Response to reviewer 2

We thank the reviewer for his constructive comments. The comments by the reviewer are in indented blocks and italic fonts.

> *This paper investigates, by means of numerical modelling, the evolution of 12 outlet Greenland glaciers in the next century (2100). The employed numerical models are a 1D flowline glacier model and 1D (ocean) plume model, they are coupled together. Two aspects represent important limitations of this work: the use of a 1D glacier model for confined glaciers and the methodology followed in forcing and using the 1D coupled plume model. Some of the assumptions of this work are not properly addressed or discussed, as well as some of the consequences on the obtained results. This paper is clearly written, with the exception of some paragraphs that may lead to some confusion about the experimental setup (e.g. It is not clear if you actually run SICOPOLIS or not. Including a "methods section" may ease the reading).*

We run SICOPOLIS and details to this can be found in our earlier study Calov et al 2018. All coauthors in this current paper contributed also to Calov et al. 2018. For this manuscript, only the output data on subglacial discharge from Calov et al 2018. were used to force the coupled glacier plume model.

Main comments

> *On the plume model:*
> *I think that using the coupled 1D plume model is a great improvement. However some experimental choices limit the validity of this improvement.*
> *At page 5 – line 2 is written that "since the plume model in some cases underestimate...*
> *we also scale the simulated melt rate profile by a factor Beta...".*
> *I have some comments on this: the relation between the plume forcings (temperature,salinity, shelf/tongue slope, subglacial discharge, . . .) and melt rate is given by robust physical equations (Jenkins, 2011; Beckmann et al. 2018). I believe that tuning the obtained melt rates with a multiplying factor waste all the efforts made in using (and coupling) the plume model. What is the need of this sophisticated model if then the computed melt rates are scaled to observed melt rates? Then why not using a simple depth dependent parameterization (e.g. Martin et al., 2011)?*

Indeed, Jenkin's model of turbulent plume is based on the first principles and therefore it is expected it provides robust qualitative relationship between submarine melt ,ocean temperature and the slope of glacier front. Whether this model is also quantitatively correct for each Greenland fjord is another issue. The real world is very different from the assumptions behind the linear plume model since during summer season significant amount of melt water is delivered into the fjord through a number subglacial channels. . At present, there is no way to simulate realistically the large ensemble of different plumes, as well as many other processes (tidal circulation in the fjord, undercutting, etc) which may also contribute to submarine melt. To describe this complex reality we proposed to use the Jenkin's linear plume model but with additional correction by parameter beta. Obviously there is no prove that this parameter will stay constant for the next 100 years but still we believe that our approach represent an important improvement compare to a much simple parameterization (we assume that the reviewer means here the parameterization by Beckmann & Goosse, 2003) since we explicitly account for the dependence of submarine melt on subglacial discharge which is very important factor for the global warming simulations.

You tuned the computed plume melt rates on present day observed melt rates. How can you assume that this "present day" scaling will still be valid in 50/100 years? This choice is crucial in terms of providing a robust basal forcing for the glaciers evolution. I think that this assumption should be discussed.

As we explained above, there is no reason to expect that a very simple Jenkin's linear plume model can accurately described complex reality of Greenland fjords even at present and nd there is reason to expect that correction parameter beta will remain constant over 50 or 100 years. The reviewer is absolutely right (see Fig. 15): the choice of melt and calving parameters is the source of the largest uncertainties in glaciers contribution to future SLR and one of the aim of our paper is to report this problem. How to fix this problem is beyond the scope of this paper.

Given the inherent large uncertainties in forcing conditions (both in CTD and in re-analysis, page 8 line 3) what about forcing the plume model with a range of plausible temperature and salinity (from CTD and/or reanalysis) and with a range of subglacial discharges instead of tuning the computed melt rate?

Obviously, uncertainties in temperature profiles and subglacial discharge also contribute to the SLR uncertainties but very unlikely they contribute to the discrepancy between melt rate simulated by Jenkin's model an real one. Indeed, typical uncertainties in water temperature of 1°C will result in 20% uncertainties in melt rate. The uncertainties of 50% in subglacial discharge results only in 15% uncertainties in melt rate (due to cubic root dependence). At the same time, as we show in Beckmann et al (2018), melt rate simulated by linear plume model can deviate from observed one by factor 2-3.

It is not clear why you decide to use reanalysis data at 200, 400 and 700 meters of depth instead of using continous vertical profiles. Moreover, for future simulations you say: "...closest 400m-depth-point neighbor...". Is this motivated by line 29 to 31 at page C28? I understand this choice but I believe that you shold explain this better, clearly motivating also at page 9.

