Author response to review by S.J. Marshall on "Stopping the Flood: Could We Use Targeted Geoengineering to Mitigate Sea Level Rise?"

Michael Wolovick and John Moore

We thank Dr. Marshall for the positive review of our article. We now respond to specific comments below.

p.4, ll.7-9, discussion of rates of sea level rise. I don't think the rates that are cited are representative of the consensus of "modern models". Rates of several m per century are only really possible from Antarctica, in association with a marine-calving collapse, i.e. the ice-cliff instability of Pollard and de Conto. From the Thwaites system, ice resistive stresses and deformational velocities generally limit the rate if deglaciation, according to most model studies to date, and this will be true for most Antarctic embayments. In the example of the last deglaciation, the sea level rise of up to 5 m/century was in a much different world, with huge mid-latitude ice sheets capable of (surface) melt rates that are not possible in the polar regions. I think these examples are still fine to mention, but don't need to be considered as the "likely" scenario for the future centuries. Especially as rates of sea level rise of an order of magnitude less than this would still be massively disruptive and would justify potential interventions.

All three of the models we cite in this part (DeConto and Pollard, 2016; Winkelmann et al., 2015; and Golledge et al., 2015) predict rates of sea level rise from Antarctica of at least a meter per century under high emission scenarios. DeConto and Pollard (2016) do indeed have both the highest rate of sea level rise and the earliest peak (up to 6 m/century in the mid-2100's, shown in their Figure 4c), but the others are fairly large as well, and neither of the other two models included the marine ice cliff instability. Golledge et al. (2015) have the slowest rate of sea level rise of these three models; they show sea level rise rates in their Figure 2a that hit a maximum of about 15 mm/yr (1.5 m/century) around the year 2300. Winkelmann et al. (2015) do not show a figure depicting the rate of sea level rise; however, they do show cumulative sea level rise in their Figure 1d, and we were able to manually measure the derivative of those curves to determine that the highest emission scenario they consider had a sea level rise rate of 5.7 m/century between the years 2200 and 2300. We show our work for this calculation in the attached Figure 1 below.

In addition, the expert judgment assessment of Bamber and Aspinall (2013) shows that glaciologists believed (even before the 2014 papers hypothesizing the onset of the MISI in the Amundsen sector were published) that the 95th percentile for sea level rise in the year 2100 was 17.6 mm/yr, or 1.8 m/century. That expert elicitation produced a highly skewed probability distribution of sea level rise, and the authors explicitly connected the "fat tail" at the high end to experts allowing for the possibility that the MISI might be initiated in West Antarctica before the year 2100. Yet a runaway collapse, even if initiated before 2100, would probably not hit its maximum rate until the centuries after that. Considering those lags in the system, it is probably safe to say that the consensus opinion of the glaciological community is that sea level rise rates of greater than a meter per century are a reasonable expectation for a runaway ice sheet collapse.

It is true that the collapse of the mid-latitude ice sheets at the last deglaciation is a very different setting than a collapse of Antarctica would be in the future. However, we felt that it was important to cite data in this section in addition to models. The example of sea level rise rates during Meltwater Pulse 1a gives an indication of the order of magnitude of sea level rise rates that ice sheets are capable of during a rapid collapse, and an argument based on both data and models is inherently stronger than an argument based on models alone. In addition, there is evidence that MWP1a may have been sourced from Antarctica rather than the mid-latitude ice sheets in the Northern Hemisphere (Clark et al., 2002). While the majority of the sea level rise that occurred during the last deglaciation was due to the melting of the Northern Hemisphere ice sheets, it is at least possible that the rapid rise during MWP1a was due to dynamic retreat in Antarctica, in which case this geologic evidence would be highly relevant. We have added wording in this part clarifying that the geologic evidence pertains to MWP1a specifically rather than the last deglaciation as a whole.

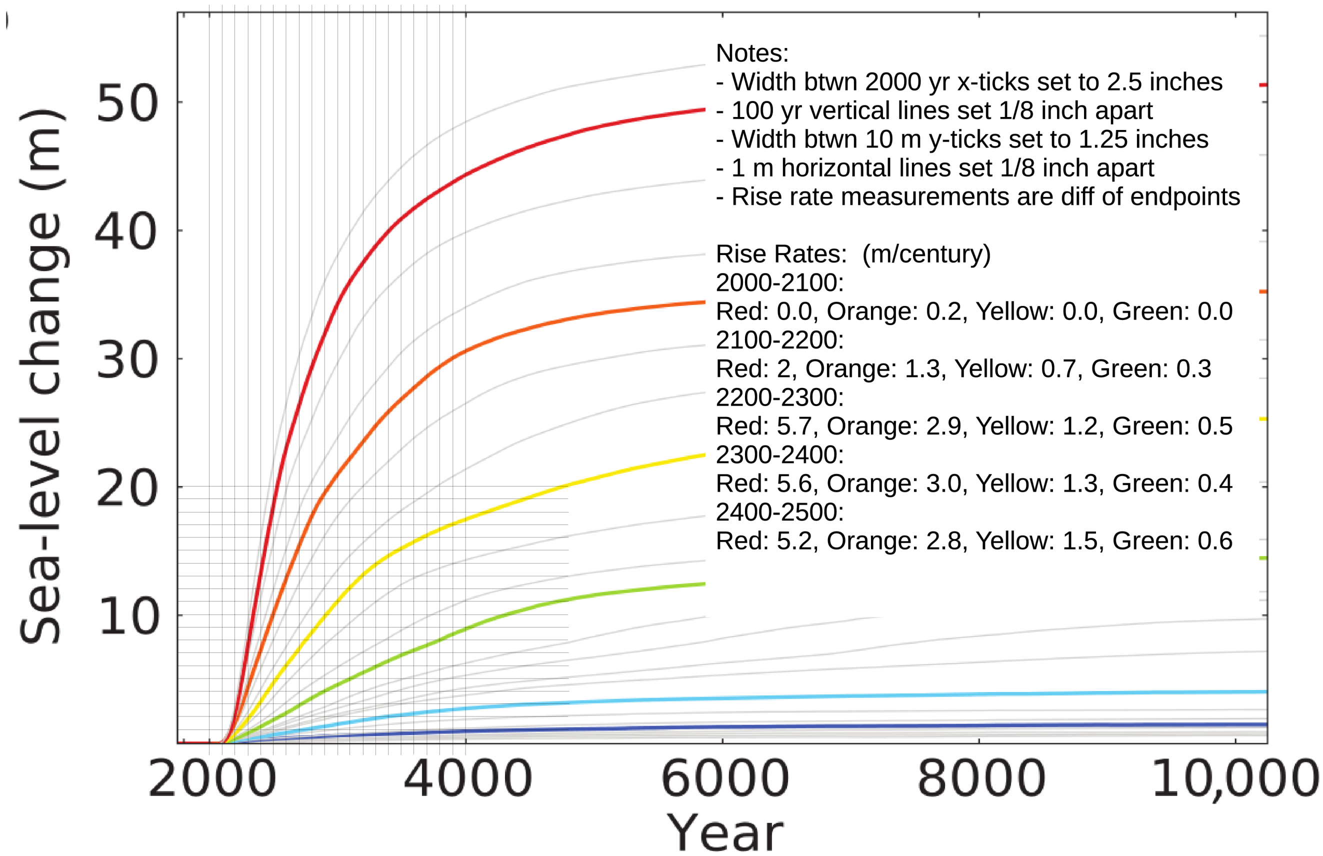

Figure 1. Our manual measurement of sea level rise rate from Winkelmann et al. (2015). The underlying figure is taken from Figure 1d of Winkelmann et al. (2015), showing cumulative sea level rise from Antarctica for a variety of emissions scenarios. We imported this image into LibreOffice Draw and manually overlaid regularly spaced horizontal and vertical lines in order to measure the slopes of the curves. The vertical lines are set at 100 yr increments and the horizontal lines are set at 1 m increments.

p.4,5, Methods. It is a little worrying that the model used for this study does not appear to consider longitudinal stresses. These are important to floating ice dynamics, grounding line migration, and the timescale of marine ice sheet instabilities. This should be discussed.

Our model does include longitudinal stresses; these are represented by the first term in Equation 2 of the supplementary material. We have changed the wording of the beginning of the methods section in the main text to clarify this.

p.5, Experiments. Really interesting. I worry a bit that the interventions don't address the mechanical conditions that drive MISI - subglacial topography, stress balance, and pinning points upstream of the grounding line. I appreciate that warm water (basal melting) strongly influences the ice thickness and then feeds back on these things, such that an ice readvance, if it can be triggered, can then bring the ice sheet back out to the manufactured sill, with possibilities to ground and stabilize.

This is a good point. One of the other interventions that we suggested in Moore et al. (2018) was a subglacial drying scheme designed to modify the stress balance upstream of the grounding line. Society will have to consider a wide variety of possible interventions before anything could actually be implemented, but for this particular paper we wanted to focus on evaluating the efficacy of one particular intervention. We leave it to future work to evaluate the relative merits of an artificial sill as compared to other potential interventions.

But I think there are some who would suggest that the MISI is a mechanical instability that is associated with the upstream geometry and, once triggered, it can continue without regard to ocean temperatures (i.e., with no need of enhanced melting). Again, a brief discussion of this could be helpful.

The MISI is indeed a mechanical instability, and it can be suppressed by the buttressing provided by a floating ice shelf (Gudmundsson et al., 2012). That is why we only considered an intervention to have been a success if the ice shelf regrounded on the artificial sill. Merely reducing the melt rate of an unbuttressed ice shelf makes no difference to the MISI, although for other glaciers whose shelves are in confined embayments, such as Pine Island Glacier, thickening the shelf will increase buttressing and slow grounding line retreat. However, for an unconfined shelf like Thwaites, reducing the basal melt rate of the shelf will only have an effect on the MISI if the shelf thickens enough that it regrounds on the sill.

Are there oceanographic or 'storm' considerations here for effective blocking of threatening CDW by a sill? That is, are conditions so strongly stratified that a manufactured sill that does not completely block the embayment can effectively isolate the ice sheet? I don't know if tidal mixing or storm- or katabatic-driven Ekman fluxes, etc., can effectively mix the water column (especially in a future with less sea ice/a longer summer open water season), limiting the efficacy of the manufactured sills. But perhaps they just need to initially trigger ice thickening and advance, and then the mechanical grounding does the job.

We have not explicitly considered ocean currents or mixing other than in the ice-contact meltwater plume. We use the scenario where the sill blocked 50% of the warm water to represent partial mixing of warm water over the sill top. As we discuss in our response to Dr. Asay-Davis' review, that scenario was not meant to represent 50% horizontal blockage, but rather full horizontal blockage with some of the water nonetheless being mixed over the sill top by tides/winds/storms etc. We have clarified our intentions with respect to this scenario in the methods section.

abstract, l.9, "is both effective and achievable"

Changed.

p.3, l.10, 1990s

Changed.

p.3, l.18. displacement of 100-500 million people per year - I think this must be total, not per year. As this would not be a very sustainable rate of migration

That number refers to temporary displacements due to episodic flooding and storms. We have modified this sentence to include both temporary and permanent population displacements. We had not initially included a number for permanent displacements since most of the literature we consulted considered the no-protection scenario to be unrealistically apocalyptic and they therefore did not quote a number for coastal refugee flows in the absence of coastal protection. We approximated the number for 21st century sea level rise by taking the number of people within 1m of sea level (131 million, Nicholls et al., 2008) and dividing by 100 years. Nicholls et al. (2008) also give a more rigorous result for permanent refugee flows in the presence of coastal protection, and found permanent population displacements of tens to hundreds of thousands of people per year depending on the scenario. We have included both of these numbers in that paragraph.

p.15, l.15. I am not sure that field tests could be decades away at the earliest - the authors argue that pilot tests in some Greenlandic fjords could be reasonable to contemplate. But point taken - we have time to develop more complete models and thoroughly consider oceanographic/marine biological considerations

We have changed these sentences from, “This is not a project that would begin soon.
Field tests would be decades away at the earliest, and humanity might not be ready to deal with Thwaites for a century or so.” to, “This is not a project that would begin soon. A large amount of modelling, data collection, planning, technological/logistical development, and field testing, not to mention public discussion and political debate, must be done first. Humanity might not be ready to deal with Thwaites for a century or so. “

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Author response to review by X. Asay-Davis on "Stopping the Flood: Could We Use Targeted Geoengineering to Mitigate Sea Level Rise?"

