Response to Reviewer 1

We thank the reviewer of the constructive reviews and suggestions. The comments by the reviewer are in indented blocks and italic fonts.

Major comments

> *As mentioned by the authors, scaling sea level rise at the scale of Greenland using results from only a few glaciers is highly speculative. However, this is still what is done in this paper without assessing the uncertainty of such a scaling. It would be important to quantify the uncertainty in the scaling by comparing results obtained with a subset of models. Also, do you think that 12 glaciers are representative of Greenland? They are many kind of different glaciers, with or without marine terminating fronts, with or without ice shelves, with completely different geometries and fjord conditions; some glaciers are mostly impacted by changes in ice front position, or subglacial hydrology(with different types of subglacial hydrology regimes, ...)? So is it reasonable to use such a small sampling of glaciers and consider that their behavior is representative of the 200 glaciers of the Greenland ice sheet?*

We agree with the reviewer that the accurate estimate of the contribution of Greenland outlet glaciers to future sea level rise will only be possible when all 200+ glaciers will be accurately modeled. The fig.14 in our paper, first of all presents the test of the "scaling up technique" used by Nick et al. (2013) who estimated the total contribution of outlet Greenland glaciers by using results of simulations made only for four major Greenland glaciers. Note that Nick et al. (2013) did not tested whether a correlation between present-day discharge and future sea level rise even exists. Here we used 12 glaciers which differ significantly by discharge and location. We do not claim that they properly represent all Greenland glaciers but, still, this is an obvious step forward compare to the previous study. We found (Fig. 14) that some correlation between present-day discharge and the future contribution to sea level rise does exist . Since we consider only a small subset of Greenland glaciers and only some sources of uncertainties, we do not think that the detailed uncertainties analysis of the relationship between discharge and sea level rise would be very helpful. However, we will demonstrate how the uncertainty ranges by the choice of glaciers in the supplementary part.

> *I don't understand why the water conditions at 400 m depth are used in the plume model for all the glaciers. All the fjords and glaciers have very different geometry conditions (with sills, ...) that should be taken into account in order to have the right conditions for the plume model. It looks like the authors are trying to set-up the initial conditions so that the glaciers are in steady state.*

This is a misunderstanding and we will improve this part of the manuscript to make more clear how we constructed the T-S profile and why we use "the water conditions at 400 m depth". Firstly, it is important to note that for all 12 glaciers used in this study, there are at least some CTD profiles from the adjacent fjords and, in spite of some problems with CTD profiles (discussed in the manuscript), the CTD profiles are always our first choice to force the plume model. However, CTD profiles are not yet available for all 200+ Greenland glaciers. This is is why (in addition to CTD profile) we tested whether T-S profiles in Greenland fjords can be constructed using the results of the ocean reanalysis project. Since the reanalysis data are only available for the open ocean, we used the nearest to the fjord's mouth reanalysis grid cells with the depth 200, 400 and 700 m (these are the top three vertical levels in the reanalysis data set). By comparing reanalysis data with the corresponding CTD data (Fig. 4 and 5) we found that at depth 200 and 400 m reanalysis and CTD data are in reasonable agreement while for the 700 m depth they are completely off, which is explained by the fact the 700m depth-points are always located outside the continental shelf .Therefore they are not appropriate to produce vertical temperature profile in the fjords . Therefore instead of interpolated between 400 and 700 m values, we choose to prescribed below 400 m as constant temperature equal to temperature at the depth 400 m in the reanalysis data.

We then compare submarine melt computed using CTD profiles with those have been computed using temperature and salinity profiles from 200 and 400 m depths in the reanalysis data. We found that results are in reasonable agreement. Therefore we recommend as a temporal option (before better data will be available) to use the nearest gridcells with depth 200 and 400 m to construct T and S profile from the reanalysis data for the fjords for which CTD data are not yet available.

