

Review of manuscript submitted to Cryosphere: “Insensitivity of mass of Icelandic Vatnajökull ice cap to solar geoengineering” by Chao Yue, Louise Steffensen Schmidt, Liyun Zhao, Michael Wolovick and John C. Moore

#### General comments

This manuscript seems to me to be a follow up to a previous study by same authors published in Earth’s future (Yue et al. 2021) now with added ice flow model. The manuscript reads as uncompleted and hastily written afterthought that does not add much information to what was already published. Limited information about the models, limited understanding of ice dynamics (section 4 in particular) and poor presentation of the ensemble mean, rather than interesting results that the 4 ESM cause very different responses to the SAI, leaves reader with more questions than answers. Also, the fact that all the forcing fields are bias corrected (see comments below, some confusion about what is done) makes one wonder if any model dependent or physically caused impacts have been masked out with this bias correction and the observed responses therefore meaningless? Below are numerous comments about presentation and needs for clarifications. This manuscript needs major revisions.

#### Specific comments:

The title of the manuscript is misleading and even misguiding. What is “solar geoengineering”? first guess would be that some engineering is done to the sun, this phrase is not used again in the paper, but “stratospheric aerosol injection” which is not directly related to “solar geoengineering”, my suggestion is to be consistent throughout the paper about what is being discussed, injection in the stratosphere is not affecting the sun, is it? Also, the mass loss of ice caps is dependent on the energy balance at the surface, flow speed, size and location, how the connection to geoengineering is made, I find lacking explanation (see comments below). My suggestion is to change the title to suit better the content of the paper.

The most interesting results and what I would think is the main results of this study, the differences between the different ESM are not really discussed and readers are left with more questions than answers. Looking at figures 4 and 5 there are many interesting things going on, but very little discussion and even misleading text, not presenting the results (for example line 145, see comment below). Why is there so big difference between the ESMs when the impact of the SAI is observed? Comparing the volume and area evolution for BNU-ESM and HadGEM2-ES it appears that the volume loss is reduced in the G4 simulations, but the reduction happens later in the BNU-ESM, the G4 line follows the RCP4.5 until about 2060, but the G4 line is off from RCP4.5 already in 2040 for HadGEM2-ES, why is this difference? For the MIROC runs the G4 lines (volume and area) follow the RCP4.5 lines. I think therefore that the numbers given in the abstract that G4 reduces mass loss from 16% to 12% misleading, as there is so big difference depending on which ESM is applied. The ensemble means and the numbers in the abstract are really showing the value in between the little MIROC response and the much larger HadGEM2-ES response to the SAI. Why are there such big differences in the responses? Also, very interesting is the area curves for the MIROC-ESM-CHEM results, the RCP8.5 reduces the area much slower than the RCP4.5 and G4 until about 2040 when it speeds up and overtakes in ca 2070 and the area and volume loss is larger than for the RCP4.5 and G4 runs. Similar, but smaller effect is also visible in the BNU-ESM results, the area (and volume) loss of RCP8.5 is slower in the first decades of the simulations but then speeds up and overtakes the RCP4.5 and G4 losses. The difference between the RCP4.5 and RCP8.5 volume and area loss is larger at 2089 in the MIROC runs than in the BNU-ESM and HadGEM2-ES, what causes this difference? I think the ensemble mean, shown in the figures furthest to the right is misleading and does

not give much information (as the numbers given in the abstract) what is interesting, and I find missing discussion of in the paper is the variable responses of the simulations forced with the different ESMs. There is no explanation of what impact G4 has on precipitation, temperature, or circulation in the model, that would be interesting, could this be added to the discussion?

The periods of the study are not consistently written through paper and it is confusing, in line 16 and 80 the period is stated 1982-2089, in line 61 2006-2089, line 96 period is 1982-1999 and in line 103 it is 1982-2005. In line 184 the 2089 is subtracted from 2020, is that present day reference (not 1999, or 2005/6?) My suggestion would be to have the periods, reference consistent through the paper. Also, the period of the forcing is not consistent, in line 60 and 91 it is monthly, but in line 91 it is daily are both daily and monthly forcing used?

The description of the mass balance model is also not consistent and confusing, in line 79 SEMIC is introduced, but in line 82 it is stated that ESM is statistically downscaled and bias corrected using ISI-MIP, in line 97 it is stated that the spin-up is driven by SMB fields from PSIM forced with a sequence of ESM (no SEMIC or downscaling used?) in line 109 it is stated that SMB are corrected and SEMIC modelled and in line 117 it is stated that T, long wave and short wave radiation that drive SMB (SEMIC?) are bias-corrected (how?) with ERA5 reanalysis. My suggestion would be to straighten the description of what is done up and be consistent throughout the paper.