Michael Wolovick and John Moore

We thank Dr. Asay-Davis for the thorough review of our article. We now respond to specific comments below.

My two most significant concerns about the work are the following. First, I am concerned that 1HD modeling is not appropriate for Thwaites Glacier because the complex topography and significant cross-flow variability are likely to provide buttressing that is fundamentally 2HD and cannot be captured through a 1HD parameterization (see detailed discussion below). I would have liked to see at least some validation of the 1HD approximation through comparison with 2HD modeling.

We feel that 1HD modeling is actually more appropriate for Thwaites than for other glaciers, since Thwaites is so wide that side drag plays a very small role in its dynamics. In addition, while there is some cross-flow variability in Thwaites' basal topography, there is no well-defined central trough or confined ice shelf as in many other glaciers; and it was specifically the presence of a central trough and a confined ice shelf that Gudmundsson et al. (2012) used to generate the lateral buttressing that can stabilize a glacier against the MISI. We expand more on this point below.

Second, the parameterization of ambient water masses in the ice-shelf cavity assumes that the properties of the deepest water masses in a partially obstructed cavity would be a linear combination (proportional to the fraction of obstruction) of those at the deepest point in the open ocean and those at the top of the sill that provides the partial obstruction. It is my assessment that ocean modeling and observations suggest that partial obstruction is not very efficient at blocking water masses form being transported horizontally. This would suggest that the warmer, deeper water mass would likely fill the deeper parts of the cavity even when most (but not all) of the width of the cavity is blocked by a sill. For many ice shelves around Antarctica, troughs either near the continental shelf break or beneath the ice shelf itself provide efficient pathways for warm water to enter ice-shelf cavities even when these troughs represent only a small fraction of the width of the shelf. To me, this suggests that a re-interpretation of the results with 50% sill blockage may be required. Again, see details below

We had not intended the 50% blockage experiment to represent 50% horizontal blockage, but rather 50% mixing of the water over the top of the sill. It is absolutely true that ocean currents are efficiently transported horizontally. We intended that experiment to represent a situation in which a sill was constructed across the entire width of the bay, but because of winds/tides/internal waves/storms/etc some of the warm water was mixed over the top of the sill. Perhaps it would have been more realistic to parameterize these processes by using a uniform water mass in the cavity behind the sill composed of a mixture between the warm deep waters and the cold shallow waters; however, we chose to preserve some of the far-field stratification since that represented a more stringent test of the effectiveness of the sill. By preserving some of the far-field stratification, we ensured that the deep water reaching the grounding line was warmer than it would have been if we had filled the cavity with a uniform water mass.

In the aggregate volume calculations in Table 1 we did not consider any sills that partially covered the width of the bay; the only partial horizontal coverage we considered was the case of isolated pinning points, in which case we assumed 0% water blockage. The different aggregate volumes we calculated for continuous sills were entirely due to different assumptions about sill position (whether in the wide open bay or on the narrower high bathymetry near the present-day grounding line), sill height (with the sill top either 300 m, 250 m, or 100 m below the surface) and aggregate strength (with an angle of repose of 15° or 45°). Both the 50% blockage experiment and the first 100% blockage experiment use the same assumed sill geometry, corresponding to superscript (3) in Table 1. That design is described as a low sill built on the higher bathymetry near the present-day grounding line. The assumption behind that design geometry is that the grounding line would have retreated to form a large embayment before construction begins. The mouth of the embayment would then form a natural constriction (relatively speaking; the length of the sill is still 80 km) roughly at the location of the present-day grounding line, and since the present-day grounding line also has the highest bathymetry in the area, building a sill there would be doubly favored. We did not actually run any experiments corresponding to (4) (low sill in the open bay), we only included that sill geometry calculation in the table as an example of a smaller open-bay design. The most effective scenario we considered (tall sill in the open bay) was (5) in Table 1.

We have added wording in the experiment description section (3.1) to clarify our interpretation of the 50% blockage experiment. We have also added wording to the caption of Table 1 clarifying which scenarios correspond to which designs.

p. 1 l. 2: "Thwaites Glacier, West Antarctica, is the largest individual source of future sea level rise". This needs to be reworded slightly, I think. You say later of Thwaites undergoing MISI, "We regard this hypothesis to be probable but not yet proven." It seems like the abstract could use similar qualification like "will likely be" or "is projected to be".

We have added the qualifier "is projected to be".

p. 1 l. 3 "coupled ice–ocean flowband simulations". In my experience, "flow band" is a meaningful term in 2D "side-view" ice-sheet modeling that parameterizes the 3rd dimension (e.g. Price et al. 2017, doi: 10.1029/2006JF000724) but it is not used in ocean modeling as far as I'm aware. So I would suggest coming up with a different term to describe the coupled model (2D; quasi-2D; 2D, side-view; or something like that).

We have changed the word, "flowband" to "quasi-2D".

Fig. 1: I rarely say this but I think some of the text may be too big in this figure. Particularly the titles of each panel seem too large. Also, you use uppercase letters for panels in Figs. 1, 2 and 5 but lowercase for Figs. 3 and 4. I much prefer lowercase (which seems to be standard) but more importantly would like to have consistent numbering

We did not know that it was possible to have the text in a figure too big!

We also do not know how big this figure will be after final typesetting, and this cartoon is a good candidate for getting squeezed into a single column when the article gets typeset into two columns, so we prefer to leave the text large. However, we have switched to lowercase lettering for the panels in Figs 1, 2, and 5.

Fig. 2: I would leave a bit more space between each panel title and the panel itself. Also, I found it distracting that the titles seem to be in a different font from the other text (though this may just be an odd boldface font).

The title font is the same, just bold. We have moved the titles slightly up and away from the panels.

p. 3 l. 12-13: "There is also uncertainty about whether the ocean forcing that (may have) pushed the ice sheet over the edge was caused by human activity (Steig et al., 2012)" I would recommend citing a other papers that make this case more forcefully: Turner et al. 2017 DOI:10.1002/2016RG000532 (see Sec. 6. Attribution of Recent Changes in the ASE). The recent evident that Pine Island began its present retreat before the 1940s (Smith et al. 2016, DOI:10.1038/nature20136) might point to a lower likelihood that anthropogenic forcing played a role in that glacier's retreat.

We have added both of these references.

p. 3 l. 13-15: "We proceed with the understanding that the societal consequences of a collapse will be the same regardless of whether or not humanity is responsible." This point is well stated.

Thank you.

p. 3 l. 17, p. 4 l. 2: I hate to keep pushing you to equivocate more but I would suggest changing "would" to something like "would, by some estimates". I know this is implied by the citations you give but with projections in general and cost estimates in specific it doesn't hurt to be explicit about what we know vs. what can only be an approximation.

We have changed the word, "would" to, "could" in order to imply more uncertainty.

p. 4 l. 17: Are other glaciers "less challenging" simply in being smaller, or are there other aspects that make Thwaites particularly challenging? If the latter, maybe mention something about these explicitly (or tell the reader you'll get to them later).

We were mostly thinking of size here, but the severely overdeepened geometry of Thwaites without a stabilizing topographic trough or substantial ice-shelf buttressing also contribute to the difficulty. In addition, the fact that the MISI may have already been triggered in the Amundsen Sea Embayment adds an additional degree of difficulty, in that humanity may be working against the clock when it comes to developing the technological and logistical capabilities necessary to stage an intervention. We have added a sentence discussing these factors.

p. 4 l. 21: "merely piles of aggregate on the ocean floor". Would aggregate be strong enough to remain intact as the ice re-advances over it? Or might the artificial sill be weak and therefore short-lived? These are engineering challenges that are probably beyond the scope of this paper but they may figure into the feasibility if building an artificial sill strong enough to serve as an ice rise turns out to be cost-prohibitive.

These are issues to be explored in future work. We have done some experiments with sill erosion that suggest that a weak sill could still be effective in delaying an ice sheet collapse. However, those rely on an arbitrary erosion parameterization, and without some sort of calibration we do not consider those results to be meaningful.

p. 4 l. 27: "We use the least complex model that can address this question…" I get that you wanted to use a simple tool. I get, also, that it's kind of a first cut, a feasibility study. But I do wonder if the answers might not be totally different in a model that can fully represent buttressing and also the *lateral variability of the topography. I guess I'm concerned that the model might be a little too simple to be able to give you a reliable answer to your questions. The flowband model is likely more prone to MISI (both is the sense of unstable retreat and unstable readvance) than a 3D model because of the fact that buttressing is parameterized as a drag or a change in viscosity. Furthermore, the nature of buttressing represented in a 1HD model is fundamentally different from that in a 2HD model (Gudmundsson et al. 2012 DOI: 10.5194/tc-6-1497-2012). Ideally, you would validate a few of your 135 model runs with a 2HD model. If that is too much to ask, I would suggest that you include here or in the discussion a thorough airing of these potential limitations of your 1HD model, in which much of the introduction and discussion material in Gudmundsson et al. (2012) is likely relevant.*

It is true that a flowband model is incapable of truly representing the full 2HD dynamics of buttressing. However, the geometry that Gudmundsson et al. (2012) considered was a very specific one: an ice stream confined to a deep central trough, connected to a laterally confined ice shelf, with higher ground on either side. The central trough served to confine both the fast-flowing trunk of the ice stream and the floating ice shelf that formed once the centerline ungrounded. It is important to emphasize that *all* of the lateral buttressing in the Gudmundsson model came from the gradient in ice velocity between the elevated flanks and the depressed central trough. The side walls of their model domain were free slip boundaries that did not provide any drag to the flow (see section 2, “Problem Definition”, in that paper). As a result, the only part of their model ice shelf that was laterally buttressed was the area that was confined by grounded ice on the elevated flanks. Further downstream, where the ice on the flanks also ungrounded, the ice shelf was completely unbuttressed and contributed no significant resistance to flow.

The “central trough and confined shelf” geometry explored by Gudmundsson et al. (2012) is a very common geometry for ice streams and outlet glaciers, so it was a sensible choice for that study. The findings of their study have obvious implication for many overdeepened ice streams and outlet glaciers, including several in the Amundsen Sea Embayment for which the onset of the MISI has been hypothesized. Pine Island, Pope, Smith, and Kohler Glaciers all have a bedrock trough and a confined ice shelf, so the findings of Gudmunsson et al. (2012) would caution against extrapolating from the recent retreats of those glaciers to the conclusion that an irreversible collapse has begun.

However, Thwaites has neither a central trough nor a confined shelf. Though it does have some cross-flow variability, it does not have the specific structures shown by Gudmundsson to stabilize an ice stream against the MISI. Our simple force balance inversions (Fig 3c) suggest that side drag is negligible in the force balance of Thwaites compared to driving stress and basal drag. Those results are broadly consistent with more complex inversions which show that the rather high driving stress of Thwaites is balanced by local (or near-local) basal drag (Joughin et al., 2009; Morlighem et al., 2013). Even the inversion of Sergienko and Hindmarsh (2013), which probably has the most non-local stress transmission of any of the Thwaites inversions, does not show stress transmission to the margins, but rather to a specific pattern of sticky patches within the ice stream itself. The Sergienko and Hindmarsh (2013) model has a balance between driving stress and basal drag when considered at wavelengths longer than the spacing between the sticky patches, and that spacing (~11 km) is much less than the width of the glacier. One of the underlying assumptions of the canonical 1HD description of the MISI by Schoof (2007) is that basal drag and driving stress are balanced in the inland region of the ice stream, with only a small boundary region near the grounding line where longitudinal stresses are important as well. Our model represents all of those terms, and if there is any glacier in the world to which the Schoof (2007) description truly applies, that glacier is Thwaites.