The first reviewer has a similar question which is addressed in our response. Obviously, this part of our paper was not clear enough and will improve it in the revised version.

On the glacier model:
I get why you decide to use a 1D flowline model: however I think that the limitations related to this approach (neglect of processes at the lateral boundaries and of buttressing, which play a crucial role in the evolution of ice masses) are not properly tackled and are mostly addressed by saying that 1D models are the only one available for this kind of study. This is probably right if you want to model 12 (or more) glaciers at the time, but for single glacier the last few years have seen important improvements in modelling alternatives that have produced results for some glaciers that are also modelled in this work (Chaulet et al., 2012; Seddik et al., 2012; Muresan et al., 2016; Peano et al.,2017; Goelzer et al., 2017). I think that the discussion about 1D model limitations should be expanded.

We agree with the reviewer and will discuss more in depth the limitation of a 1D glacier model. Also will we introduce more work from other authors on 3d models on glaciers.

Specific comments

Page 1 – line 15: "factor analysis". With factor analysis it is usally meant a statistical method like the Empirical Orthogonal Functions (EOFs), in your work you just exclude
(one at the time) the different forcings, I would not strictly define this procedure as a factor analysis.

We will change “factor-analysis” to “ sensitivity analysis of the forcing-factors”.

Page 2 – line 5: instead of “global” I would use “atmospheric”

We will adapt accordingly.

Page 2 – line 4 to 8: I found this paragraph ok, but I would rearrange it a little bit putting the described processes in the same order you are introducing them.

We will improve this paragraph.

Page 2 – line 6: “marine terminating” instead of “marine- terminating”

We will adapt accordingly.

Page 2 – line 16: “In order to...” this should be a new paragraph

We will adapt accordingly.

Page 2 – line 32: “that” is repeated two times
We will delete the second ‘that’.

Page 2 – line 35: “Since we are..” this should be a new paragraph

We will insert a new paragraph.

Page 3 – line 1: I would say that the main (and only) improvement consists in using the coupled plume model. I consider the fact of studying more glaciers just as an “extension” of Nick et al. 2013 work.
Moreover, from the scaling perspective, are we sure that the considered glaciers are really representative of all the Greenland
glaciers? especially given their variety in terms of glaciers and of confining fjords geometries/conditions.

Agreed, we will change the part to:, “...we followed an approach similar to (Nick et al. 2013) but with one notable improvement.: For calculations of the vertically distributed submarine melt, we used a turbulent plume parameterization following (Jenkins et al. 2011).”
We considered 12 glaciers as in improvement compared to Nick et al. and selected them since they represent different ice flow regimes and different environmental conditions. Nonetheless a greater sample size could decrease always uncertainty, which is a factor we will add in the discussion.

Page 3 – line 4: ok, but submarine melt rate depends also on the geometrical features
of the tongue (shape, slope,…)

Agreed, we will change the sentence to. “According to this parameterization, the submarine melt rate depends not only on ambient water temperature in fjords but also on seasonally varying subglacial discharge, shape and angle of the glacier tongue.”

Page 3 – line 9 to 11: Maybe you can think about shortly describing how the scaling works.

Agreed we will give a short explanation of the scaling here.

Page 5 – line 1 to 5: I would expand the plume paragraph since it is the real innovative part of this study. Maybe a short introduction of the basic physics and *equations. Otherwise is not clear what do you mean with the E entraiment parameter unless looking at Beckmann et al. (2018) (or already knowing what you are talking about).*

We agree with the reviewer and extended now the whole paragraph and add the equation for the plume model.

Page 5 – line 17: "to the vertical mass balance term B", add the equation number

We will add the equation number.

Page 5 – line 18 to 20: I imagine that when the plume detaches the melt rate is set to zero but this is not written explicitly. Is this the case?

We thank the reviewer for spotting the lack of information.
The plume never detaches from the glacier in the model, it only ceases by slowing down the velocity to zero. When this happens, the melt rate is set to a minimum melt rate to ensure background melting. We will describe this in the revised version as the following: ". If the plume already ceases before reaching the calving front x_cf, we numerically introduce a minimal background melting determined by the last melt rate value before the plume ceased".

Page 5 – line 21: this part confused me. "...off-line using the ice sheet model" which one? This is the first time that you mention the use of an ice sheet model. Later it appears that it is SICOPOLIS.(see comment to page 6 – line 15 to 25)

Yes, we used SICOPOLIS output data which is described detailed in Carlov et al. 2018. We will change the sentence to:
"Subglacial discharge Q for each glacier was provided off-line by the data output from simulations of the ice sheet model SICOPOLIS (Calov et al. 2018) with explicit treatment of basal hydrology (Section 3.3), then applied to the line plume in distributed form $q= Q(W)^{-1}$."

