Review of "Quantifying and attributing the role of anthropogenic climate change in industrial-era retreat of Pine Island Glacier" by Alexander Bradley et al.

This study investigates the long-term (multi-century) historical behavior of Pine Island Glacier (PIG) using a model framework that assimilates sparse but important constraints over this period. The approach enables the authors to calibrate several free parameters, among them aspects of climate forcing, so as to reproduce the known century-scale retreat of PIG. As a result, they can quantify the likely role of anthropogenic trends and a natural early-20th century climate anomaly in driving this retreat. They find from their optimized ("posterior") ensemble of simulations that an anthropogenic trend is necessary to produce the full magnitude of observed retreat, but importantly, major retreat also occurs without an anthropogenic trend.

This study contributes substantially to the line of research attempting to disentangle the drivers of ice loss in the Amundsen Sea region. Notably, while previous studies have advanced understanding of the atmospheric and ocean drivers, the present study is the first to model the ice dynamics of PIG in an attribution framework and thus stands to make an important advance. The simulation + emulation framework seems useful for approaching the considerable uncertainties surrounding this problem, and promising broadly for future work on historical simulations. Overall the study is well-written with clear figures and descriptions of the calibration/emulation methods. And the authors provide transparent discussion of several caveats, which is important and appreciated.

That said, I do have some questions about the experimental setup and results that I think need resolving, and possibly reframing, before publication. I detail these major comments below, as well as some minor and technical comments.

Major comments

1) Most generally, I think the attribution assessment should be framed more explicitly as being conditional on the assumption that the glacier is losing mass from 1750 on, regardless of 20th-century forcing. Whether or not this was an explicit assumption at the outset, it is built-in by the combination of the initial condition (which the authors suggest may be too large) and the calibration procedure that is constrained to match the 2015 volume. Consider an alternative possibility that PIG was closer to steady state (i.e., not losing mass) prior to the 1940s event – such a case is plausible given uncertainties in preindustrial conditions, but is not really possible to consider in this framework, by virtue of the initial condition used. And I think that as long as fully communicated, that is fine – it is still valuable to explore the family of possibilities stemming from this initial condition, and the authors include thoughtful discussion around the implications of the initial condition (including considering a range of IC's in future work). But, I think this built-in feature of mass loss should be worked more directly into the summary statements around the anthropogenic component of retreat, as it might exclude some plausible scenarios that could yield different attribution numbers. Again, the discussion around caveats is already helpful, but it could be clearer that the particular numbers reported are conditional on this context of significant mass loss for all simulations (Fig 7b really brings this home for me).

2) Related to the initial condition, it seems several aspects of the model setup would cause initialization artifacts that propagate into the simulations, which seems potentially problematic for assessing different drivers of retreat, and possibly for the calibration procedure.
   - As I understand, forward simulations with perturbed parameters are branched from a common 1750 initial condition. I don't see anything about repeating the spinup procedure with the perturbed parameters (If that is what's done, please clarify!). So in the case of temporally-static parameters (viscosity, sliding, and melt-rate prefactors), the model is responding transiently to a parameter perturbation at 1750 which, given long ice-sheet response times, I'd expect blurs into the effects of climate forcings of interest in the 20th century.
   - Also, the preindustrial state is achieved by shutting off melt for 500 followed by a 50-year spinup with nonzero melt (but cold conditions). 50 years is very little time for the ice dynamics to adjust. Would the GL be stable here indefinitely with the low but nonzero melt rates? (i.e., even before calibrating parameters to match observations). Also, if I'm not mistaken, there is a jump in the pycnocline depth imposed at the start of the historical simulations, from -600 m in the spinup (line 215) to -500 m as the central value in forward simulations (line 208). Does this add another initialization shock?

   Overall, these add ambiguity to the trajectories going into the 20th century. How long do these adjustments persist? The authors do suggest that longer spinups would be desirable, but also that 1750 is a sufficient start time (L 559). It would be helpful to discuss the rationale in more detail, and more ideally to provide some sort of control simulation to help characterize such transient effects.

3) I also wonder about the plausibility of the climate forcing inferred by these simulations. I see the value in the approach to treat B_0 and T_0 as free parameters in the calibration framework (and then removing them in counterfactual scenarios). However, the posterior values for both B_0 and T_0 end up as very strong forcings. Again, to the authors' credit, this is clearly pointed out, but I think some more discussion is warranted.

   In particular, the posterior B_0 of > 200 m is quite large compared to the imposed stochastic anomalies. I'll note the example in Fig. 2 (B_0 = 50 m) is not very representative: B_0 = 200 m would be literally off the chart. Is this really realistic? The posterior T_0 of ~ 200 m/century is also quite large compared to the example shown in Fig 2. The authors provide helpful references for comparison, but still, both B_0 and T_0 seem larger in a signal-to-noise sense than the corresponding event / trends in reconstructions that are cited (e.g., O'Connor et al 2023; Naughten et al. 2024). Granted these references are providing different variables (winds, sea level pressure, subsurface temp), but it's hard to imagine the signal-to-noise would be greatly amplified for pycnocline depths. Alternatively, perhaps the stochastic variability applied here is too small.