Nonetheless, in a real retreat of Thwaites Glacier it is likely that different areas of the ice stream would retreat at different rates, and this asynchronous retreat would create embayments in which a well-buttressed ice shelf could form. Such temporary buttressing would probably slow the retreat, since it would preferentially apply buttressing to the areas that are retreating the fastest, and that sort of process is not something that we can represent in a 1HD model.

We have added wording to the methods section elaborating on the weaknesses of a 1HD model and commenting on the relationship between Thwaites' geometry and the geometry considered by Gudmundsson et al. (2012).

p. 6. l. 15-16: "For the 50% blockage experiment, the ocean properties forcing the sill model were a linear combination of the properties at the sill top and the far–field stratification." Could you explain this choice further? Ocean dynamics is typically mostly horizontal, suggesting that the deepest water mass would flood the cavity for any percentage less than 100% sill blockage (assuming the percentage is meant to represent a horizontal fraction of the channel width that is covered by a sill). I do not think the the choice to have colder water in the cavity because a sill blocking 50% of the channel width is not consistent with observations or modeling of ocean dynamics in similar topographies. The warmer, denser water is perfectly content to flow around the obstacle and fill the region behind it, preventing the cooler, less dense water from descending over the sill to mix at depth. I think your 50% simulation is more representative of the behavior if you had a sill that was half as high (at least from the ocean's perspective) but covered the full width.

It is absolutely correct that the ocean currents should flow around a horizontal obstacle. As mentioned above, we intended the 50% blockage experiment to represent a case where the sill was built completely across the width of the bay, but was only partially effective at preventing transport of the warm water due to winds, tides, storms, internal waves, and other sources of variability in the thermocline depth. We have added wording to clarify our interpretation of this experiment.

p. 7 l. 3-6: "The price of this feature is that our model cannot include the marine ice cliff instability, which could play an important role in accelerating West Antarctic collapse (DeConto and Pollard, 2016)." I didn't follow this argument. Are you saying that you wouldn't get accelerated calving for large cliffs because you would have a slow calving rate rather than a fast one for large H compared with H_0?

Yes, we were trying to say that our model would produce a low calving rate rather than a large one when H is much larger than H_0. We have clarified the wording here.

"However, this feature also guaranteed that our model never produced unphysically large ice cliffs in the first place, so in practice this was not an issue." Some in the field would dispute the implication that MICI requires "unphysically large ice cliffs." While that may be true, I think wading into that particular controversy is beyond the scope of this paper and should probably be left out.
Over all, found these two sentences to be strange. You suggest you're missing a potentially important bit of calving physics if you encounter large ice cliffs but then dismiss it because your calving parameterization is such that you never do encounter large cliffs. Should we be relieved or does that just point to more potentially missing physics in your calving parameterization?

This is a good point. We have removed the second sentence.

Fig 4: All fonts seem giant, but maybe this figure is meant to be smaller in the published version? As in Fig 2, the title font seems weird compared with the non-bold font and titles seem really close to the top *of each panel.*

We have moved the titles slightly away from the panels. We want to leave the font size large, however, as we do not know how big the figure will be after final typesetting.

p. 7 l. 30-33: These two sentences come as something of a non-sequitur. I presume the point is that you simply prescribed a change in the thermocline depth because you didn't feel you could derive changes from CMIP5 simulations. Even so, it's not clear where the justification for the 200-300 m shoaling comes from.

The 200-300 m shoaling is an arbitrary choice. The recent increases in sub-shelf melt and associated grounding line retreat in the Amundsen Sea Embayment have been caused by upwelling of warm CDW onto the continental shelf and associated increases of warm water transport into the sub-ice cavities, rather than substantial warming of the water mass itself. We wanted to create a forcing for the warming scenario that mimicked and magnified this trend, thus increasing the odds of collapse and making it harder to reverse the collapse with an intervention. We felt that the 200-300 m shoaling was a good order of magnitude for a plausible change in the destabilizing direction, but we wanted to put caveats at this part of the text emphasizing the uncertainty in actual projections of ocean circulation changes on the Amundsen shelf and especially in the sub-ice cavities. We have added wording here explicitly stating that the choice of 200-300 m was arbitrary.

p. 7 l. 32: Another appropriate citation here would be Little and Urban (2016, DOI: 10.1017/aog.2016.25).

We have added this reference.

p. 9 l. 1-3: "For lower blocking percentages, the water properties behind the sill were a linear combination of the far–field stratification and the water properties at the sill top." Same complaint as on p. 6: This doesn't seem consistent with ocean dynamics.

See our earlier responses.

Fig 6: I think both the y axis and the quantity being plotted in color need further explanation. Presumably the y axis is representing the percentage of model runs with that rate of sea level rise or lower, correct? Otherwise I really don't understand the y axis. Regarding the color map, is this the instantaneous rate the moment regrounding occurs? Or at the end of the 1000 year simulation? Or averaged over some time?

Before plotting, we sorted the model runs in order of post-regrounding sea level rise rate, so that is in fact the correct interpretation of the y-axis. The post-regrounding sea level rise rate shown in color is determined by the slope of a linear least squares fit of the time series between the time of regrounding and the end of the 1000 year model run. We have added wording to the caption clarifying these points.

p. 12 l. 6-7: "With knowledge of the route of ocean currents in the sub-ice cavity, it may be possible to get the water–blocking performance of a continuous sill with less material." For the reasons I discussed above, this seems unlikely to me. Ocean water at depth is efficient at flowing around obstacles. It is energetically very favorable to flow along constant density surfaces and a partial blockage is unlikely to impede the flow or reduce the temperature of water in the cavity in a way that significantly reduces melting.

We have removed this sentence. Instead, we have added a reference to the subglacial drying proposed by Moore et al. (2018). This reference works better in this position as an example of an alternative glacial geoengineering technique that could be explored, and also provides context for our mention of basal water pressure later in the discussion.

p. 13 l. 14-`5: "and it would have only a 30% probability of success" → "and our results suggest that it would…" or something along those lines.

We have qualified this sentence.

p. 13 l. 24-25: "How should the citizens of low–lying nations value ocean circulation in the sub–ice cavities of the Amundsen Sea?" Perhaps the ambiguity is intentional but it is not clear what you mean by "value". Do you mean monetary value (or at least a tangible value that can be monetized) or something more intangible and cultural, political or otherwise sociological?

We were originally thinking only of monetary/material value, but now that you point it out we quite like the ambiguity of interpretation that is possible here. We have reworded the next sentence from, "How much should the international community be willing to spend on the basal water pressure of important outlet glaciers?" to, "How much importance should the international community place on the basal water pressure of key outlet glaciers?" in order to ensure that the entire section can now be read with many meanings for "value".

p. 13 l. 24-25: "How much should the international community be willing to spend on the basal water pressure of important outlet glaciers?" I don't follow this question. Up until now, basal hydrology didn't figure into this discussion and it is not clear to me that there are any known or proposed interventions that would affect basal water pressure in a controlled way. So I am not aware of any way in which the international community could spend money on basal water pressure in any meaningful way. If the intention is to posit a fanciful means of further geoengineering ice sheets and glaciers, that probably needs to be made more explicit.

In a previous Nature Comment in which we proposed research into glacial geoengineering (Moore et al., 2018), subglacial drying was one of the ideas that we proposed as a potential avenue of research. We did not investigate that method in this paper, but this particular sentence was meant to refer to other potential intervention techniques. We do not think that our paper is the right place to get into detail about many alternative intervention techniques, but since this paragraph is discussing the merits of glacial geoengineering research on a very broad level we felt that it was appropriate to include examples of things other than the specific sills and pinning points that we considered in this paper. We have added a reference to subglacial drying in the "Cost and Feasibility" section in response to an earlier comment above, so hopefully this sentence does not appear to come out of nowhere anymore.

p. 13 l. 32-33: "However, in this case simplicity may be a virtue." I don't find that this case is made sufficiently to warrant this statement. Presumably the virtue is that you are able to perform well over 100 simulations with different model configurations. But I don't think the implications of these simplifications are sufficiently explored.

We have removed this sentence.

"Our ice model is mostly the same as the 1D model that Schoof used to define the modern theoretical

understanding of the MISI (Schoof, 2007)." A lot of literature (notably Gudmundsson et al. 2012, mentioned above) has explored the limitations of the 1HD understanding of MISI as well as 1HD approximations of 2HD buttressing.

We have also removed this sentence. We have replaced it, and the previous sentence, with: "Our model is the simplest model that can capture the mechanics of the MISI; indeed, it is mostly the same as the 1D model that Schoof (2007) used to define the modern theoretical understanding of the MISI. More advanced ice and ocean models are needed to fully explore lateral buttressing and ocean circulation in the sub-ice cavity."

As we mentioned above, Thwaites has neither a central trough nor a confined ice shelf, which are the two geometrical features which Gudmundsson used to get around the MISI in 2HD. We did include a parameterization of lateral buttressing in our model, but that term was only a small component of the force balance (Figure 3c). The buttressing that our intervention relies on is not lateral buttressing but rather longitudinal buttressing, which is fully represented in our model. We have mentioned the Gudmundsson caveat, along with the geometrical differences between Thwaites and the idealized glacier that they considered, in the methods section.

p. 14 l. 1-2: "The exact values of collapse timing, sea level rise rate, and "point of no return" (the date at which an intervention would no longer be effective) will change with more advanced models, different forcings, and different intervention designs." I think this sentence implies that differences between 1HD and 2HD modeling are likely to be in the small details. I don't think this is well established, and I would not be surprised to see qualitative changes in behavior (e.g. reduced MISI but also potentially increased difficulty re-advancing with new pinning points) with a 2HD model compared with the 1HD model used here. I feel like the tone of this sentence kind of undermines the point made just above that, "The designs we considered were very simple and our reduced dimensional model may miss important elements of the ice–ocean system."

We have added "success probability" to the list of things that might change with future work, but otherwise we are leaving this sentence as is. A reduced likelihood of unstable retreat due to the MISI, or a slower rate of retreat once the collapse is initiated, are both covered by changes in collapse timing, sea level rise rate, and the "point of no return". Similarly, slower recovery and increased difficulty re-advancing are covered under (post-regrounding) sea level rise rate and success probability.

p 14-15: I really appreciated this discussion of the political and ethical implications of this work. It is atypical of a paper in The Cryosphere but it a vital part of a discussion of a new potential geoengineering project.

Thank you.

p. 15: "Code availability. Model code available from the authors by request." Do you have a compelling reason for not making the code publicly available? If so, in my view, this should be state here. If not, I think the code should be made public (even if in an unsupported and perhaps poorly or undocumented form). I realize this is not the policy of The Cryosphere but I ask you to consider it anyway.

You are quite right, there was no good reason for us not to post the code online. It is now at github.com/MichaelWolovick/Flowline_v1.

S3: I'm wondering how you handled "subglacial lakes" between two grounded regions that are visible in some of the animations in the supplementary material. Was there any melting in these regions? Hopefully not, since these regions presumably aren't actually supplied with heat from the ocean. Also, the plume would need to be re-initialized at each grounding line, which would be technically tricky.

There was no melting in the "subglacial lakes". The plume started from the outermost grounding line. The freshwater forcing that initialized the plume was derived only from the outermost grounded region.

Typographical and grammatical corrections:

p. 1 l. 2-3 and elsewhere: "the MISI" is typically just "MISI" in most texts I've read (just as it's not typically "the WAIS", though that would make grammatical sense). Obviously, this is a matter of taste.

