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

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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.

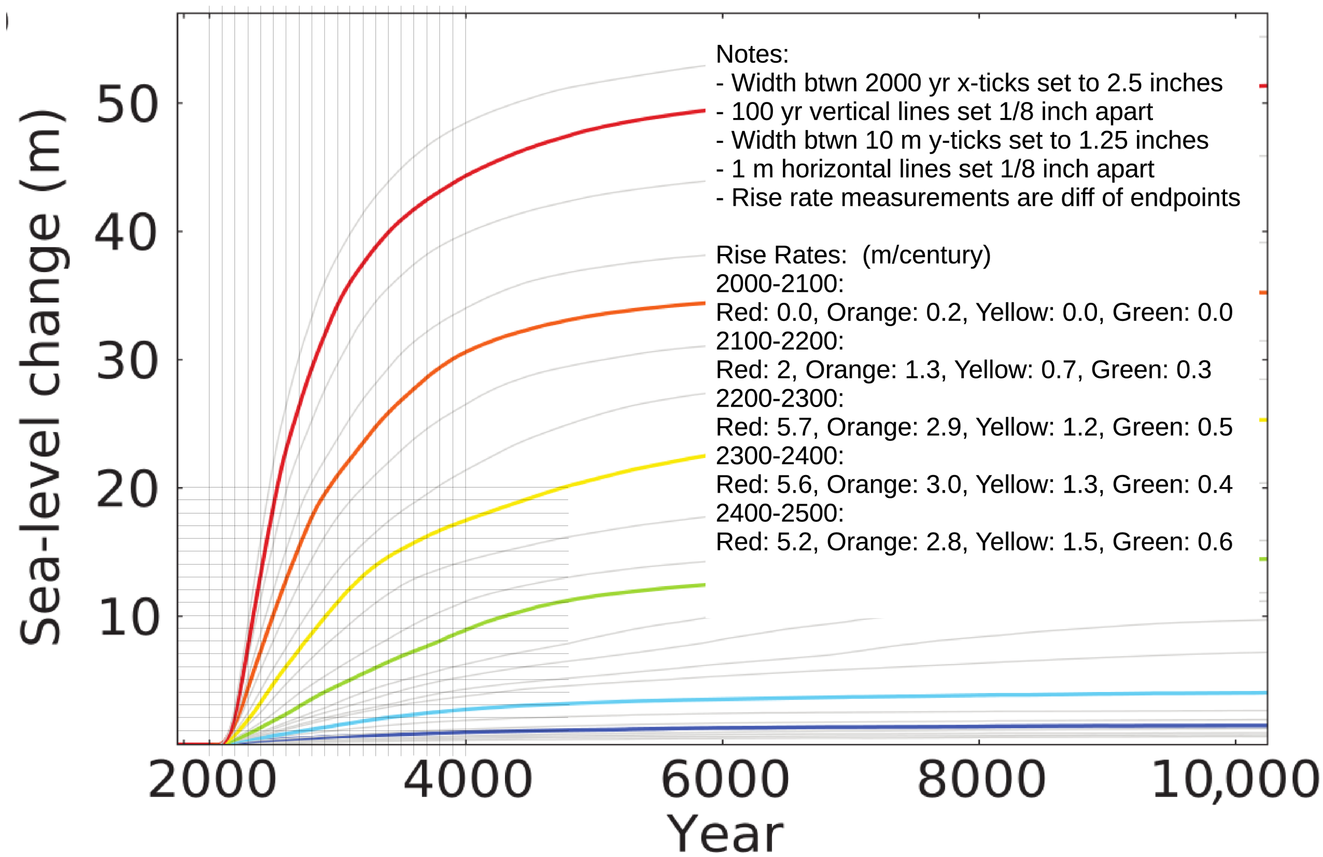

Figure 1. Our manual measurement of sea level rise rate from Winkelmann et al. (2015). The underlying figure is taken from Figure 1d of Winkelmann et al. (2015), showing cumulative sea level rise from Antarctica for a variety of emissions scenarios. We imported this image into LibreOffice Draw and manually 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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