Many glaciers experienced large changes over the past couple of decades and they are therefore not in steady-state. I think it is more important to have initial conditions close present state than close to a steady-state, especially as initial conditions impact the system for a very long time (much longer than the simulation time in this paper). Furthermore, it is difficult to add the present trend to the simulated changes as glaciers are not exactly linear systems.

The meaning of the "present day" should be properly defined – otherwise it causes confusion. We did not assume that the glacier are in the equilibrium state at present, i.e. in the year 2018. In our paper under "present day" we mean the years 2000 which we use as the starting time for all our forced simulations. The choice of the year 2000 is motivated by the fact that the mass loss of GrIS during the last decade of 20the century was rather small (ca. 0.1 mm/yr) compare to that has been observed in the 21st century. This justify our assumption about quasiequilibrium state of Greenland glaciers at the beginning of experiments. Some inconsistency arises from the fact that the database (BedmACHInev2) we used to initialized the glaciers at the year 2000 are actually based on the measurements made in 2008/2009.However even the total contribution of GrIS to global seal level rise during the first 8 years of the 21st century was only about 3 mm and glaciers contributed not more than half of that. This is of course a very small number compared too our total estimate of 50 mm of glaciers contribution during the entire 21st century. For this reason we see no need in adding "present day" trend to the results we obtained. To the contrary, we extracted from the results a very small trend diagnosed in the control (unforced) run.

I don't understand why only one atmosphere model is used for the future forcing while several ocean models are used.

It is well-known that for the entire GrIS contribution to sea level rise, climate change scenarios (both in term of GHGs concentration and the model output) are the major source of uncerstainties. In this this study we decided to concentrate on the new issue, namely glacier-ocean interaction. Therefore, we used output of only one regional climate model for a single climate change scenario (RCP8.5) and concentrated on the uncertainties related to parameterizations of submarine melting and calving. We agree with the reviewer that applying another atmospheric model would introduce additional uncertainties in sea level rise via different smb and subglacial discharge. We would like to address this in future work.

There is also no clear distinction between the spread in results caused by the different climate scenarios and the different initial states, and their relative importance for the different glaciers. Is it more important to improve the external forcings (and which one) or to improve the initial conditions to reduce the uncertainty in future glacier's evolution?

We do not agree with the reviewer here. In Figure 15 we demonstrate the spread of results for each single forcing scenario and therefore attribute the spread to the different initial states, thus beta, and fwd. For clarity we will state in the section future results: "We attribute the major source of uncertainty to the different combinations of the model parameters fwd and beta. "

It seems like the authors read a coupled of references [Nick et al., 2013; Goelzer et al., 2013], and keep using them all over the manuscript. They are also many regional models (that are not 1D) that should be used to compare the results of this study.

Citing of Nick and Goelzer is natural since we used a similar approach and compare our results with these two studies. Of course, we are aware about regional Greenland ice sheet modeling and, although in most cases it is difficult to compare directly our results with regional modeling, , we will include a comparison to other regional models for a broader discussion.

Finally, I must say that I am a bit tired of seeing studies based on flowline models in 2018. I agree that such studies are still very useful to investigate new processes for example, but they should not be used to do future projections of ice sheets, given the importance of buttressing, lateral effects, complex topography ... when 2D regional models can be run at high resolution and provide more accurate results.

We respect the reviewer's opinion on the issue which models should be used for future projections of ice sheets. However, the focus of our study is not on future projection of the Greenland Ice Sheet but rather on the response of outlet marine-terminated glaciers to climate change and, primarily, on the analysis of uncertainties related to two poorly constrained processes: submarine melt and calving. We appreciate the importance of buttressing, lateral effects and complex topography which unavoidably are treated in a rather simplistic way in the 1-D model. However, we doubt whether at present 2D models can really provide "more accurate results" since accurate modeling of marine terminated glaciers would require as input accurate knowledge of present and future (i) fjord bathymetry, (ii) temporal variability of the 3D fields of temperature, salinity and velocity in the fjord, and (iii) the spatial-temporal distribution of subglacial discharge of melt water into the fjords. Even at present all these characteristics are not accurately known for Greenland glaciers and in most cases they are not known at all. At the same time we agree that the paper will benefit from the discussion of model limitations and future perspectives.