Grammatical conventions around acronyms are weird. People also say things like, "ATM machine" and "PIN number" and they sound correct even though those expressions are redundant. In most places where we say "the MISI" it would be inappropriate to just use "MISI" alone: or example, "The hypothesis that the MISI has already been triggered in the Amundsen sector..." makes no sense as, "The hypothesis that MISI has already been triggered...", but would sound okay if we said, "The hypothesis that a MISI has already been triggered...". For now we would prefer to leave it as "the MISI" since that actually makes sense if you expand the acronym into "the Marine Ice Sheet Instability".

p. 1 l. 3 "flowband" should probably be "flow band" or "flow-band" if you choose to retain this phrase.

We have replaced "flowband" with "flow-band". We removed the term from the abstract in response to a previous comment, but we have left the term in the main text.

p. 2 l. 5: "(MISI)(Fig 1)" would be cleaner as "(MISI; Fig 1)"

Changed.

p. 3 l. 7: "West Antarctica(Joughin" missing a space before the parenthesis.

Fixed.

p. 4 l. 18-19: "The question that we seek to answer is…" Shouldn't this be, "The questions that we seek to answer are..."?

Fixed.

p. 4 l. 27: "this question" → "these questions"?

Changed.

p. 6 l. 3: "supplementary section 1.3" should probably just be "S1.3" for consistency with the rest of the text.

Fixed.

Many places: phrases like "low–lying" and "sub–ice" are separated by en-dashes that should be normal dashes. (Presumably something the typesetter will handle.) This is as opposed to "ice–ocean", which arguably should have an en-dash.

Fixed.

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Stopping the Flood: Could We Use Targeted Geoengineering to Mitigate Sea Level Rise?

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Abstract. The Marine Ice Sheet Instability (MISI) is a dynamic feedback that can cause an ice sheet to enter a runaway collapse. Thwaites Glacier, West Antarctica, is projected to be the largest individual source of future sea level rise and may have already entered the MISI. Here, we use a suite of coupled quasi-2D ice–ocean ~~flowband~~ simulations to explore whether targeted geoengineering using ~~an~~ either a continuous artificial sill or ~~artificial ice rises~~ isolated artificial pinning points could counter a collapse. Successful interventions occur when the floating ice shelf regrounds on the ~~pinning points~~structure, increasing buttressing and reducing ice flux across the grounding line. Regrounding is more likely with a continuous sill that is able to block warm water transport to the grounding line. The smallest design we consider is comparable in scale to existing civil engineering projects but has only a 30% success rate, while larger designs are more effective. There are multiple possible routes forward to improve upon the designs that we considered, and with decades or more to research designs it is plausible that the scientific community could come up with a plan that ~~was~~ is both effective and achievable. While reducing emissions remains the short-term priority for minimizing the effects of climate change, in the long run humanity may need to develop contingency plans to deal with an ice sheet collapse.

1 Introduction

Human emissions of carbon dioxide are altering the Earth's climate in ways that are likely to have long-lasting consequences for both human societies and natural ecosystems (IPCC, 2013). Emissions cuts promised by existing national commitments are insufficient to achieve the 2°C goal set by the international Paris Agreement (UNEP, 2016). Geoengineering, in the form of either carbon removal or solar radiation management, has been proposed as a method to close this gap (Shepherd et al., 2009). Carbon removal, or "negative emissions", is a set of methods to remove CO_2 from the atmosphere and sequester it either in the ground or in the deep ocean (Shepherd et al., 2009). Solar radiation management is a method to limit the rise in global temperature by increasing the planetary albedo and reflecting more sunlight back to space, for example by injecting aerosols

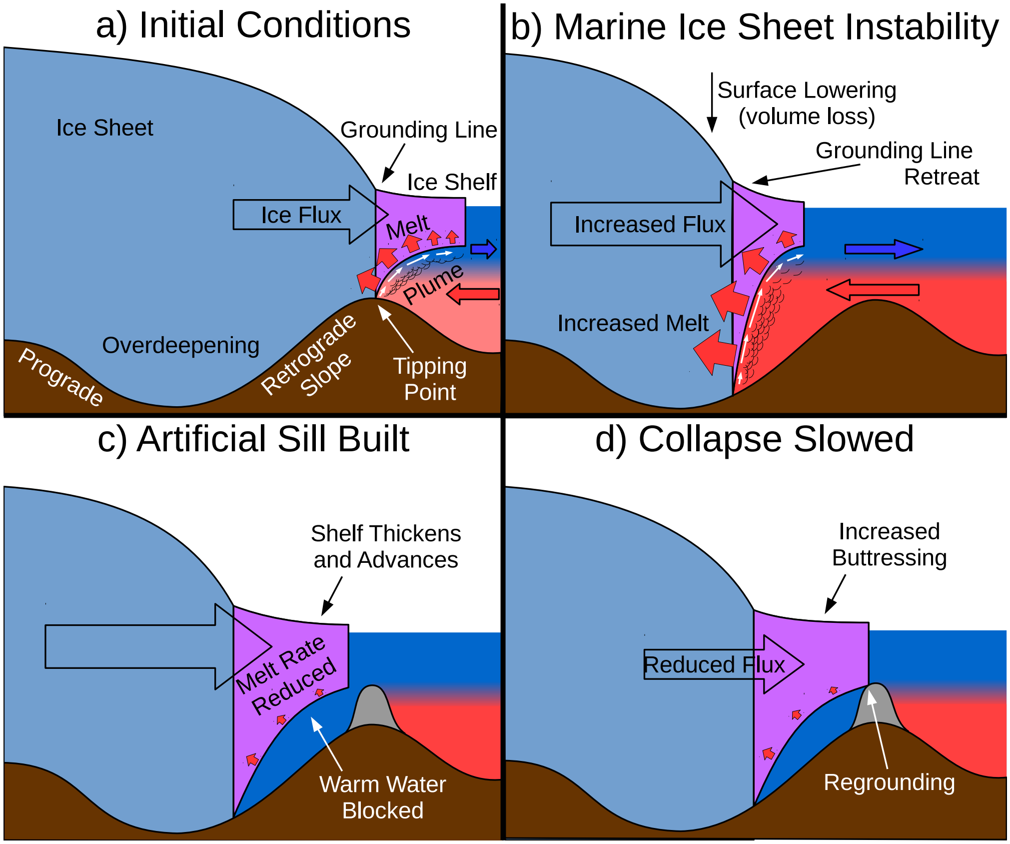

Figure 1. Schematic diagram of marine ice sheet instability and mitigation with an artificial sill. Brown represents bedrock, light blue represents grounded ice sheet, purple represents floating ice shelf, and gray represents an artificial sill. Ocean temperatures are drawn to represent the typical stratification faced by marine-terminating ice streams: warm salty water at depth and cold fresh water near the surface.

into the stratosphere (Shepherd et al., 2009). Solar radiation management has been extensively studied in the GeoMIP6 project (Kravitz et al., 2015), but its effect on the ice sheets remains unknown (Irvine et al., 2018).

Instead of trying to modify the entire climate, humanity could employ a locally targeted intervention aimed at specific high-leverage locations such as ice streams and outlet glaciers (Moore et al., 2018). Here, we explore whether it could be possible to use either a continuous artificial sill or isolated artificial ~~ice rises~~ pinning points to counteract the Marine Ice Sheet Instability (MISI~~)(~~; Fig 1). The MISI is a dynamic feedback that can cause an ice sheet to rapidly collapse due to a runaway retreat of the grounding line, the point at which the ice lifts off the bedrock and goes afloat on the ocean. Ice sheets are vulnerable to the MISI when their grounding line is located on a retrograde bed, meaning that the base slopes down towards the center of the ice sheet (Hughes, 1973; Weertman, 1974; Thomas and Bentley, 1978; Mercer, 1978; Schoof, 2007).

The instability operates as follows: as the grounding line retreats down a retrograde bed, the ice thickness at the grounding line increases, and ice flux across the grounding line increases strongly with local ice thickness (Schoof, 2007). As flux across

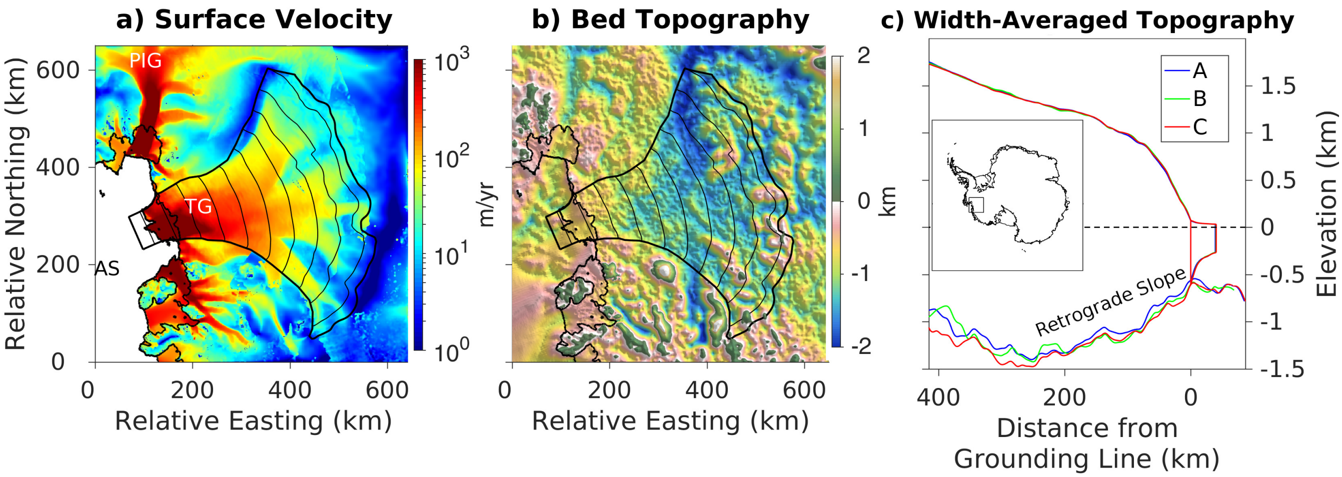

Figure 2. The Amundsen Sea sector of West Antarctica. ~~A~~a) Surface velocity (observations (Rignot et al., 2011) merged with balance velocity), ~~B~~b) bed topography (Fretwell et al., 2013), and ~~C~~c) width-averaged bed and surface profiles for Thwaites Glacier. AS=Amundsen Sea, PIG=Pine Island Glacier, TG=Thwaites Glacier. Inset shows location within Antarctica. Overlay lines show wide ~~flowband~~ flow-band boundaries and 50 km contours of ~~along–flow~~ along-flow distance, as well as grounding line and calving front. Three seperate methods were used to compute the width-averaged topography in (~~C~~c) (see S1.3). Note severely overdeepened bed geometry in (~~B~~b) and (~~C~~c). Vertical exageration in (~~C~~c) is 150.

the grounding line increases so does the rate of stretching and thinning, leading to further grounding line retreat (Hughes, 1973; Weertman, 1974; Thomas and Bentley, 1978; Mercer, 1978; Schoof, 2007). In the canonical 1D treatment of the problem, a grounding line on a retrograde slope is unconditionally unstable (Schoof, 2007). Stable grounding lines on retrograde slopes require complicating factors such as lateral buttressing, variable basal drag, or gravitational effects (Gudmundsson et al., 2012; Robel et al., 2016; Gomez et al., 2010). The initiation of the MISI is especially sensitive to basal melting caused by the presence of warm ocean waters near the grounding line (Joughin et al., 2012). Some authors have suggested that encroaching warm water has already triggered the MISI in the Amundsen Sea sector of West Antarctica (Joughin et al., 2014; Favier et al., 2014; Rignot et al., 2014), including at Pine Island and Thwaites Glaciers (Fig 2).

