

Review of Wolovick and Moore (2018) “Stopping the Flood: Could We Use Targeted Geoengineering to Mitigate Sea Level Rise?”

Reviewer: Xylar Asay-Davis

I wish my name to be relayed to the authors, as I do not support the practice of anonymous review.

General Comments:

The paper describes a set of 135 simulations with a simplified (1 horizontal dimension, 1HD) coupled ice sheet--ocean model designed to explore the implications of constructing a sill or a series of ice rises in the vicinity of Thwaites Glacier, West Antarctica. The simulations show that the intervention is increasingly successful depending on a combination of 1) how much warm water is blocked from entering the ice-shelf cavity and 2) the amount of additional buttressing provided to the ice shelf. The authors find an ~30% of reducing retreat with an isolated ice rise but much higher odds of reducing or reversing retreat with more extensive sills of various sizes and fractional widths.

The authors also discuss various practical, ethical and political questions that arise with regard to the feasibility and desirability of a geoengineering project along the lines they explore in this paper. These include the requirement that the technological capability would need to be proven and explored at smaller scales; that local environmental and societal impacts would need to be fully explored; and the possibility that other physical processes not captured in the simulations the authors performed, notably surface melting and subsequent ice-shelf breakup, might render the intervention ineffective.

I think the manuscript presents a compelling platform for further discussion of the possible benefits but also the challenges and potential unintended consequences of geoengineering related to ice sheets and glaciers. I think the manuscript is appropriate for publication in *The Cryosphere* but would like to see so revisions that are mostly minor, as detailed 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.

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 from 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.

Specific Comments:

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”.

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).

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

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).

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.

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.

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.

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).

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 of building an artificial sill strong enough to serve as an ice rise turns out to be cost-prohibitive.

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 in 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.

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 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.

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 ?

“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?

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.

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.

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

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.

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?

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.

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.

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?

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.

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.

“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.

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.”

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.

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.

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.

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.

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

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

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

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..."?

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

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

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.