This paper presents a series of statistical methods that can be utilised to constrain and supplement simulations of ice flow in order to reduce uncertainty associated with the model initialisation procedure. The authors use statistical emulation to forecast additional simulations within a predetermined parameter space, combined with the use of spatial observations with which to calibrate the emulated ensemble.

General comments:
As an ice sheet modeller unfamiliar with some of the statistical methods, I found it challenging to follow the narrative of the various steps involved, what data was being used and why each step was being performed. Each method appears to be presented separately rather than sequentially with continuation from the previous procedure. The sequential process should be more clearly outlined at the beginning of the methods section. A flow diagram would be useful to highlight each procedure and the data used. As a paper presenting a new methodology that could be beneficial to the ice sheet modelling community (by reducing the computational expense required for large ensembles), it is of great importance that the methods are conveyed clearly and can be reproduced by the reader. In its current form this is not the case.

It is unclear what the purpose of Section 2.2 is. Moreover, the study discusses using 7 years of dh/dt satellite observations, but the dataset is presented as 1992-2015. Which years are used? Why 7?

On page 12 you propose that the imposed basal melting has a "delayed impact" on the dynamics of the system which is contradictory to a number of studies that highlight ocean forcing to be the primary driver of immediate dynamical response in the ASE. On what timescale do you consider the response to be delayed? Numerous studies have shown that the ASE is highly sensitive to perturbations in ocean driven melting (e.g. Pritchard et al., 2012; Jenkins et al., 2018) whereas you are suggesting that the region is somewhat insensitive and requires considerable melting/thickness change to impact dynamics? This is an important point and it would be worth commenting on this in more detail if such a claim is to be made, or perhaps this should be described more carefully if this is not the case.

If the weighted average of C and phi are 0.47 and 0.45 this infers that a more slippery bed and softer ice result in better estimates of dh/dt than the optimum (0.5) from the velocity inversion. This should be commented upon. Could this mean that the 0.5 values underestimate sea level contribution?

It is unclear what the purpose of the methods performed in section 3.2 are. Why have the observations been reprojected?

As was highlighted in the previous round of reviews, I am concerned with the presentation of results as projections of future sea level contribution given the absence of additional forcing throughout the simulation. This should be clarified throughout, and the use of the term "projections" be reconsidered as this is more generally applied to future simulations involving some form of climate forcing. Further, the emphasis of the study should be on the novel methods presented, this, in addition to what the methods employed are, should be more clearly presented in the abstract.

In the previous round of reviews it was suggested that the emphasis of the paper should be on the use of new statistical methods for model calibration and emulation, instead of the 50 year projections of SLE that arise from the investigation. Whilst you begin to do this by emphasising in the discussion that future ocean forcing of the ASE will accelerate the dynamic response of the region, you then contradict this by stating "climate scenarios would have a small net impact on our 50-year projections". Studies have indicated that the range of possible forcings within the RCP scenarios could have a substantial impact on the response of the region over a 50 year period as simulations have shown that the region responds linearly to ocean melting (see Alevropoulos-Borrill et al. 2019). Furthermore, Alevropoulos-Borrill et al. (2019) find that region becomes more sensitive to the perturbed model parameters (investigated by Nias et al. 2016) as the ocean forcing increases and therefore climate scenarios would impact the projections in this investigation. Future studies would benefit from the application of the method presented in this investigation to climate forced projections and this should be highlighted in the discussion.

Continuing from the previous point, mentioning the large uncertainties associated with future ocean forcing and the implementation of basal melting in ice sheet models is a viable point however the relevance of this to why you apply no additional forcing to your simulations is unclear. If the uncertainty associated with ocean forcing is so wide, is this really captured in a halving and doubling of the optimal ocean melting obtained during the initialisation procedure?

I believe the figures were modified following the previous round of reviews but these updated figures were not included in the revised manuscript. This should be amended and avoided in the future.