The whole section 4 reflects little or limited understanding of dynamics of ice caps and how the system responds to climate. See comments below. Ice cap in balance state loses mass at the edges and gains in the centre and the ice flow redistributes these to maintain the size and shape of equilibrated ice cap. The discussion in section 4 is strangely worded in many places and my suggestion would be to rewrite the whole section to better include known dynamics of ice caps and effect of SMB.

The Discussion section is confusing and has many unclear statements that don't make sense in the context of the presented study (see comments below) suggest reworking and clarifying and perhaps discussing the physical impacts of G4 on precipitation, temperature and why there is such a big difference between the 4 ESMs. In figure 8 results from 8 ESMs are presented, why are not all 8 used in the analysis before? The correlation between AMOC and SMB is shown, but there is no discussion of how this correlation might come about, there is no direct link, so some physical explanation of the relationship is missing.

Technical comments:

#### Abstract

Line 11-14 the first two sentence of the abstract are speculative and not useful as an entry for a paper that has title "Insensitivity of mass loss ...." Suggest to state the findings of the study in the abstract to entice readers, not start with a speculative sentence: "SAI may reduce the mass loss by slowing surface temperature rise" does it, or does it not? (see comment above on title of the paper). The second sentence does not make sense: "although SMB is affected by the local climate, the sea level contribution is also dependent on ice dynamics" - this connection Although .... also ... is strange, the sentence needs restructuring.

Line 17-19 this sentence is unclear, suggest to edit: "Ice dynamics are important for the ice cap loss rates ... but making no difference to mass loss difference under the scenario"

Line 19-20 The following sentence does not make sense either and is not really supported by the material in the paper and conclusions: ... “dynamics are remarkably insensitive to climate forcing “ dynamics of ice caps are forced by geometry (slope, thickness) and rheology (ice viscosity) and therefore strange to relate to climate forcing or because “AMOC compensation to SMB and low rates of iceberg calving” suggest to rewrite this sentences. Also, the “AMOC compensation to SMB” is not shown in the paper and calving is not really discussed either, suggest to either delete or rewrite these statements.  
Line 21-22 this statement may be true, but is not supported by material in the paper, also the sentence reads strangely, suggest to edit and clarify and make a section in paper to support this statement.

## Introduction

Line 26 “the unique climate” is strange here, every location on Earth really has unique climate, right?  
Suggest to edit sentence

Line 29 edit something strange here “which since”

Line 29 is there a reference supporting this statement?

Line 30 strange sentence, suggest to edit, glaciers in Iceland are very sensitive to changes in forcing and experience high mass throughput, Vatnajökull, the subject of this paper is however very large and is losing mass at slower rate than the neighboring Hofsjökull and Langjökull.

Line 31, suggest to delete “expected to accelerate” this is not shown in the references

Line 34 suggest to edit, strange sentence “obvious and deeply moving for Icelanders” what does that mean?

Line 37 more recent references, such as Aðalgeirsdóttir et al., 2020, Wouters et al., 2019 and Hugonnet et al., 2021 show that the mass loss rate has been slightly reduced after 2010 so this sentence should be edited.

Line 42-43 limiting global warming to less than 2°C is not an IPCC target, but the Paris agreement, IPCC is not prescriptive

Line 45 what does “relatively cheap way” mean here? Suggest to edit

Line 49 Vatnajökull is not in direct contact with the ocean (an outlet of Vatnajökull, Breiðamerkurjökull is calving into a lagoon that is connected with the ocean through a short river). Suggest to edit this sentence, calving and basal melt are not driven by changing climate or warming ocean

Line 50, suggest to delete “It is this component that we tackle here” see comment above

Line 51-53 this is very strange sentences, suggest to edit. The atypical behaviour of the North Atlantic is not discussed in this paper and neither is the compensatory effect of the climate forcing on the AMOC, suggest to either delete or explain better.

Line 55, is there a reference for this statement (warming at least twice as fast as the global mean)?

Line 57 missing “for” in front of “its”?

Line 57-58 not clear, what are “unwelcome impacts from geoengineering”?

Lines 62-64, the descriptions of the two scenarios (“close to future emissions under the 2015 Paris agreement” and “extreme failure to mitigate scenario”) are strange, suggest to use some other descriptor, like temperature by 2100 to describe these.

