulations as suggested, and made responses to their suggestions. The abstract has been expanded and we have been more careful not to over-generalize from our results. We have started some of the suggested new simulations (the ice sheet wide ensembles take about 3 weeks to run) which do not change the results in a substantive way. In other places we were able to mitigate the need for new simulations by clarifying the text.

As for the suggestion to move sections 3-3.3 in front of section 2, while this may make more sense from the point of view of establishing theory first and then showing a more realistic large-scale simulation result, part of the goal of this study is to get readers to buy-in that noise-induced drift is an issue for realistic ice sheet projections, which is why we put the large-scale ensembles first before explaining the theory behind the results. Also, the way the discussion has been structured, we refer back to the large-scale simulations as examples of these different mechanisms of noise induced drift in section 3, and this would not be possible if the theory/idealized model sections come first. We strongly feel that the order of present a result that needs explaining-and then explaining it after is an equally valid way to structure the paper, but if the reviewer has an idea of how to restructure in a way that preserves the current mode of discussing (without needing to cut significant portion of the discussion), then we would be open to it.

However, we think you will find that the edits that have been made greatly improve the clarity in the ways suggested and the revised manuscript is improved as a result.

Abstract: To me, the abstract seemed too short and not very informative. It would be more informative if it included more details about the methods used. Also, it would be good to point out that stochastic variability in frontal ablation changes the mean state more than variability in other parameters.

We have added further details in the abstract, specifically describing the methods used, the results for stochastic variability in SMB, and the different causes of noise induced drift.

Introduction:

The introduction is quite a bit more general than the abstract which refers to frontal ablation. It's almost as if the purpose of the paper changed from abstract to introduction. Please streamline.
This perception of the reviewer is probably the result of this paper originally being written as a brief communication, and then expanded. However, our expansion of the abstract, and then our inclusion of more specific wording in the introduction make the two parts feel more in tune with each other.

Line 21: "we show that noise-induced drift is expected in [...] any numerical modeling of ice sheets." I am pretty sure it is possible to construct counter-examples which do not show noise-induced drift, so you might want to be careful with such sweeping statements.
Changed to be less absolute: “noise-induced drift is expected to occur in real ice sheets and numerical modeling of ice sheets”

Section 2:
The variability in water pressure is quite a bit smaller than the variability in calving and only specified in an unspecified "region near the glacier front", which makes a direct comparison with calving and SMB variability difficult. I recommend either including more information or leaving it out completely.
We have removed this ensemble completely, as it was challenging to make a direct comparison with the other ensembles, as this reviewer points out.

Line 75: "the rate of drift is also approximately proportional to the amplitude of the variability in calving rate": Fig. 1 does not show this. The figure shows that the drift observed with stochastic calving appears to increase with the noise variability (interpolating from 2 data points) and that the mass change induced by stochastic noise might be reproduced by a higher deterministic calving rate. More data examples would be required to make more definite statements. If feasible, I highly recommend including more data in order to analyse the behaviour in more detail.
This is a good point, and one that perhaps we were a bit too expansive in claiming. We have softened the language here and then we have added a discussion of the question of drift proportionality to stochastic forcing amplitude in section 3. Adding further ensembles would be computationally expensive, and not necessary to accomplish the principle goals of this paper.

It would also be interesting to see what the model behaviour for stochastic noise in calving, SMB and pw combined is. Are drifts due to different processes simply additive, or might there be some nonlinear bahaviour?
This is a good suggestion. We had done some initial short simulations which showed that the processes are additive (though drift is dominated by calving forcing). We have decided to re-run these completely, and the combined ensemble will be included in the revised manuscript.

Figure 1 legend: what does St. stand for? I think writing "stochastic" out would be clearer.
Changed

Section 3:
Equation: personally, I would prefer f and g to be bold too, to be consistent with x as a vector
Changed

Line 158-160: The effective pressure used in this study should be $N=\rho_i g H + \rho_w g b$. It would be useful to write this out here, so that readers not familiar with this particular choice of sliding law are immediately aware of the dependence of N on H. While I see how N varies linearly with changes in ice thickness, it does not necessarily follow that variations of the terminus position lead to linear variations in ice thickness or that there is a linear dependence of u on H; please rewrite this to be clearer.'

We added the definition of effective pressure back in section 2 where the model is explained, and then referred back to this equation in section 3. We also removed "linear" to avoid confusion about the point here, which is simply that velocity at the front is indirectly effected by terminus fluctuations through the effective pressure and sliding law.

More generally in section 3.2 it is not clear why the non-linear dependence of u on H leading to drift is fundamentally different from the case considered in 3.3. To me, the only difference is that the effect on effective pressure is located away from the terminus, which makes it less amenable to a Reynolds decomposition - and which would also explain the potentially smaller effect of these perturbations.

The dependence of basal friction on effective pressure is linear in the Budd sliding law considered here. It is true that there are other nonlinearities in the momentum balance that could lead to drift, but the distinction that is made in the statistical physics literature is whether the asymmetry arises directly in the variable that is being stochastically perturbed (termed "nonlinear noise") or due to state-dependence (whether linear or not) on other system variables ("multiplicative noise"). While this may seem a bit of a semantic distinction, particular in systems as complex and nonlinear as ice sheets, our goal here is to introduce the glaciology community to the way that this problem is thought about in the physics and math literature, and so we see it as important to preserve this distinction. We have added further clarification to make the distinction clearer:

"In this particular sliding law, the dependence of basal friction on effective pressure (and therefore ice thickness) is linear, though there are other nonlinearities elsewhere which may play a role in generating drift. Since the variable that is being perturbed stochastically is linear, and the nonlinearities arise elsewhere in the ice sheet sheet dynamical equations, this is considered to be 'multiplicative noise'."

Line 211: though -> through

Fixed

The analysis in section 3.3 makes specific predictions about the size of noise-induced drift depending on σ, and . It would be helpful to verify and illustrate these results with the idealized marine-terminating glacier model.

We are currently running new idealized simulation ensembles with changes in the amplitude of variability to show this point. We anticipate including these simulations in the revised manuscript.