   Either way, I think it would be important for the authors to clarify further how plausible they expect these posterior values are. Given that the model calibration process seems to push all parameters towards values that enable retreat, it seems it may be selecting for forcings that are stronger than realistic. (On the other hand, it is interesting that despite these strong inferred forcings, there is still large-scale retreat in the counterfactual simulations without them.) It is concluded that anthropogenic forcing contributes ~20% of the observed retreat, but if either B_0 and/or T_0 are on the high end of what's plausible, that would seem to be an important caveat. I also think it is important to show the reader an example of an actual posterior pycnocline timeseries (as opposed to the example in Fig. 2c) so it is clear what magnitudes of forcing are implied here.

Minor and technical comments

- Abstract (and elsewhere) – I'm not sure how directly the Holocene retreats should be invoked here, since they aren't directly addressed in the present study. I think it is a great discussion point, but even though the simulations here start off with mass loss in 1750 so are consistent with this idea, the residual of multi-millennium retreat is not being directly simulated here, so I would consider qualifying how this possibility is raised.
- 135 – "multiplies the…" (something is missing)
- 150 – How does this set value for C compare to the areas where it is inferred?
- Fig 1c – is the preindustrial profile shown that without melt, or after the melt is re-introduced? (And especially if the latter, I'm not sure it should be referred to as a steady state.
- 210 – It would be helpful to specify more about the stochastic variability imposed. Specifically, is it Gaussian-distributed and just truncated at -2, 2? If so, what does alpha correspond to in terms the distribution? (I'm guessing 4*sigma, but it should be specified). Also note the timescale of the autoregressive process.
- 222 – given that the 1940s event is associated with internal climate variability, it seems somewhat inconsistent to superimpose a representation of it on top of stationary stochastic anomalies. So, those realizations with positive R(t) anomalies around 1940 will add further to B_0, creating a double(ish) anomaly? I understand the rationale for directly imposing the 1940s event, but perhaps the reader should be alerted to this.
- 270 – is there a source for this error estimate, or just an order-of-magnitude estimate?
- Table 1 – specify units for T_0 – meters per century?
- Fig 3 – specify – results are shown for a single realization of stochastic forcing? (To disambiguate from different iterations of the imposed event/trend forcings).
- ~285-295 – check case for thetas – it seems inconsistent. Or is capital vs. small theta supposed to mean something?
- 343 – interesting that using all iterations improves emulator performance – is this because it's still a valid mapping between parameters and model state? (and matching observations isn't important for training emulators?)
- 345 – I'm confused by the notion of coverage here – isn't the percentage of emulator predictions falling outside 2 stdv just defined by stdv? Or is this comparing two

distributions? Is the emulator standard deviation defined by the analytic uncertainty estimates mentioned earlier?

- 370 and on – I appreciate the plain-language descriptions alongside the more formal descriptions – I think this will be helpful for readers.
- 405 – perhaps “particular” rather than “precise”? The latter seems inconsistent when followed with “broad errors on observational constraints”
- 438 – this follows my major comment above, but I’m skeptical that the CES procedure here should be taken as inferring a lot about the climate forcing. Around line 423, it is noted that via the melt prefactor, the procedure is causing higher melt rates than observed. It seems like the constraint to make the model lose a lot of mass by 2015 might also be biasing the B_0 to be high.
- 440 – I would clarify “the *full magnitude of* the 20th century retreat…” as is done elsewhere. Here, it could be construed as no retreat occurring without anthropogenic forcing, which is not what is found.
- 515 and on – it is quite an interesting finding that there are points where the fraction of attributable retreat decreases when the grounding lines across scenarios are pinned at the same bedrock highs. It makes me wonder whether a fraction of attributable volume loss would show the same? It seems the anthropogenic warming could drive more mass loss during the periods of retreat, though I’m not sure of my intuition here.
- 523 – when providing these projections, perhaps remind the reader this is subject to the extending the simplified forcing scenario. It seems this could vary across future projections.
- 553 – I’m unclear on what is meant by pre-1940s forcing being too week. Baseline melt rates, or the stochastic variability?
- 593 – That ice-shelf area change is not included in the model seems significant, and should probably be noted much earlier, in the model description. What are the implications of this? Presumably there is still an effect on buttressing through ice-shelf thinning, right?
- 601 – again, I suggest specifying “attribute the component of retreat due to…” since the full magnitude is not found to be attributable to anthropogenic forcing.

References

Naughten, K.A., Holland, P.R. & De Rydt, J. Unavoidable future increase in West Antarctic ice-shelf melting over the twenty-first century. *Nat. Clim. Chang.* **13**, 1222–1228 (2023). https://doi.org/10.1038/s41558-023-01818-x

O'Connor, Gemma K., et al. "Characteristics and rarity of the strong 1940s westerly wind event over the Amundsen Sea, West Antarctica." *The Cryosphere* 17.10 (2023): 4399-4420.