The hypothesis that the MISI has already been triggered in the Amundsen sector is consistent both with the available data and with glaciological theory, but the available data (mostly) begin in the ~~1990's (e.g. Shepherd et al., 2012) .~~ 1990s (e.g. Shepherd et al., 2012) . The short length of the available time series makes it difficult to draw definitive conclusions, although sedimentary evidence suggests that Pine Island Glacier, at least, has been retreating since the 1940s (Smith et al., 2017) . We regard this hypothesis to be probable but not yet proven, and we proceed with the understanding that the probability of an ice sheet collapse need not be 100% for the risk to be an important societal concern. There is also uncertainty about whether the ocean forcing that (may have) pushed the ice sheet over the edge was caused by human activity ~~(Steig et al., 2012)~~ (Steig et al., 2012; Turner We proceed with the understanding that the societal consequences of a collapse will be the same regardless of whether or not humanity is responsible.

Without extensive investments in dikes, levees, and other coastal protection infrastructure, a sea level rise of 0.6–1.2 m in 2100 ~~would produce~~ could produce ∼US$50 trillion/yr in economic losses, destruction of many coastal communities and small island states, permanent forced migration on the order of one million people per year[1], temporary population displacements of 100-500 million people per year due to ~~flooding, permanent depopulation of many coastal communities,~~ short-term flooding, and widespread loss of wetland ecosystems ~~(Hinkel et al., 2014; Jevrejeva et al., 2016)~~ (Nicholls et al., 2008; Hinkel et al., 2014; Jevrejeva The coastal protection infrastructure required to prevent (most of) that destruction ~~would itself cost US~~could itself cost $27-71 billion every year to build, maintain, and upgrade (Hinkel et al., 2014). Even with extensive investment in coastal protection infrastructure, permanent forced migration is still estimated to peak at over a hundred thousand people per year and to persist at a level of tens of thousands of people per year for centuries (Nicholls et al., 2008) .

And yet those figures are based on much less sea level rise than the 3.4 m or 19 m that would result from a collapse of the ~~marine–based~~ marine-based portions of West or East Antarctica, respectively (Fretwell et al., 2013). Glaciologists believe that this much sea level rise will probably not occur by 2100 (Bamber and Aspinall, 2013), but only because most models predict that it will take until the 22nd or 23rd centuries for a collapse to reach full speed (DeConto and Pollard, 2016; Winkelmann et al., 2015; Golledge et al., 2015). Once a collapse reaches full speed, sea level rise rates of several meters per century are common in modern models (DeConto and Pollard, 2016; Winkelmann et al., 2015; Golledge et al., 2015), consistent with geological evidence that sea level ~~rose~~ rise rate peaked at 4.1–5.3 meters per century in Meltwater Pulse 1a during Earth's last deglaciation (Deschamps et al., 2012). It is unknown if traditional coastal protection could keep up with such a rapid rate of worldwide sea level rise, and such rapid sea level rise would probably be just as harmful to society in 2200 or 2300 as it would be in ~~2100, so~~ 2100. In light of those risks, targeted geoengineering could be a cost-effective adaptation strategy.

2 Proposal

Here, we explore the possibility of using either a continuous artificial sill or isolated artificial ~~ice rises~~ pinning points to counter the MISI (Moore et al., 2018). We explore the effect of this intervention on the largest ice stream for which the MISI may have already been triggered (Joughin et al., 2014): Thwaites Glacier, West Antarctica, since if it works there then we would expect it to work on less challenging glaciers as well. Thwaites is the most difficult glacier for an intervention because it is extremely wide, severely overdeepened, and lacking either a central trough or a confined ice shelf to provide stabilizing buttressing (Fig 2). Standard theory suggests that, once initiated, the MISI must continue until the grounding line retreats onto a prograde slope or the ice sheet completely collapses (Schoof, 2007). The ~~question~~ questions that we seek to answer ~~is~~are: can an ongoing collapse be slowed or reversed by modifying the bathymetry in front of the glacier? And how much must the bathymetry be modified to achieve that goal?

[1] This approximate number was derived by taking the population exposed to a 1 m sea level rise, 131 million (Nicholls et al., 2008) , and dividing by 100 years. Most references that we saw did not give a number for permanent population displacements in the absence of coastal protection, as they considered those scenarios to be unrealistically apocalyptic. The idea that society could face such severe disruption without doing •itsomething about it was deemed politically impossible.

We envision both the sill and the ~~ice rises~~ pinning points as extremely simple structures, merely piles of aggregate on the ocean floor, although more advanced structures could certainly be explored in the future. The artificial sill would be a continuous barrier built across the front of the glacier, designed to both block warm water transport and to provide physical buttressing should the floating ice shelf reground on it. The isolated pinning points would be a line of independent mounds that are incapable of blocking warm water transport but which could provide buttressing and nucleation points for artificial ice rises should the shelf reground on them. We use a reduced complexity ice/ocean model to investigate the effectiveness of several different designs, and to explore how the effectiveness is reduced when some or all of the warm water is allowed to bypass the sill. We then discuss how these model results translate into rough design requirements for a successful intervention.

3 Methods

We use the least complex model that can address ~~this question~~these questions: for the ice, we use a ~~flowband model with~~ flow-band model with longitudinal stresses, basal drag, and parameterized lateral buttressing (S1.1), while for the ocean we use a model of the turbulent buoyant plume at the ice base (S1.2) following Jenkins' model (Jenkins, 1991, 2011). Our width-averaged flow-band model uses the Shallow Shelf Approximation (also called the Shelfy-Stream Approximation, both abbreviated SSA), meaning that our model considers velocity, viscosity, and stresses averaged in both the across-flow and the vertical dimensions. An along-flow SSA model is the minimum level of complexity needed to represent the dynamics of the MISI (Schoof, 2007) . Although we included a parameterized representation of lateral buttressing, such forces can only truly be represented in a 2HD model, and lateral buttressing is known to stabilize ice streams against the MISI (Gudmundsson et al., 2012) . However, Gudmundsson et al. (2012) investigated a very specific stabilizing geometry: a central bedrock trough that confined both the fast-flowing parts of the ice stream and the floating ice shelf. This "central trough and confined shelf" geometry may be very common for glaciers and ice streams, including for many in the Amundsen sector, but that stabilizing geometry is not found at Thwaites (Fig 2).

We used multiple ~~width–averaging~~ width-averaging schemes to produce our ~~flowband~~ flow-band profiles (S1.3) and then inverted the surface velocity data ~~long~~ along those profiles to get basal drag (S1.4). The inversion was performed for a linear sliding rule, and the inverted drag coefficient was split into spatially variable velocity and stress scales, $u_0(x)$ and $\tau_0(x)$, in order to allow subsequent experiments to change the slip exponent without rerunning the inversion. An example of the inversion results is shown in Figure 3. We also ran resolution tests of our model to ensure that it could accurately capture both the ~~steady–state~~ steady-state and the transient dynamics of the MISI (S2). The model results presented in this paper were run with a nominal grid size of 500 m and a timestep of 0.02 yr.

3.1 Experiment Description

We assembled a sample of model experiments in sets of three. We defined the ~~flowband~~ flow-band in three different ways, described in ~~supplementary section 1.3~~S1.3, to sample the uncertainty associated with using a reduced-dimension ~~flowband~~ flow-band model. We explored three separate calving laws and three separate sliding exponents, in order to sample the uncer-

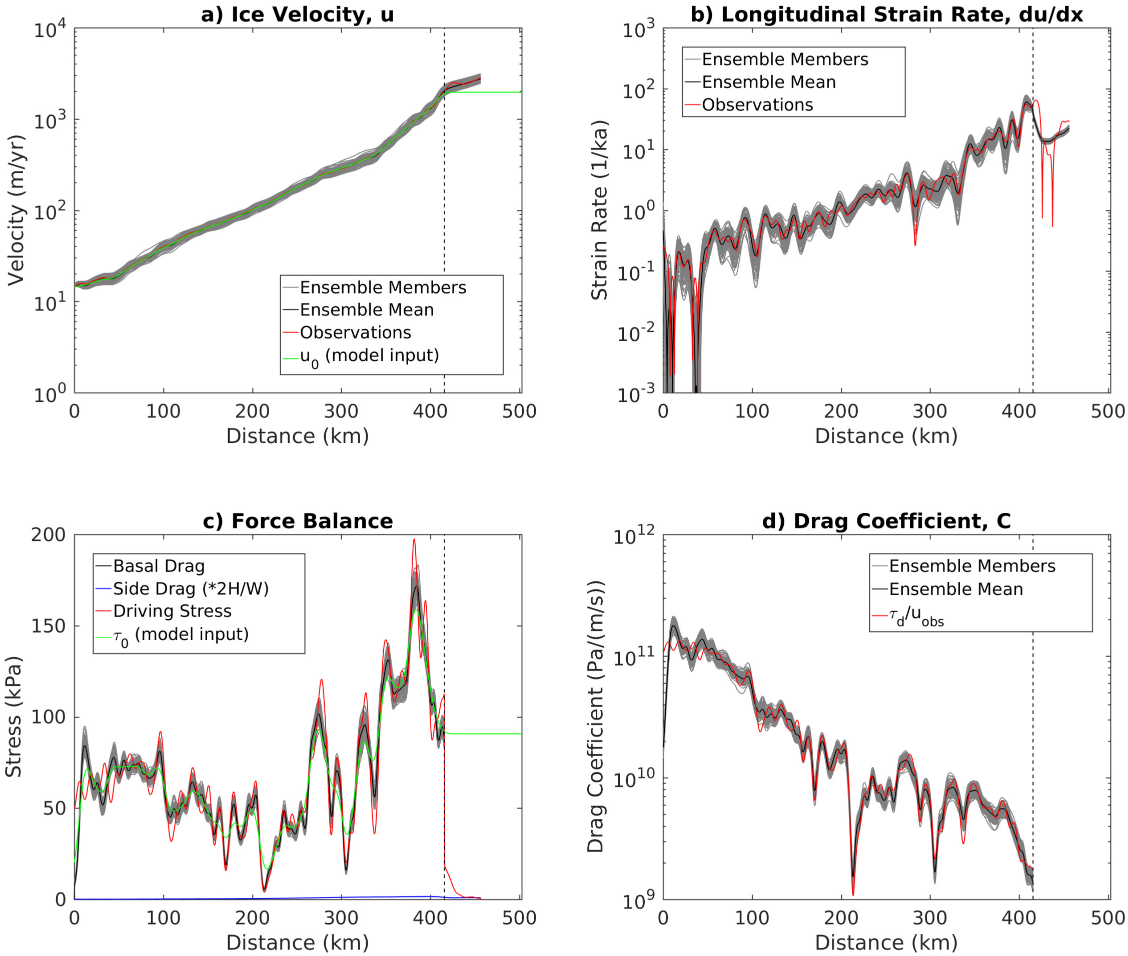

Figure 3. Velocity inversion for the C ~~flowband~~ flow-band (wide boundaries with ~~flux–weighted~~ flux-weighted averaging). a) Velocity, b) strain rate, c) stress, and d) drag coefficient. Gray lines show individual members of the final population of models within the inversion (S1.4), black lines show the ensemble mean, red lines show observations, and green lines show the quantities used as inputs to the model sliding rule. Vertical dashed line represents the grounding line; note that τ_0 and u_0 were extrapolated beyond the grounding line to allow the glacier to advance.

tainty associated with different parameterizations of physical processes. We used values of the sliding exponent of 1, 3, and 10, in order to capture a range of ice-bed properties from viscous to plastic. All of the sliding laws had the same form, namely, a power-law relationship between shear stress and slip rate (Equation S6). For iceberg calving, on the other hand, we used different functional forms reflecting different variables that could plausibly impact calving: thickness, velocity, and melt rate.