Specific comments:
Page 1: Given that the investigation is heavily methods focused, the abstract does not fully convey the methods employed which should be more clearly stated for the reader (this is a more important focus than the 50 year SLE "projections").
Page 1 line 1: Calibration of what with observations?
Page 1 line 2: "…particularly if this exploits as much of the available information as possible (such as spatial characteristics)" seems vague?
Page 2: The introduction, particularly the first paragraph is very long- could this be shortened and maintain a more study-relevant focus?
Page 2 line 12: Consider removing the clause "centered at the Ellsworth Mountains".
Page 2 line 23: Unclear why the sentences in brackets are relevant.
Page 2 line 29: This sentence could be more concise.
Page 2 line 31: Remove ie and replace with "in order" or equivalent.
Page 2 line 31: Why does reducing uncertainties matter? This should be included in the introduction.
Page 3 line 10: Rethink paragraphing of this as the beginning sentence better fits with the previous paragraph.
Page 3 line 15: "in the following section"
Page 4 line 25: "…Hypercube design by Nias et al., (2016)."
Page 5 line 3: Move "For a full description of the model…" to a different paragraph
Page 5 line 10: "We use a compilation of five satellite altimeter datasets of surface elevation changes…" to do what?
Page 5 line 21: "to represent"
Page 5 line 31: Principal Component Decomposition in section 3.1 is performed on what, the whole Nias ensemble?
Page 6 figure 1: The figure caption is vague. The modes of variation of what variable? Within the Nias ensemble? Relative to the dh/dt observations? No scale bar label.
Page 6 line 8: U)? Typo?
Page 6 line 9: Sometimes you have Figure 1 sometimes Fig. 1 in the text. Make these consistent.
Page 7 figure 2: Mean observed ice thickness change using which dataset? Over what time period? In what way have the observations been reprojected, there is little discussion of this in the text and it is unclear what the purpose of this figure is to the reader. The caption should be less vague.
Page 7 line 6. Does this sentence mean the observations are assumed to be temporally constant over the 7 year period?
Page 7 line 9: five in letters
Page 8 line 25: lowercase S
Page 9 line 28: "can" in the wrong place
Page 9 line 29: rephrase sentence and remove e.g.
Page 10 line 25: If 1.4% of the parameter space cannot be ruled out, does this mean you discard 98.6% of the emulated parameter sets?
Page 10 line 6-7: rethink paragraphing
Page 10 line 29: Both using Eq. and Equation in the text. Choose one and make it consistent.
Page 11 line 9: "These 14 realizations are used in exactly the same way as described before…" this could be more clear.
Page 11 line 13: "many other" could you not specify how many additional tests you explored?
Page 12 line 2: What does "good model configuration" mean?
Page 12 line 11: If the linear sliding law is favoured due to the density of central ensemble members, shouldn't this be presented as a caveat of the method? Are there any methods that would help to identify such a biasing of results?
Page 12 line 12: Clarify what you mean by 'fast' and 'slow' simulations
Page 12 line 16: Your editor response gives 32% to 68% whilst the revised manuscript gives 28% to 72%.
Page 12 line 17: Rhetorical question unnecessary, reword.

Page 12 line 30: In Nias (2017; Ph.D. thesis) it is suggested that the halving and doubling of the initial melt rates did not capture a wide enough range. It might be worth mentioning this.
Page 13 line 23: Reconsider paragraphing of this.
Page 13 line 25: Should this be viscosity parameter not velocity?
Page 14 line 1: Grey and Brown need not be in capitals.
Page 14 line 3: wile? Typo?
Page 15 line 1: 6mm SLC
Page 15 line 1: Inconsistency with SLE and SLC, choose one and stick with it throughout.
Page 15 figure 5b: The grey shading makes it difficult to read the histogram.
Page 17 line 9: Two commas.
Page 17 line 25: See also Alevropoulos-Borrill et al. (2019).
Page 17 line 23: “Figure 2”
Page 18 line 6: What sort of variations are performed in the cited papers?
Page 18 line 8: “Probabilistic calibrations are an assessment of model setups to be the best of all tested cases” this sentence is unclear.
Page 18 line 11: Consider moving paragraph to conclusions or beginning of discussion.
Page 18 line 31: As mentioned in the general comments, claiming that ocean melting has a slow impact on ice sheet behaviour is ambiguous and the author should be more careful with wording such a statement.
Page 18 line 26: Given that the simulations include no future climate forcing, is it realistic to present the findings as the next 50 years. The absence of climate forcing should be more clearly highlighted here.
Page 19 line 6: Final paragraph in the conclusion conveys that the estimates are “projections” and does not include the fact that there is no additional forcing applied in these simulations. This is misleading.

References:
Alevropoulos-Borrill, A. V., Nias, I. J., Payne, A. J., Golledge, N. R., and Bingham, R. J.: Ocean forced evolution of the Amundsen Sea catchment, West Antarctica, by 2100, The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-202, in review, 2019.

Pritchard, H., Ligtenberg, S.R., Fricker, H.A., Vaughan, D.G., van den Broeke, M.R. and Padman, L., 2012. Antarctic ice-sheet loss driven by basal melting of ice shelves. Nature, 484(7395), pp.502-505.

Jenkins, A., Shoosmith, D., Dutrieux, P., Jacobs, S., Kim, T.W., Lee, S.H., Ha, H.K. and Stammerjohn, S., 2018. West Antarctic Ice Sheet retreat in the Amundsen Sea driven by decadal oceanic variability. Nature Geoscience, 11(10), pp.733-738.