Line by line comments

p.1 l.6: crudely → simplistic (models use simple parameterizations because the processes remain unknown as mentioned at the beginning of the sentence)

Agreed, we replace "crudely" with "simplistically"

p.1 l.10: Is the regional climate model used only for the SMB or also for other properties?

The other property is surface runoff as mentioned in the sentence." ...forcing the model with changes in surface mass balance and surface runoff…,"

p.1 l.12 (and l.14 and l.15): use present tense instead of past in the abstract: used →
used.

Agree, we will adapted the tense.

p.1 l.22: the scaling is quite speculative. What happens if you do the scaling with a smaller set of glaciers? What is the uncertainty in this scaling?

We thank the reviewer for this interesting suggestion. We will derive such an uncertainty as proposed.

p.1 l.19 and l.23: If I understand correctly, the numbers given here do not include the current trend in mass loss from the Greenland ice sheet (13.8 cm in the conclusions). This is rather confusing and provides numbers smaller than expected.

The numbers given in the abstract (14 mm for twelve glaciers and 50 mm obtained by scaling up for all glaciers) can be considered as complimentary to the numbers order of 100 mm computed in the coarse-resolution GrIS models (e.g. Calov et al., 2018) because in the latter the mass loss of the GrIS is mostly controlled by changes of SMB, while mass loss of outlet glaciers is primarily controlled by increased submarine melt. We will make this point very clear in the revised manuscript.

p.2 l.4: "Two processes are largely responsible": What are the other processes that *account for mass loss to a lesser extent?*

We will add the percentage in brackets: (60 %) surface melting and (40 %) dynamical processes.

p.2 l.6: "marine-terminating" → "marine terminating"

Agreed.

p.2 l.8: It is not just warming of the ocean, but also changed in the circulation.

We agree, the circulation changes led to the warming of the ocean. The sentence will now read: "...which can in turn be attributed to a warming of the subpolar North Atlantic ocean, induced by circulation changes, and increased subglacial discharge"

p.2 l.9: I doubt that the lower contribution in Fettweis et al. [2013] is 0 cm, it should be 50 mm (9 ± 4 cm). This seems rather contradictory with the actual contribution.

We thank the reviewer for spotting this mistake. 0 cm was cited in Fettweis et al. 2013 from other studies. We changed the number to 50 mm.

p.2 l.13: Adding references to papers that detail the limitations of modeling of the Greenland ice sheet would be appropriate (e.g. Goelzer et al. [2017]; Khan et al. [2014]).

We thank the reviewer for this suggestion. We will cite the above-mentioned literature.

p.2 l.18-20: I think this is disregarding all the efforts made to improve continental scale models, as some models now have a resolution of about 1 km in marine terminating glaciers [Goelzer et al., 2018]. This is also a bit oversimplifying the problem: the limitations of numerical models are not just resolution, there is also limited observations,external forcings not appropriate, ... So this part of the introduction has to be more balanced.

We fully agree with this point and will modify the introduction accordingly.

p.2 l.21-25: Following along the same line, I think jumping from continental scale 3D models to 1D flowline models is a bit reductive, as they are many things in between. Several regional models with 2D or 3D models are starting to show interesting results [Muresan et al., 2016; Bondzio et al., 2017]. Some studies even included representation of ocean with a plume model [Vallot et al., 2018]. So I think the introduction should be improved and not just reduced to Goelzer et al. [2013] and Nick et al. [2013].