Finally, we used three forcing scenarios: constant climate control runs, climate warming runs, and climate warming runs with geoengineering. The sill was rapidly built beginning 100 years into the 1000 year model runs, with construction lasting 10 years. We ran experiments with four different designs: a tall sill built in the open bay, and a short sill built on the ~~present–day~~ present-day grounding line that blocked either 100%, 50%, or 0% of the warm ocean water. We took the experiment with 0% water blockage to represent isolated ~~ice rises~~ pinning points instead of a continuous sill; in that case the structure still provides physical buttressing if the ice shelf regrounds on it, but it does not modify the water properties behind it. We took the experiment with 50% water blockage to represent a scenario in which a sill is built across the entire width of the glacier, but where some of the warm water is mixed over the sill top by tides, winds, storms, internal waves, and other sources of variability in the thermocline depth. We limited the last three scenarios to the wide flowbands because the narrow flowbands did not enter a runaway collapse (see Results) and we were interested in the question of whether an ongoing collapse could be stopped. For the 50% blockage experiment, the ocean properties forcing the sill model were a linear combination of the properties at the sill top and the ~~far–field~~ far-field stratification. The sill was not erodible and had the same sliding properties as were extrapolated to the rest of the ungrounded region (Fig 3). Overall, we performed 135 model runs, 81 of which of which tested some version of an intervention.

3.2 Calving

For iceberg calving, we use one of three calving laws,

$$\dot{c}(H) = u_0 \frac{H_0}{H}, \tag{1}$$

$$\dot{c}(u,H) = u \frac{H_0}{H}, \text{and} \tag{2}$$

$$\dot{c}(\dot{m},H) = u_0 \frac{H_0 \dot{m}}{\dot{m}_0 H}, \tag{3}$$

where $\dot{c}$ is the calving rate, u is ice velocity, H is ice thickness, and $\dot{m}$ is frontal melt rate. Values with subscript 0 indicate constants set at the beginning of the model run, values without subscripts indicate model variables. The reference constants are taken from the present-day geometry (Fretwell et al., 2013) or frontal velocity (Rignot et al., 2011) of the glacier. For the melt-dependent calving rule (Equation 3), m_0 is taken from the geometric mean of the calving front melt rate in the first year of a model run with the front position held fixed. The melt-dependent calving rule is inspired by the melt-multiplier calving effect (O'Leary and Christoffersen, 2013). The velocity-dependent calving rule (Equation 2) is inspired by the known weakening effect of high ice velocity and associated high strain rates (e.g. Benn et al., 2007; Alley et al., 2008). All calving rules used an inverse-thickness dependence to prevent the formation of ice shelves that pinch out to zero thickness at their front. Without increased calving rates for small ice thicknesses, early versions of the model often produced ice shelves that advanced and thinned until the front pinched out to zero thickness at the waterline. Real ice shelves and tidewater glaciers almost always terminate in a frontal cliff rather than pinching out to zero thickness (e.g. Fretwell et al., 2013; Morlighem et al., 2014). The price of this feature is that our model cannot include the marine ice cliff instability, which could play an important role in accelerating West Antarctic collapse (DeConto and Pollard, 2016)~~. However, this feature also~~ , since this guaranteed that our ~~model never produced unphysically large ice cliffs in the first place, so in practice this was not an issue~~calving rule produces a low calving rate when $H \gg H_0$.

3.3 Climate Scenarios

For the constant climate scenario, we used present-day surface mass balance taken from the mean of two datasets (Arthern et al., 2006; Van de Berg et al., 2005) accessed through the ALBMAP compilation (Le Brocq et al., 2010) and ~~width–averaged~~ width-averaged onto our flowbands (S1.3). The plume model was forced by a piecewise linear stratification chosen to be similar to observations (Jacobs et al., 2011). The piecewise linear stratification is shown in Figure 4c.

For the warming scenario, we generated a schematic set of climate forcings loosely representing business as usual. Our goal was not to make a precise projection based on a specific IPCC climate scenario, but rather to capture the general features of climate warming as it affects the ice sheet in order to produce a baseline against which we could measure the performance of the intervention. This approach to the forcings is similar to that taken by, for example, the SeaRISE project (Bindschadler et al., 2013). We included surface ablation at low altitudes, a mild increase in accumulation, and shoaling of the thermocline but no warming of deep ocean waters. All changes proceeded along an exponential approach to a new steady state, with an e-folding time of 200 years. The ultimate increase in accumulation rate was 10%, similar to the values found by climate models for the Amundsen Sea sector of West Antarctica under medium to ~~high–end~~ high-end emissions scenarios (Bracegirdle et al., 2008). Surface ablation was parameterized by a rising elevation profile; at the beginning of the model run the elevation of zero ablation was assumed to be sea level, and this elevation then rose and asymptotically approached a maximum value with the same 200 year e-folding time as the other climate changes. For an assumed 6°C of eventual warming in the Antarctic, comparable with estimates from climate models running ~~high–end~~ high-end emissions scenarios (Bracegirdle et al., 2008), and a 7°C/km lapse rate, the height of zero ablation ultimately climbed 857 m. Below the elevation of no ablation, the ablation rate increased with a 1 m/yr/km lapse rate (Fig 4b). Ablation was confined to a 4 month summer ablation season. Surface melt from ablation was assumed to drain to the bed and flow to the grounding line, where it served as a boundary condition for the plume model of sub-shelf melt. Surface melt had no effect on basal sliding and no direct effect on iceberg calving, although an indirect effect existed for the melt-dependent calving law (Equation 3) because changes in freshwater forcing at the grounding line produce changes in the submarine melt rate at the calving front. The thermocline began between 700 m and 300 m, roughly following observations (Jacobs et al., 2011), and was assumed to finish between 400 m and 100 m. This choice of shoaling was arbitrary. Climate model simulations of the Southern Ocean are known to be rather poor at present with large model spread over the coming century ~~on even~~ and in some cases disagreement on the sign of ocean forcing ~~(Sun et al., 2016)~~ (Sun et al., 2016; Little and Urban, 2016) . Most models also lack the resolution and ice-sheet coupling necessary to produce good projections of circulation in the sub-ice cavities. Additionally, melt rates depend in practice on local grounded icebergs, ice shelves, and sea ice (Cougnon et al., 2017). However, it is known that the present-day grounding line retreats have been caused by increased upwelling of warm water onto the continental shelf and associated increases in warm water transport into the sub-ice cavities, rather than an increase

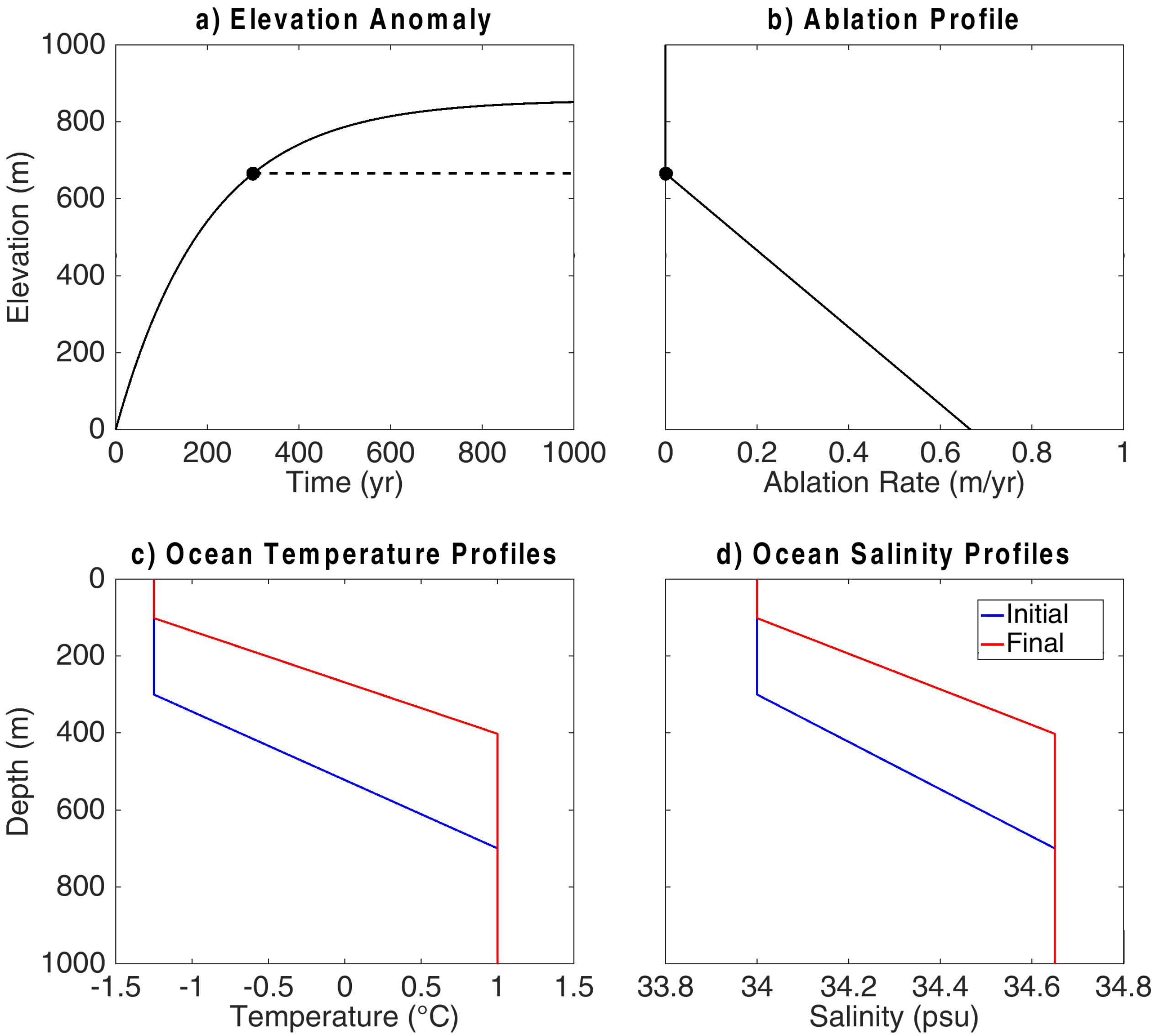

Figure 4. Climate forcing used for the warming runs. Panel ~~A~~a) shows the asymptotically rising elevation of ablation, panel ~~B~~b) shows the annually averaged ablation profile below that elevation, and panels ~~C~~c) and ~~D~~d) show the shoaling CDW. The ablation rate in any year is computed by taking the elevation of zero ablation in that year (black dot in (~~A~~a) and (~~B~~b)) and applying the lapse rate shown in (~~B~~b). Ablation is only applied during a ~~four–month~~ four month summer ablation season, so the instantaneous ablation rate is higher than the annual average shown in (~~B~~b). Net surface mass balance is the sum of the vertically variable ablation rate and the horizontally variable accumulation rate (not shown).

in temperature of the water masses themselves (e.g. Turner et al., 2017) . We therefore chose to use a forcing for the warming scenario that continued this destabilizing upwelling trend into the future.

For the geoengineering scenarios, we used the same climate forcing as the warming scenarios, but added an artificial sill after 100 years. The sill height increased linearly over a 10 year construction interval. When the sill blocked 100% of the warm water, then ocean properties at the sill top were assumed to overflow and fill the basin behind the sill. For lower blocking percentages, the water properties behind the sill were a linear combination of the ~~far–field~~ far-field stratification and the water properties at the sill top. We used a Gaussian sill profile with a 2σ width of 7.5 km (tall sill) or 5 km (short sill) to ensure that the sill was smooth relative to the model grid size. ~~The model results presented in this paper were run with a nominal grid size of 500 m and a timestep of 0.02 yr.~~

4 Results

Under constant climate forcing, 50% of the experiments that we performed on Thwaites Glacier experienced a runaway marine ice sheet collapse within 1000 years. In the warming scenario, that number increased to 70% (Animation 1)~~—~~- and all the exceptions were using a narrow ~~flowband~~flow-band. Of the model experiments that represented Thwaites with wide ~~flowband~~ flow-band boundaries, 80% collapsed in a constant climate and 100% collapsed in a warming climate. Because we are interested in the question of whether an ongoing collapse can be mitigated, we limit our analysis to the wide flowbands in the rest of the paper. Our results are consistent with the hypothesis that the MISI has already been triggered for Thwaites, but they also suggest that the probability of collapse depends both on the climate forcing and on poorly known sliding and calving processes. The exact timing and rate of the collapse varied between model experiments, but in general it was slow for the first century as the grounding line retreated the first 50-100 km from the present-day position and then accelerated as the grounding line moved onto weaker (Fig 3c) and deeper (Fig 2c) bed further inland. In some experiments the model grounding line had retreated as much as 150 km from its ~~present–day~~ present-day position in the century before the ~~sill was built~~intervention began.