Line 66 Model and Verification, suggest to replace with “Validation”, the convention is to use Verification for check if code is solving the equations right, but validate to compare to observations

Line 73, delete s in schemeS, suggest to replace “ice flow” with “constitutive equation”

Line 75, something is missing “Eigen sheme” does not make sense. Suggest to refer to PISM manual or website

Line 76 suggest to edit: “surface and bedrock elevation” or geometry, these two would provide the ice thickness, so it is redundant to include also ice thickness

Line 77, missing d in re-grided what does “these” mean here? From where are these data? Some reference to essential data for this study is missing. I would suggest to refer to Björnsson and Pálsson, 2020 for the bedrock data : <https://www.cambridge.org/core/journals/annals-of-glaciology/article/radioecho-soundings-on-icelandic-temperate-glaciers-history-of-techniques-and-findings/4B1BDA5F075411D018245B4CEB7E9730> and surface mass balane a reference to Finnur Pálsson (2017) and maybe Aðalgeirsdóttir et al., where all smb data in Iceland is summarised.

Line 78, see comment above, is the daily SMB filed used or monthly as stated in line 60?

Line 82-82, what does “lapse rate approach” mean? Do you correct with a temperature lapse rate? What is the value for the rate?

Line 83, what does “in reasonable agreement” mean? Some quantification or comparison would be useful here.

Line 86 (figure 1 caption) A) is not a location map, it only shows the Vatnajökull ice cap not where it is located in Iceland, suggest to put inset map that shows whole of Iceland and where Vatnajökull is located in figure 1a), not that one ‘ is missing in Tungnaárjökull (the second a should be á) , in d) is the “annual average”? suggest to clarify

Line line 89 “equilibrium line boundary” is a strange wording, suggest to use the commonly used “equilibrium line altitude” , add something like “applied” or “assumed” before upward geothermal heat flux

Figure 1, see comment above, there is space in this figure (lower right corner) to add observed SMB that would aid the missing comparison with observation (see line 83)

Line 91, here it is stated that PISM is forced with monthly SMB fields (see comment line 78), what is the time resolution of the forcing?

Line 92-93 sentence is strange, something is missing, suggest something like: The final year of the spin-up simulation is then used as the initial condition in the experiments (or scenario simulations).

Line 96, figure 2 caption, suggest to add “simulation” after spin-up and also state if the forcing is annual, monthly or daily averaged over this period (hat is the time resolution of the forcing?) and also make sure the period is consistent, here it is stated 1982-1999, in Figure 1 the average surface mass balance is shown for the period 1982-2005.

Line 97 here it is stated that PISM is forced with 4 different ESM, is then the SEMIC model not used? See comment above, suggest to be consistent in describing the surface forcing method.

Line 99, it is strange to show the ensemble mean spatial distribution, as 2 of the models in the 4 piece ensemble have negative and 2 have positive difference, these could therefore cancel out in some location, suggest to either show only one, or all four, so it is possible to assess the performance of each simulation.

Line 101, is the magenta line the ensemble mean extent? See comment above, it is more useful to show each model separately.

Line 103 suggest to add a reference for SMB-altitude feedback. Add “change” after elevation. See comment above about the period, in caption for Figure 2 the period is stated 1982-1999

Line 104, suggest to use another word than “correct”, It is not clear that the resulting SMB is more correct than the original (how can you assess that?), in equation it is called SMB<sup>adj</sup>, why not call it then “adjusted” with more explanation?

Line 105 suggest to use different wording for “ESM-dependent “SMB lapse rate”” suggest to explain better what is meant and define what k is and how it is determined.

Line 109, see comment above, suggest to “adjusted” rather than “corrected”

Line 110, is this the modelled ice thickness in 2005? In Figure 2 it appears to be in year 1999 why is 2005 selected? See comment above, how is k determined?

Line 112-113, this text reads awkwardly, suggest to use volume change for the evolution, but here write the difference between steady state and measured, or something like that. Is the average over one year used? From Figure 2 it appears that the seasonal volume change is considerable.

Line 113 suggest to replace “Ice area loss” with difference between simulated state state and measured, see comment above. Suggest to replace “over” with “at”

Line 115, suggest to add “measured” before “ice thickness”. Also suggest to use difference between steady state (or spin-up state) and measured, rather than “changes”

Line 116, this phrasing “are consistent across all the ESMs” is strange, suggest to write something like the spin-up steady states forced with the 4 ESM have similar steady-state geometry, or something like that

Line 117 here is strange wording, suggest to replace “that drive SMB” with something mentioning SEMIC model. Here is for first time the “bias-correction with ERA5 reanalysis mentioned, it should be clearer before that the all the ESM are “bias-corrected” with the same data. In line 82 it is stated that ESMs were bias corrected using ISI-MIP. What does that actually mean? Are the annual or monthly averaged added or subtracted from the ESM values?