We thank the reviewer for pointing out this different studies. We will mention these and several other applications of 3D models to study response on regional and shorter time scalesAs far as a very interesting paper by Vallot et al. (2018) nicely illustrates that high-resolution and physically based modeling of glacier-ocean interaction is already possible but absolutely impractical for the study of glacier response to global warming. While Vallot et al. (2018) studied only one melt season with the glacier model., they were able to run plume model only for 10 minutes and only for a small fraction of the ice front. Therefore we cannot see an alternative to highly simplified parameterization of the glacier-ocean interaction when the centennial time scale response is concerned.

p.2 l.32: "that that"

Will be deleted.

p.3 l.1: You just mentioned that the approach from Nick et al. [2013] is not appropriate, but you follow the same one, just with slightly more glaciers. I am not sure I understand the logic here.

We did not state that the approach from Nick et al "is not appropriate". This study has obvious limitations, such as using of only four largest glaciers to project the the entire 200+ glaciers contribution to sea level rise as well as a very simplistic parameterization for the submarine melt. We wrote that "we followed an approach similar to Nick et al. (2013) but with several notable improvements" and our major improvement is using of more glaciers and more physically based parameterization for submarine melt.

p.3 l.28: “with 3D ice sheet model” → “with 3D ice sheet models”

Agreed, will be changed in the revised version.

p.3 l.30: “we used instead” → “we use instead”

Agreed, will be changed in the revised version.

p.3 l.31: I agree that continental scale Greenland models are not the best tool to study these processes, but why not use 2D basin models that would at least include lateral deformations and buttressing is important to correctly capture the behavior of narrow outlet glaciers terminating in fjords.

We thank the reviewer for his suggestion but we want to point out that 1D flowline models include lateral deformation and buttressing in a simplistic manner. We will however mention the limitation of the 1D model in the discussion.

p.4 Eq.2: Can you explain the choice made to incorporate the lateral stress?

The lateral stress term is necessary because the glaciers we considered for this study, like most Greenland marine-terminating glaciers, narrow-down toward their terminus (width of the order of 5 km, besides Petermann), with velocity of the order of 1000 meters per year (10 000 m/a for Jakobshavn Isbrae, according to present observations), making it impossible to neglect lateral drag. The stress term was derived e.g. by Veen and Whillans (1996), and used by various authors since (e.g. Nick et al., 2013; Enderlin et al., 2013; Schoof et al, 2017).

p.4 l.9: Where does the basal sliding coefficient come from?

The basal sliding coefficient (equation 2) was determined, along with other uncertain parameters, from calibration to present-day state. We will give a list ob basal sliding coefficient in the SI.

p.4 Eq.5: How different is this from simply applying water pressure at the front?

It is not and we thank the reviewer for spotting the lack of explanation here. We will introduce the description of equation 5 with “while at the calving front, the difference of the hydrostatic pressure between ice and sea water is balanced by the longitudinal stress gradient”.

p.4 l.21: So is there a point exactly at the grounding line position? This should be better explained. Also, how is treated the stretching of the grid, in particular the variables assigned to the new grid points?

Yes, there is. Grid stretching is performed so that there is always a cell edge at the interpolated grounding line position. The new calving front position is determined so that the total glacier volume is not modified by interpolation. For every new point in the interior, model variables are interpolated from previous grid. The first grid point at the ice divide remains unchanged. If ice grid points on the new grid lie outside the ice domain on the previous grid, as it is typically the case for the last cell before the calving front, ice thickness from the last grid cell is extended.
We will clarify this and give a deeper explanation of the stretching grid in the next version of the paper.

p.4 l.27: How about the ice front? Does it evolve with time? And following what criteria? You need to describe the subgrid-scale treatment of the ice front.

We agree with the reviewer and will describe the subgrid-scale and ice-front treatment in the revised version of the paper.

p.5 l.2: A quick explanation of the plume model in a few sentences should be added.

Agreed, for completeness we will add and describe now the equations of plume model.

p.5 l.8: How is a vertical profile of melt applied to a 1D model, in which there is basically no vertical dimension? So what values is used for the melt (maximum, average, ...)?