Regrounding of the floating ice shelf is key to the glacier's recovery from such a severely retreated position (Animations 2-5). As the grounding line retreats onto deeper bed, ice flux across the grounding line increases, removing mass from the grounded ice sheet and adding it to the floating shelf. If this mass input exceeds basal melt and frontal calving, then the shelf will thicken and flow outward. The thickened and lengthened shelf regrounds on the sill (Fig 5d). The initial regrounding splits the ice into a well-buttressed inland shelf and a seaward shelf with little buttressing. The seaward shelf is unprotected from melt or calving and thus shrinks over time, while the inland shelf thickens and regrounds (Fig 5e). In some experiments the inland shelf completely regrounds and the glacier regains mass, while in others the innermost grounding line eventually starts retreating again and mass loss resumes, albeit at a lower rate than before (Fig 5a,f).

Both the odds of regrounding and the odds of mass gain were strong functions of the ~~sill~~ intervention design (Fig 6). Isolated ~~ice rises~~ pinning points (represented in our model by a sill that blocked 0% of the warm water) successfully regrounded 30% of the time. A smaller sill worked 70% of the time if it could block half the warm water and 90% of the time if it could block all of it, and a larger sill that blocked all of the warm water worked 100% of the time. More effective designs were also more

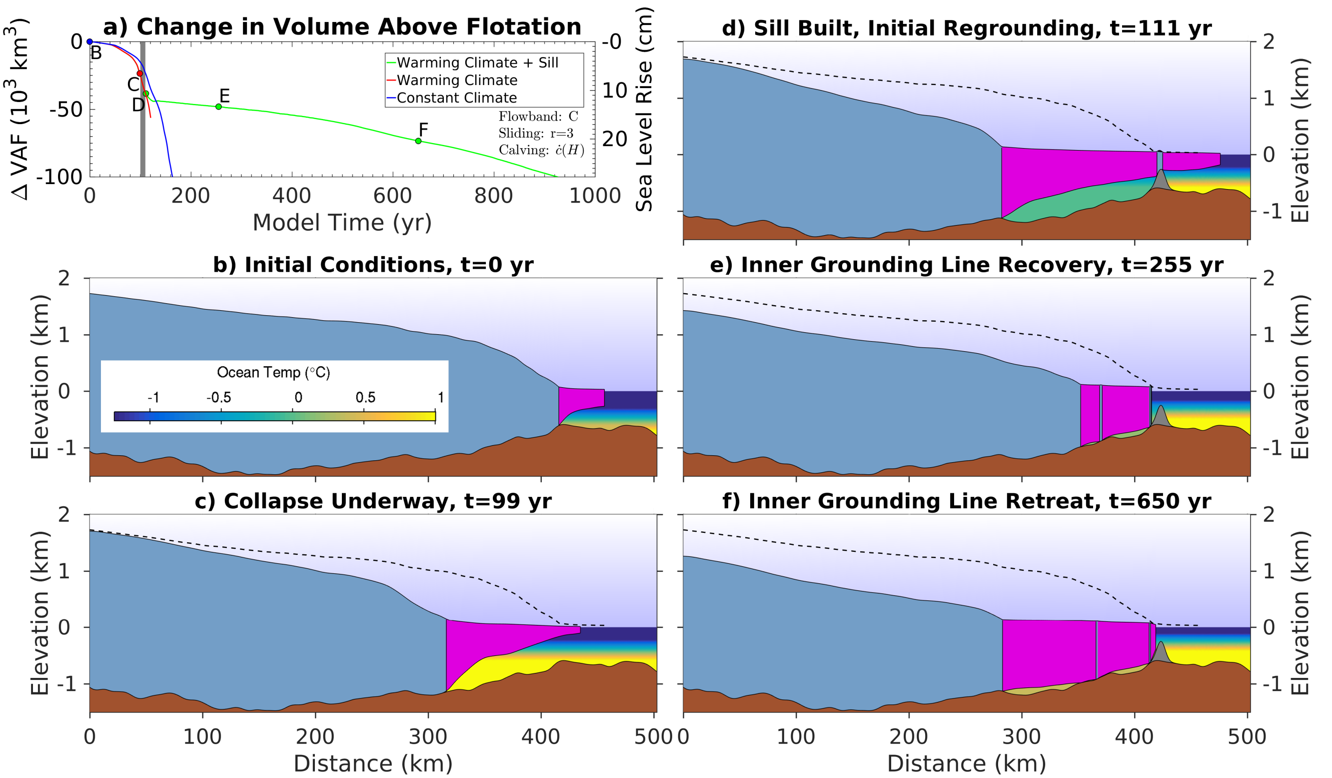

Figure 5. Example model output for an intervention that regrounded Thwaites glacier and slowed (but not reversed) sea level rise. The sill blocked 50% of the warm water in this scenario. ~~A~~a) Time series of volume above flotation and equivalent sea level rise. Vertical gray bars show when the sill was built. Snapshots (~~B-F~~b-f) show model geometry and ocean temperature. Brown regions represent bedrock, pale blue represents grounded ice, purple represents floating ice shelf, and gray represents the sill. Dashed lines represent initial ice surface. Vertical exaggeration is 50. This model run also shown in supplementary animation 3.

likely to regain mass after regrounding (Fig 6). As discussed below, the isolated ~~ice rises~~ pinning points are the only design that is a comparable scale to existing civil engineering projects, but our model results suggest a way that that design could be improved.

In some simulations in the isolated pinning points scenario, the ice shelf floated above the ~~artificial pinning point~~ pinning points but was too thin to touch down because of the high basal melt rate caused by the unblocked warm water. If the ice shelf was locally thickened above the pinning ~~point~~points, it might reground and the intervention would be successful. Over 30 years ago, ~~MacAyeal (MacAyeal, 1983)~~ MacAyeal (1983) proposed that artificial ice rises could be created in the Ross Ice Shelf by pumping seawater onto the surface in the winter, so that it would freeze in place and thicken the shelf from above. More recently, ~~Frieler and others (Frieler et al., 2016)~~ Frieler et al. (2016) proposed a similar scheme at a larger scale to offset sea level rise by adding mass to the ~~slow–flowing~~ slow-flowing areas of East Antarctica. While seawater pumping at a large enough scale to directly offset sea level rise is impractical, seawater pumping as a targeted method to thicken specific key

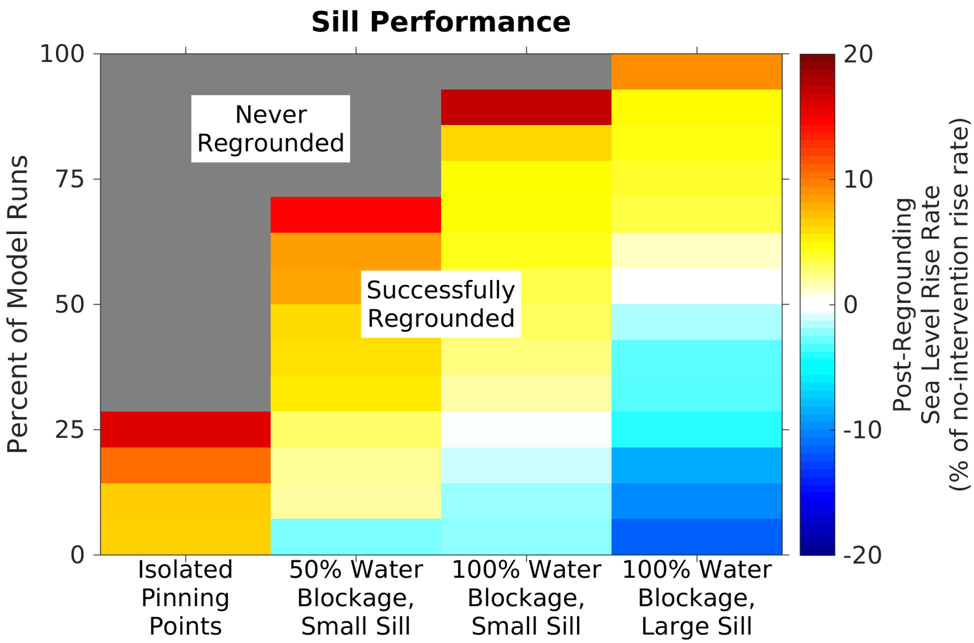

Figure 6. Summary of sill performance as a function of design scenario. ~~"~~“Isolated ~~Ice Rises"~~ Pinning Points” are represented in the model by a small sill that blocks 0% of the warm water. Results are only shown for model runs that had entered a collapse before the sill was built. Gray areas represent model glaciers that never regrounded and continued a runaway collapse. Color areas represent model runs that successfully regrounded. Color value represents the rate of sea level rise after regrounding, expressed as a percentage of the sea level rise rate without an intervention. Post-regrounding rise rate determined by the slope of a ~~sill~~linear least-squares fit to the time series between the date when the model regrounds and the end of the model run. The models have been sorted by post-regrounding rise rate before plotting, so that the value on the y-axis can be interpreted as the percentage of model runs that had a certain performance or better.

locations of an ice shelf could be more feasible. With a pinning point being built up from below at the same time as the shelf was being thickened from above, the probability of regrounding would increase. If we include the model runs where a thin ice shelf floated over the pinning point without regrounding, then the success rate for this design would double to 60%.

5 Cost and Feasibility

Estimating the monetary cost of a project that will not begin for a century or two is difficult. An accurate estimate would require making assumptions about technology, economy, and Antarctic logistics a century hence. While it is tempting to assume that the remoteness and harshness of Antarctica precludes a large civil engineering project, consider that the annual budget for the US military is $583 billion (OMB, 2017), while the logistical budget for the US Antarctic Program is only $270 million (NSF, 2017), a difference of over three orders of magnitude. If rapidly rising sea level made Antarctica a global priority, then investment in the continent could easily increase by several orders of magnitude even without accounting for future economic

Table 1. Aggregate Volume Requirements

Structure Description	Sill Length (km)	Water Depth (m)	Sill Depth (m)	Strong Volume (km^3)	Weak Volume (km^3)
Palm Jumeirah	0.10				
Panama Canal	0.20				
Hong Kong International Airport	0.30				
Suez Canal	1.0				
South to North Water Diversion Project	1.6				
Jakobshavn[1]		265	150	0.0032	0.044
Jakobshavn[2]	5	265	150	0.066	0.25
Helheim[2]	7	550	200	0.86	3.2
Kangerdlugssuaq[2]	8.5	450	100	1.0	3.9
Petermann[3]	19	350	210	0.37	1.4
Petermann[2]	20	430	100	2.2	8.1
Pine Island[1]		685	420	0.039	0.54
Pine Island[3]	40	685	420	2.8	10
Pine Island[4]	50	685	300	7.4	28
Pine Island[5]	50	685	100	17	64
Thwaites[1]		545	250	0.11	1.5
Thwaites[3]	80	545	250	7.0	26
Thwaites[4]	120	600	300	11	40
Thwaites[5]	120	600	100	30	110

Table 1. 1) isolated pinning points (two for Jakobshavn and PIG, four for Thwaites); 2) Sill built in fjord mouth; 3) sill under shelf; 4) low sill in open bay; 5) tall sill in open bay. For PIG, the sill under the shelf is located on Jenkins Ridge, while for Thwaites it is located on the high bathymetry and (relative) lateral constriction around the present-day grounding line. Note that both the 50% water blockage experiment and the 100% water blockage experiment correspond to design (3), with different assumptions about the efficacy of the design for preventing warm water from spilling over into the cavity behind the sill. For all designs, volume calculations assume that the sill is shaped like a triangular prism defined by a fixed angle of repose. Pinning points assume a conical shape with the same angle of repose. "Strong Volumes" use an angle of repose of 45°, "Weak Volumes" use 15°. Note that the "length" of the sill is the cross-flow dimension of the glacier or fjord. All volumes have been rounded to two significant figures.

growth. Consider also the rapid expansion of Antarctic infrastructure that occurred in the half century between the "heroic age" and the 1957/8 International Geophysical Year. In 1902 the entirety of humanity's Antarctic infrastructure was a wood hut by the shore of McMurdo Sound; 60 years later, McMurdo Station installed a nuclear reactor (AP, 1960).