Line 118-124 this whole explanation is very confusing, suggest editing the whole paragraph. The discrepancies are not caused by surging glaciers, the fact that most of the outlet glacier of Vatnajökull on the north and western side are surging and the model does not include any surging could be the reason for the model failing in simulating the observed ice thickness, that should be made clearer in this paragraph. Suggest to take out “not parameterized” and use something like, not modelled or not included.

Line 127, In Table 1 only 2089 relative to 1982 is shown, not the difference during 1991-2014, was that intended?

Line 127-128 neither the overestimation of SMB nor the disappearance of fast melting region are shown, more explanation is needed here.

Line 132, suggest to edit this sentence, it is very vague and more quantification and comparison would be useful, “likely reason” and “somewhat difference ice cap geometry” could be made clearer or better quantified.

Line 135-137 suggest to edit the whole figure caption and reconsider the ensemble and scenario averaged, suggest to show only one, or maybe two (there is space in the figure for at least, if not 3 more subfigures). The text is redundant in two places “RCP4.5 and RCP8.5” are two times in same sentence and “average” and “mean”, suggest to delete one of the two occurrences.

Line 137 suggest to replace “spaced” with “spatial resolution” and replace (upper left) with (upper right)

Line 139 see comment above Table 1 does not show historical changes as stated in lines 126 ad 140  
Line

Line 141 are those 12% and 22% values relative to initial (which?) or maximum volume? It is not clear from text

Line 142, add “loss” after “volume”

Line 144 missing ‘ over second a in Tungnaárjökull

Line 145 This statement is not correct as shown in the 4<sup>th</sup> row of figure 5 for both the MIROC simulations, the difference is 0 (negative values are not shown, if there are any?) and the volume and area loss of G4 and RCP4.5 are very similar as shown in Figure 4

Line 145-146 this statement of G4 increasing ensemble ice thickness is strange, see comment above about ensemble mean not being useful, and that G4 increasing thickness is not true, the response of the model when G4 is that the thinning of the ice cap is reduced.

Line 149 see comment above, the ensemble mean is really not useful here, as it is taking the attention away from the interesting differences in the model responses.

Line 150 suggest to replace “Estimates considering ice dynamic from PISM” with “volume and area loss simulated by including ice dynamics”

Figure 5 in top line MIROC-ESM is misspelled as MIROE. The two bottom line figures should be shown with the same color scale for aiding comparison it is misleading to show differences with same color scale but different values, suggest to have both scales go to 100 m so that for example yellow color doesn’t show 50 m in one and 70 m in the other row. It is not clear (figure caption states ice thickness differences between 2089 and 1982 is it the same initial state or ESM specific 1982 state? How different are the initial states at 1982?

Table 1 In this table no historical differences are shown as stated in Lines 127 and 139 (see comments above). See comments above that the ensemble mean with 4 ensemble members is not useful here. This table shows that very little difference is between the runs that couple ice dynamics with the SMB and the runs that have only SMB, therefore the statement in abstract line 18 seems an overestimate, how is  $\frac{1}{4}$  and  $\frac{1}{3}$  difference found?

Line 163 “with maximum of more than 400 m” this seems large, given the mean thickness of the ice cap. Over how long period? What are the velocities that move this accumulated mass? Is this realistic or not?

Line 165-166 suggest to edit, “the smallest area of surface thinning” is strange wording. Also given the known higher temperature in RCP8.5 it is not surfacing that surface thinning is stronger for that scenario, by how much? Is even over the ice cap? Is it realistic differences? Why is there so little difference between RCP4.5 and RCP8.5 in the MIROC simulations?

Line 166 this sentence “Non-SMB components display the opposite pattern to SMB” should be deleted, it indicates little understanding of dynamics of ice cap.

Line 166-169 suggest to delete or edit this sentence to include ice dynamic understanding as it is written is seems like authors are analysing model results that are little understood.

Line 170-176 See comments above, the interesting results are that there is difference between the responses of the different ESM forcings, giving numbers for the ensemble (and showing in Figure 6) is hiding these interesting results.

Line 178-182 analysing the ensemble mean really hides the results shown in Figure 4, suggest to focus on that, rather than the ensemble mean with such small number of members and varying responses.