The cumulative melt rate is calculated as a volume flux and added to the mass balance term. We describe in more detail the treatment of the cases floating tongue and tidewater glacier.

p.5 l.12: What happens above the plume? Zero melt?

We thank the reviewer for spotting this lack of information.
Above the plume, so if the plume ceases, we set the melt rate to a minimum background melt which is given by the last melt value of the ceasing plume. We will add the important information in the revised version of the paper.

p.5 l.17: I don't understand "added to the vertical mass balance term B". Is the melt applied to retreat the ice front? Or just to thin the ice close to the ice front? This melt should cause ice front retreat.

We thank the reviewer for mentioning this unclarity. The melt is applied to thin the front, and does not cause retreat. Thinning subsequently leads to calving.
We will explicitly describe the ice front treatment of the glacier model in the 2nd version of the paper.

p.6 l.1: "Also, did we include" → "We also included"

Will be chnaged to the reviewers suggestion in the revised version of the paper.

p.6 l.9 "BedmACHINEev2" → "BedMarchine v2". Also there is new version [Morlighem et al., 2017] that compiled all existing bathymetry data around the Greenland. p.6

We are aware of this new data set which we used when constructed vertical temperature profiles from the reanalysis data. However we derived glacier geometries when this dataset was not yet available and we had no time to repeat this work with the new dataset. We will use it in our future work.

l.15:"in the ice sheet" → "in a previous ice sheet"
the sentence will be change to: "...running the ice sheet model SICOPOLIS by Calov et al. 2018"

p.6 l.24: Why not use the mask in Calov et al. (2018)? Combining difference sources for the different datasets might lead to some inconsistencies between the datasets.

We used the same ice mask. For clarity we would write "defined by the ice mask from SICOPOLIS in Calov et al. 2018"

p.6 l.26: Explain that the change in "basal melt" refers to ice shelf basal melt and not grounded ice basal melt. I was initially confused given that the previous paragraph talks about subglacial hydrology.

Basal melt here is the melt under the grounded ice sheet, that does as well contribute with the surface runoff to the subglacial discharge. The sentence will be changed to: "In our future scenarios when simulating subglacial discharge we include changes in surface runoff, basal melt (of the grounded ice sheet)..."

p.7 Eq.9: To be honest I don't like this flux correction in the SMB. The problem of inconsistent datasets and initialization procedures is a real problem that we are facing as a community, and that deserved better treatment than a simple flux correction. This is calibrated for the initial state, but as the glacier evolves with time it is most likely not to be valid anymore. How does this correction impact the results?

The need for using of flux correction or similar methods originate from imperfectness of climate and ice sheet models and there is no reason to like it. Eventually, when ice sheet models will be improved, the flux correction will be abandoned as it happened already in the climate modeling community. However, at present, it is not possible to simulate accurately present-day elevation and spatial extend of GrIS using the SMB obtained from regional climate models. This is why we believe that using of flux correction is superior compared to using of a completely unrealistic initial state of GrIS simulated with the realistic SMB, especially, for the purpose of modeling GrIS response to climate change on centennial time scale. The dependence of simulate sea level contribution on the used corrected flux does exist, however, we found it to be not very strong for most of glaciers, by performing experiments with different relaxation times. Such weak dependence can be explained by the fact that for the outlet glaciers (unlike the rest of GrIS), changes in SMB plays only a secondary role in glaciers retreat compare to changes in submarine melt and calving.

p.8 l.19: I am confused about this comparison at different depths? Why not use temperature profiles over the entire depth? Also how did you choose these depths? Do they correspond to the depth of warm or cold water? Or the changes in the thermocline? What is the rational for this choice?