The simple designs we envisage here allow direct comparison with existing engineering projects. A line of four isolated ~~ice rises~~ pinning points requires 0.1-1.5 km^3 of aggregate to build, depending on the strength of the aggregate (Table 1). That is comparable to the 0.1 km^3 that was used to create Palm Jumeirah in Dubai (US$12 billion), the 0.3 km^3 that was used to create Hong Kong International Airport ($20 billion), or the 1.6 km^3 that was moved for China's South to North Water Diversion Project ($80 billion). Continuous sills require one to two orders of magnitude more material than this (Table 1), but reward their increased difficulty with increased odds of success (Fig 6).

The key to improving our designs is therefore to figure out how to get higher performance from less material. Small ~~ice rises~~ natural ice rises and ice rumples presently stabilize huge areas of ice shelf (Fürst et al., 2015), so buttressing alone does not require the construction of very large structures. As discussed above, we could coordinate the construction of artificial pinning points from below with seawater pumping to thicken the ice shelf from above (MacAyeal, 1983; Frieler et al., 2016). ~~We could also try to optimize the tradeoff between warm water blocking, buttressing, and structure volume by using fully-coupled three-dimensional ice-ocean models and assimilated ice and ocean observational data. With knowledge of the route of ocean currents in the sub–ice cavity, it may be possible to get the water–blocking performance of a continuous sill with less material~~The construction of pinning points in the ocean could be coordinated with attempts at subglacial drying under the grounded ice (Moore et al., 2018) . We could also coordinate construction with an atmospheric intervention designed to remove warm water from the ~~sub–ice~~ sub-ice cavity by producing ~~downwelling–favorable~~ downwelling-favorable winds in the Amundsen Sea~~, although~~ . However, adding an atmospheric intervention to a targeted geoengineering project may make it harder to keep the side effects confined to a local area. More fancifully, some have even proposed using large fiberglass curtains to block ocean currents (Cathcart et al., 2011), which we could use to block warm water transport between the pinning points.

Regardless of what design we ultimately choose, it would be prudent if humanity attempted smaller glaciers first in order to develop technology, prove the concept, and gain experience with attempting to manage ice dynamics. For example, at Jakobshavn Glacier in Greenland, two isolated pinning points designed to jam the iceberg melange would only require 0.003-0.04 km^3 of material, while a sill that completely blocked the fjord mouth at 150 m depth would require 0.07-0.25 km^3 (Table 1), and most glaciers in Greenland are smaller than Jakobshavn. Humanity could approach the challenge of Thwaites by sequentially climbing the difficulty ladder of smaller glaciers. Each rung of the ladder will require several decades to master the new challenges that will undoubtedly appear as the scale increases.

6 Discussion

We are not advocating that glacial geoengineering be attempted any time soon. An ice sheet intervention today would be at the edge of human capabilities. ~~It~~ The easiest design that we considered would be comparable to the largest civil engineering projects that humanity has ever attempted, it would be located in a much harsher environment than the ones in which those projects were built, and our results suggest that it would have only a 30% probability of success. What we are advocating instead is the beginning of an incremental process of design improvement. In Section 5, we suggested multiple possible routes forward to improve the design, and there are likely to be many additional possibilities that we have not considered. With decades or perhaps centuries to work on the problem, the scientific community could work towards developing a plan that was both achievable and had a high probability of success.

Most of the research that needs to be done to move this process forward is research that we must do in order to predict future sea level rise anyway: coupled ice–ocean models; field studies of key glaciers; better understanding of basal hydrology, sediment transport, and erosion; oceanographic data from the ~~sub–ice~~ sub-ice cavity; calving and fracture studies; and more. Glacial geoengineering is a dramatic topic that can capture popular interest (e.g. Meyer, 2018), providing a stimulus and popular appetite for more glaciological research. Glacial geoengineering also provides an additional set of questions that can inform the way we think about ice dynamics. How should the citizens of ~~low–lying~~ low-lying nations value ocean circulation in the ~~sub–ice~~ sub-ice cavities of the Amundsen Sea? How much importance should the international community ~~be willing to spend~~ place on the basal water pressure of ~~important~~ key outlet glaciers? What exactly is the societal value of changes to the force balance of far away ice shelves? Geoengineering provides a framework for analyzing problems in glaciology that centers and quantifies the relationship between esoteric ice sheet processes and the concrete consequences of those processes for human societies and human lives.

The results that we have presented here are only the first step towards answering those questions. The designs we considered were very simple and our reduced dimensional model may miss important elements of the ice–ocean system. We only fully resolve one dimension in either the ice or the ocean, and we do not include any representation of ocean currents or mixing except in the ~~ice–contact~~ ice-contact meltwater plume. ~~However, in this case simplicity may be a virtue. Our ice model is~~ Our model is the simplest model that can capture the mechanics of the MISI; indeed, it is mostly the same as the 1D model that ~~Schoof~~ Schoof (2007) used to define the modern theoretical understanding of the MISI~~(Schoof, 2007)~~. More advanced ice and ocean models are needed to fully explore lateral buttressing and ocean circulation in the sub-ice cavity. The exact values of collapse timing, sea level rise rate, success probability, and "point of no return" (the date at which an intervention would no longer be effective) will change with more advanced models, different forcings, and different intervention designs. The robust conclusions that can be drawn from our results are: 1) regrounding an ice shelf would slow an ongoing collapse, and 2) regrounding is more likely the more warm water is blocked from reaching the ice base. Neither of these two points is controversial (e.g. Joughin et al., 2014; Seroussi et al., 2017), but taken together they suggest that consensus ice physics provide an opening for a large-scale civil engineering project to make a meaningful difference in the probability an ice sheet collapse.

One of the biggest potential failure points that must be addressed in future models is ice shelf disintegration caused by summer surface melt. The intervention we proposed relies on the buttressing force provided by the floating ice shelf in order to work, but surface meltwater damages the structural integrity of ice shelves and can cause them to disintegrate catastrophically, as Larsen B did in 2002 (Scambos et al., 2003). However, some ice shelves are protected by surface rivers that efficiently export meltwater off the shelf (Bell et al., 2017). Future research is required to determine the extent to which surface meltwater reduces the probability of success for glacial geoengineering, to quantify how that probability reduction depends on atmospheric warming and hence on carbon emissions, and to determine whether it would be possible to deliberately modify supraglacial hydrology so as to encourage meltwater export.

Regardless of whether or not the intervention is successful, it is likely to have unintended consequences. One of the advantages of locally targeted geoengineering is that many of those unintended consequences are likely to also be local in nature. In the case of an artificial sill, changes to the local ocean circulation will be extensive by design, and turbidity will be increased during construction. Both of these are likely to have effects on marine biology. Not only must all side effects be addressed in detail before the sill could actually be built, but an additional set of moral and political questions must be addressed as well.

One of those questions is the issue of decision-making. The mass balance of Greenland and Antarctica affects nations around the globe, but no legal mechanism currently exists for deciding how humanity should go about trying to control those ice sheets. Antarctica is governed by the Antarctic Treaty, but the Greenland Ice Sheet is under the sovereign control of a specific nation, with a local population of 58,000 in a ~~semi–autonomous~~ semi-autonomous relationship with Denmark (CIA, 2013). We don't know whether authority over geoengineering legally resides with Copenhagen or with Nuuk, but morally we do not believe that geoengineering should proceed in Greenland without the consent of the Greenlandic people.

Another question is moral hazard, the risk that geoengineering may be used as a political argument to justify continued carbon emissions, and that research into it will therefore undermine climate mitigation. We could counter this by pointing out that the MISI may have already begun in the Amundsen sector (Joughin et al., 2014; Favier et al., 2014; Rignot et al., 2014). If that is so, then humanity will still have to deal with an ice sheet collapse even if we stopped all emissions tomorrow. However, the point is moot if knowledge of geoengineering does not actually decrease people's support for climate mitigation, and empirical support for the moral hazard hypothesis within the social science literature is mixed (Burns et al., 2016). Properly contextualized discussion of geoengineering can actually *increase* concern for climate change (Kahan et al., 2015; Merk et al., 2016), consistent with other research demonstrating that positive, practical, or ~~solution–based~~ solution-based messaging is more effective at communicating climate science than negative, apocalyptic, or ~~fear–based~~ fear-based messaging (O'Neill and Nicholson-Cole, 2009; Feinberg and Willer, 2011).

In addition, the denialist argument that carbon emissions are justified by geoengineering is wrong on the merits. Firstly, there are many harmful consequences of climate change in addition to rising sea levels, such as droughts, floods, heat waves, extreme weather, ocean acidification, and more (IPCC, 2014). Glacial geoengineering does nothing about these other threats, or even about sea level rise due to ocean thermal expansion. Secondly, atmospheric warming increases the production of summer meltwater on floating ice shelves, which could lead ~~the~~ our intervention to fail through shelf disintegration as discussed above. Thirdly, in a warming climate the collapse of other overdeepened basins in Antarctica becomes more likely (e.g. DeConto and Pollard, 2016), multiplying the number of interventions required. Finally, even if the interventions work as intended we still could not save the ice sheets indefinitely if humanity does not get emissions under control. On millennial timescales, the evolution of the ice sheets is controlled by cumulative CO_2 emissions (Winkelmann et al., 2015). In a strongly warming world, the only viable ~~long–term~~ long-term goal of glacial geoengineering is a managed collapse.

7 Conclusions

Many of us feel an understandable aversion to the thought of deliberately controlling the Earth's climate. Locally targeted interventions may offer a milder alternative to traditional ~~large–scale~~ large-scale geoengineering. Rather than trying to manage the entire planet, we could focus our intervention on specific ~~high–leverage~~ high-leverage areas, like ice streams and outlet glaciers. This is not a project that would begin soon. ~~Field tests would be decades away at the earliest, and humanity~~ A large amount of modelling, data collection, planning, technological/logistical development, and field testing, not to mention public discussion and political debate, must be done first. Humanity might not be ready to deal with Thwaites for a century or so. In the short run our priority remains reducing emissions, because our emissions today will impact the climate for over a hundred thousand years (Keeling and Bacastow, 1977; Archer, 2005). But in the long run we need plans to deal with the committed climate changes that are already in the pipeline, one of which may be an ice sheet collapse (Joughin et al., 2014; Favier et al., 2014; Rignot et al., 2014). Those plans could include both traditional coastal protection and targeted geoengineering. Managing sea level rise at the source has the advantage of benefiting the entire world~~equally~~, while a strategy that relies only on local coastal protection is more of an every-nation-for-itself approach that may leave many poor countries behind. The ideas that we have put forward here are only the beginning of a long incremental process of design improvement that will be necessary before the scientific community settles on the right plan. Perhaps, after careful consideration, we may conclude that glacial geoengineering is unworkable and the right answer is to spend heavily on coastal protection and retreat inland where that is not practical or economical. However, we owe it to the 400 million people who live within 5 m of sea level (Nicholls et al., 2008) to ~~carefully~~ at least consider the alternatives.

Code availability. ~~Model code available from the authors by request.~~Model code and input files can be found at github.com/MichaelWolovick/Flowline_v1

Competing interests. The authors declare that they have no competing financial interests.

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