Figure 6 See comment above about the ensemble mean, the different responses between the 4 ESM is really interesting and that is lost in this figure that only shows the means and therefore misleading. Here the reference is year 2020 but both in Figure 5 and Table 1 the reference year is 1982, why not have the same reference in all figures and table? In figure b) large difference is between the dynamic (here called (dynamic), in (a) it is called (non-SMB), suggest to be consistent). How can the dynamic part be so different with same ice dynamic model? Figure 7 shows that the non-SMB part is very similar for all simulations, this figure is really strange showing such a large difference. The difference between G4 and RCP4.5 is very small, but Figure 4 shows that each of the ESM has very different response.

Line 192-196 see comment above, suggest to discuss separately each ESM response, as shown in Figure 7, than the mean. The large 95% confidence interval with N=4 clearly shows how variable the responses are.

Line 202-203 this sentence could be more clear, the non-SMB appears to have similar value throughout, which I think is clearer information than the the fraction becomes less important.

Line 207 this is strange, what about the impact on precipitation or temperature? I would think that it directly the forcing that impacts the response, rather than the degree of imbalance, could you confirm?

Line 209 "Iceland has been closer to balance until recently" is not very clear, what is recent here? The **glaciers** in Iceland were close to balance in period 1960-1995, after 1995 the mass balance became negative, and the rate of mass loss reduced after 2010.

Line 211 it is strange to discuss the relative effectiveness of SAI on reducing surface runoff, what is the effect on precipitation, temperature, atmospheric circulation?

Line 212 It is not clear what the "compensating impact of AMOC changes" are here, the correlation between AMOC and SMB is shown, but what are the physical relationship? (what effect of precipitation and temperature are caused by AMOC changes?) this needs more discussion

Line 219 what is "SMB behavior" clarification is needed

Line 222 the sentence "may induce larger dynamic effects earlier" is not clear, needs editing. The dynamic effect appears to be very similar throughout the simulations as shown in Figure 7

Figure 8 Why are now 8 different ESM shown? Why are not all included in the analysis earlier in the paper?

Line 228 "annual mean maximum" is strange here, how is it both mean and maximum?

Line 236 "effects might be expected to be rather too small to be seen" is strange here, suggest to edit section and clarify

Line 239 something is missing "changing elevation-SMB" add "feedback"?

Line 242 not clear why "extreme maritime environment" (what is extreme about it?) makes a glacier most likely to exhibit a dynamical response, suggest to edit and clarify and also why such an effect I not seen in the experiment in this study.

Line 246 The sentence "Furthermore, retreat of the margins from the ocean" is not right here, there are no outlet glaciers of Vatnajökull residing in the ocean, the Jökulsárlón is inland lagoon, connected to the ocean by a river, but it is not ocean.

Line 251-251 sentence is strange and no connection between first and second part of it, suggest to edit.

Line 255 suggest to edit "in various basin ice thicknesses by 2089" does not make sense here

Line 258 what does "the relatively parameterized SEMIC model" mean, suggest to clarify

Line 259 suggest to edit “is still not perfectly captured” better to quantify, would you expect perfect capturing? When?

Line 260-261 strange sentence suggest to edit and clarify, not clear how albedo compensates for resolution?

Line 265 what is “de-weighting” suggest to edit

Line 265-268 strange sentences and suggest to edit, it is speculative “could perhaps provide improved polar impact studies

Lines 270-275 strange section and speculative, suggest to edit or delete

Line 270 what does “not particularly effective” mean?

Line 271 “unique geographical location” is strange, isn’t every location unique? “we may infer” is strange here, suggest to delete

Line 272 sentence is strange “will not lead to greater mass loss of any glacier or ice cap” suggest to edit or delete

Line 274-275 suggest to delete. What is “palatable governance issues”? Moore et al., 2020 is not in reference list

Line 278 “reduces VIC mass loss by 4 percentage points” is strange, why not 4% ? suggest to edit

Line 279 “SAI could help preserve VIC from melting” is not true, the melting of the ice cap happens also in G4 simulations (suggest to replace “melt” with “mass loss” melting happens every summer)

Line 281 “compensating changes in temperature and accumulation due to AMOC” is not discussed before and should be better explained earlier in paper

Line 283 “VIC is relatively insensitive to climate scenario” does not make sense here ,suggest to edit or delete

Line 283 “relatively unaffected by changing air and ocean temperature” is not clear, ocean temperature does not affect dynamics as VIC is not in connection to ocean and the results of the study show that the dynamics is affected through changes in geometry of the ice cap. Suggest to edit or delete.

Line 384 the paper by Schmidt et al is now published and this reference should be replaced by the Cryosphere paper

Line 388 two places there should be ð instead of o: Aðalgeirsdóttir and Guðmundsson