As we explained above (and we will make it more clear in the revised manuscript) we use the reanalysis data as the fallback option for the fjords for which there are no CTDs available. The reanalysis data are available at the regular grid and at the vertical level 5, 30, 50, 100,200, 400, 700, ???, etc m. Since most of submarine melting occures below 100 m and typical depth of Greenland fjords is up to 700m m, we restricted our comparison of (continuous) CTD profiles with the reanalysis data at these three available depths – 200, 400 and 700 m. The main conclusion we made is that when constructing vertical temperature profile using reanalysis data it is better (better agreement with CTD) when we fixed temperature below 400 m rather than interpolate between 400 and 700 m. We will add a corresponding figure to make this part of discussion more clear.

p.8 l.23: This is also the case for Jakobshavn (figure 4).

Yes we agree that also Jakobshavn shows the same feature but this is shown in Figure 4. The actual location of the reanalysis point is only shown in Fig. 6 exemplary for Store Glacier. We will therefore write now:

"Figure 4 and 5 compare the temperature at these depths from reanalysis data with available CTD profiles measured over past several decades exemplary for Jakobshavn-Isbrae and Store Glacier. Since Greenland is surrounded by the continental shelf with typical depths of 200-400 meters, most of the 700-meter depth points in reanalysis data are located outside the fjords in the deeper ocean, far away from the glacier mouth as shown in Fig. 6 exemplary for Store Glacier"

p.8 l.26-32: I have the impression (and this is not very clear in the manuscript) that you don't use the sill depth in the fjords to determine the water properties in front of the glacier. The sills block the warm water at depth, which can significantly impact the water properties. This should be included for the plume model. Why not use that instead of an arbitrary depth of 400 m? Accurately including the fjord properties in important to separate the response due to the trend in climate changes from the impact of local conditions of the glaciers and the fjords.

Again, we thank the reviewer for this suggestion. We will demonstrate that for temperature profiles derived from reanalysis data, changes according to a (shallow) sill depth do not improve the temperature profile in comparison to terminus-close CTD measurements.

p.8 l.29: "larger" → "deeper"

Agreed, will be changed accordingly.

p.9 l.10: Again here, why used the temperature at 400 m depth and not the temperature at the grounding line depth? I think the value used should be designed to best represent the conditions in each and every fjord instead of using a generic value systematically applied to all the fjords.

Here we only derive a trend at the 400m depth point, from CMIP 5 models, since the continental shelf only allows water masses to pass from 0 to 400 m depth and the deep bottom water controls submarine melting. We add this trend to the total temperature profile (measured and reanalysis) which includes the temperature at the grounding line depth. We will clarify this in the revised version.

p.9 l.16-21: It would be great to see the values of the different results, and especially how the different runs agree with the observations. More details on the choice of runs selected should also be added.

We demonstrate or results of the spin-up experiments of the present-day tuning in Figure 9. For completeness we now list the values of the 4 dynamcial parameters, beta and fwd range in the SI.

p.9 l.22-26: This paragraph is not clear.

Agreed. We would rewrite the paragraph to:

"Once the four dynamic parameters and the relaxation time scale are set in our pre-calibration, we switch to the coupled glacier-plume model for the spin-up experiment.

In the spin-up experiments the submarine melt rate is now derived by the plume equations which require subglacial discharge and a temperature and salinity profile as input-data. We used monthly subglacial discharge for the year 2000. Vertical temperature and salinity profiles in these experiments were taken from reanalysis data, averaged over the time interval 1990-2010 or from recent CTD data, and held constant over time. Nonetheless, in the spin-up experiments the submarine melt rate isn't necessarily constant since changes in the grounding line depth and shape of a floating tongue (if present) affect the plume equations"

p.9 l.27: scaling of what? How is that done?

That was explained in 2.2 but we will rewrite in brackets :" (factor in a range from 0.3 to 3 that multiplies the simulated melt rate profile)"

p.10 l.6: What is 3.3?

We forgot the word "section".

p.10 l.14-18: I think this could be easily simplifies in saying that you use the volume above flotation.

Agreed. We will delete the lengthy explanation with equations and added the sentence. "The contributing ice volume V_SLR is determined by the lost ice volume above flotation from each glacier"

p.10 l.21: Mention that is the present-day simulated state.

We will.

p.10 l.21: It is not clear what you mean by calving ratio.

We will explain more carefully in the revised version.

p.10 l.23: The grounding line position is not clear on the figure, the ice front position is. Also most of these glaciers do not have any floating tongue, so it would be better to use the term ice front in this case.

We will add a close up view of the grounding line position in the SI. Glaciers named as tidewater glaciers as e.g. Helheim still evolve small tongues mostly before the melt season. We will added the sentence:

“Note that we allow for small floating termini, since many tidewater glaciers still evolve them on a seasonal scale and in nature they are also mostly undercut and do not have a pure vertical cliff”

p.10 l.21-30: Do you actually want the glaciers to be stable or to be representative of the present-day conditions? Because many of these glaciers are losing mass and retreating today, so how much should a spin-up with present-day conditions lead to stable conditions?

See our response to the 3rd major comment

p.11 l.2: I thought that most of these glaciers did not have floating termini anymore!

We now address this issue by explaining why we allow for glacier tongues to evolve in our glacier model. (see answer to two comments above)

p.11 l.6 “by Enderlin ..”

Will be adapted.

p.11 l.17: The numbers you provide do not include the present day changes? This is quite surprising and ends up presenting very low sea level change numbers that are not in good agreement with today’s observations. It also questions the initialization procedure of the model, how much can we separate the present state and future changes given that the initial conditions have a lasting effect on the results.

We are not certain which numbers are meant here by the reviewer and how these numbers can be in agreement (or disagreement) with observations. In our paper we give only the contribution to SLR for the period 2000 – 2100. Our median estimate for the all Greenland glaciers based on upscaling is 50 mm, which is within the previous estimates for the same value. We argue that this number is complimentary to the SLR contribution simulated by a global GrIS model which does not account for ice sheet-ocean interaction (e.g. Calov et al., 2018). The sum of these two separate contributions (see our Discussion) gives ca. 140 mm, which is well within the range of existing estimates (e.g. IPCC, 2013; Fürst et al., 2015;). At the same time the recent estimates for the total GrIS contribution to SLR around the year 2000 is about 0.2-0.4 mm/a of which only half is attributed to the enhanced solid discharge (Enderlin et al., 2014). These numbers are not negligible but still significantly smaller than the average SLR which we simulated for the entire 21th century. Therefore the assumption we made that glaciers at 2000 were in quasi-equilibrium cannot have significant effect on our estimates for the SLR.

p.11 l.26: “excluding” → “separating”

Will adapt accordingly.

p.11 l.27: This is not very clear, try to better separate the numbers for SMB only, elevation feedback, climate change trend, ocean, ... as is done in figure 11.

We will clarify the statements.

p.11 l.30: “substantially” → “substantial”

Will adapt accordingly.

p.12 l.13: The potential SLR and grounding line retreat are actually not listed in the tables.

We disagree, since they are listed in Table 3 and 4.

p.12 l.15: “uncertainties” → “spread”

Will adapt accordingly.

p.12 l.18: There is only one model used to generate SMB, so where is the spread *coming from? It is not clear if is caused only by the different initial conditions used or if here is something else. Also, why is there only one model used to generate SMB and several for the ocean?*

The spread is actually coming from the different initial condition caused by the freshwater depth and beta. We will insert now this explanation “ Since there is only one SMB forcing the spread originates from the different initial states cause by the different fwd and beta combination.”

p.13 l.1: “1D line plume model” → “1D plume model” (same in other places in the manuscript). Also “Jenkins (2011)” → “(Jenkins, 2011)”

Will adapt accordingly throughout the manuscript.

p.13 l.12: How does that compare to other 2D or 3D models of Jakobshavn [e.g., Muresan et al., 2016; Bondzio et al., 2017]?

We will compare and discuss our resultsto the mentioned literature .

p.13 l.19-20: remove

Will adapt accordingly.

p.13 l.30-35: Use present tense instead of past tense

Will adapt accordingly.

p.14 l.5: What are the numbers for the entire Greenland if you only take the same glaciers as Nick et al. [2013]? How are these numbers impacted by the choice of glacier? So, if you only include a subset of the 10 glaciers used in this study, how does the sea level contribution of Greenland vary? It would be interesting to compute some kind of uncertainty associated with this method.

We thank the reviewer for this interesting suggestion and will derive an uncertainty estimation.

p.14 l.7: “our our”

Will be deleted.

Fig.2: Is there a white dot in the fjord? It’s not very clear. I don’t understand the choice or the use of CTD profiles. Why not use all (or a combination of the different) profiles?

Since the plume equation require the temperature of the ambient water that entrains into the plume, we chose the closest,deep avaible CTD measurement (closest to the glacier terminus) not CTDs far away. We will improve the Fig. 2.

“depth of 400 m” → “depth of at least 400 m”. “od” → “of”

Will adapt accordingly.

Fig.4: same as Fig.3

Yes, therefore we never use temperature profile from reanalysis data at 700m depth, since the are located outside the continental shelf.

Fig.6: It would be better to label all the dots (they are only 12). Again here, why use the depth-averaged temperature and not the temperature that most impact the plume model?

This was done to get a overview of how far the profiles of CTD and reanalysis data are actually of. Nonetheless, we will plot now all the CTD an reanalysis data temperature profile for each glacier in the SI .

Fig.8: Why present the results from only one ocean model and not from all of them?

Results of only one model and only for one location is shown in Fig. 8 just for illustration. The total range of temperature trends derived from different models and for different locations are given in Table 1.

What is the implication of large discrepancy at 700 m depth between the model and the CTD measurement?

The likely reason for this discrepancy (actually 1°C error is not large for the GCMs) is that the nearest model grid point with the depth 700 m is located far from the CTD location. This is why, similarly to constructions of the vertical temperature profile from the reanalysis data, where the lowest depth we used was 400 m, to construct ocean warming scenarios we also disregarded levels below 400 m and instead prescribed temperature trend simulated by CMIP5 models at the depth 400 m. Note, that for climate change scenarios we did not use absolute values but only the anomalies simulated by CMIP5 models. These temperature trends for different locations and models are listed in table 1 (SI).

> *Fig.9: Is the observed bedrock directly taken from the BedMachine dataset along thecenterline or is it representative of the entire glacier (of its entire width)? How many sta-*
> *ble states are used for each glacier? I could not find this information in the manuscript.*
> *And as mentioned above, do you really want the initial configuration to be stable or to represent the current state of the glacier? I am not sure "transparent lines" is the appropriate term.*

We will clarify the source from our bedrock. The number of stable states is depicted in the SI, for each stable state one dot is shown. We will ad the number of stable states to our parameter list.

> *Fig.10: "median-range3": repeat the superscript meaning here. Fig.11: "vom" → "from"*

Will adapt accordingly.

> *Fig.12: Try to use the same order as for Fig.11 for the lines. Fig.13: Would be better to repeat the entire caption.*

Will adapt accordingly.

> *Fig.14: What is "ocean temperature trend 1"?*

Listed in Table 1. We will correct in the revised version.

> *Tab.2: I thought that most glaciers in Greenland did not had floating termini any more,so why are there relatively large ratios of melting?*

They do, within the season the can evolve short termini that are after the melt season mostly calved. We will give some literature references for this.

> *Tab.4: It is not clear what the sum of grounding line retreat represent. It is a rather unusual metric.*

We agree with the reviewer and will change the entry in the table to the average grounding line retreat.