Page 6 – line 1: "did we" "we did". Could you explain better in what this upscaling consists and how it works?

Agreed, we will describe better the upscaling.

Page 6 – line 2: it would add more clarity defining what is meant with "melting to calving ratio"

Ageed, we will insert" (Grounding mass flux lost by submarine melting divided by mass loss of calving)"

Page 6 – line 12: just a detail: I would number the figures in the order of appeareance in the manuscript.

Agreed,we will adapt accordingly.

Page 6 – line 15 to 25: From here it looks you actually run the ice sheet model, is this correct? (look comment to page 10 – line 6).I suggest to introduce explicitly the fact that you have run SICOPOLIS.

Yes, we run Sicopolis earlier and details can be found in Calov at el 2018. However, after the SICOPOLIS simulation we used the subglacial output data to force the coupled glacier plume model off-line. The sentence will be changed to:
"The former two sources are computed directly from the ice sheet model SICOPOLIS by (Calov et al. 2018)."

Page 6 – line 23,24: "...is assigned to the closest glacier within a maximum of 50 km". This is an important approximation since is related to the plume forcing, *however is not properly discussed, expecially in terms of uncertainty in the obtained results.*

We will discuss this more in the revised version

Page 6 – line 27,28: "neglect the effect of grounding line retreat".As above, this represents another important assumption but it is not properly discussed.

True. We will add :
"For neighboring glaciers with a competing catchment area, a strong grounding line retreat may strongly affect the distribution of the subglacial discharge between those glaciers (water piracy, Lindbäck et al 2015). This affect is not included in this study."

Page 8 – line 12: "...presence of sills in the fjord...in the vicinity of the glacier front." I would explain why is that after this line, instead than explaining it later for the continental shelf (at page 8 – line 24 to 30).

We will adapt accordingly.

Page 9 – line 16: could you provide a table with the prescribed submarine melt rate and the range of values for the dynamic parameters? (maybe in the supplementary)

Agreed, we will provide such a table in the SI.

Page 9 – line 25: with "...only factors.." do you mean that since temperature and salinity are "held constant" (thus not changing) their contribution in impacting melt rates is constant in comparison to the impacts due to a varying grounding line depth and tongue shape/slope? I suggest to reformulate this paragraph

Yes, we just wanted to point out that although the temperature-salinity profile is held constant the melt rate isn't necessarily constant due to the glacier's changing geometry. We will now write "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 any) affect the plume equations. "

Page 9 – line 29 "...is close to equilibrium state.." what do you mean with equilibrium?
Later you speak about stable state. Do you mean steady? I would argue that currently Greenland glaciers are definitely not in a steady condition.

The same issue was addressed by reviewer 1 (major comments) and we defined our definition on the present-state more clearly. This will be added in the revised manuscript.

Page 10 – line 5: "...each glacier 3.4..." something is missing between glacier and 3.4

We will delete 3.4.

Page 10 – line 6: "...glacier individually 3.3..." something is missing between individually and 3.3

We thank the reviewer for spotting the mistake we will insert the word "section".

Page 10 – line 6: Here it is not clear if you took the data from Calov et al. 2018 or if you actually run the model

This paper and Calov et al. (2018) are closely related. They originate from the same project and are written essentially by the same group of authors. Calov et al. (2018) describes the model of Greenland glacier system, experimental setup and results of several climate change experiments. In Calov et al. (section 5) we also described how we computed time-dependent subglacial discharge for individual Greenland glaciers using SICOPOLIS and MAR output, and the basal hydrology model HYDRO. In the current work, we used this time-dependent discharge as the forcing for modeling of 12 selected glaciers. We will clarify this issue in the revised manuscript.

Page 10 – line 22,24: this part about the interplay between melting, calving and bedrock is interesting. I would add few more details.

Agreed, we will add a broader discussion in the revised version.

Page 11 – line 6: a space is missing before "Enderlin"

Will insert space.

Page 11 – line 15: "model versions" do you mean the the spin-up ensemble?

Yes, we will now write.:"After obtaining the present-day state, we then ran the spin-up ensemble with all valid beta/fwd combination ..."

Page 11 – line 16: why not changing also the subglacial discharge? It is such an important forcing for the plume and comes from several approximations (fixed grounding line and closest neighboring approach).

We fully agree with the reviewer that in this study we explore only a fraction of uncertainty sources. In particular subglacial discharge as well as SMB also depends on the choice of the regional climate model and the global climate models which has been used to provide boundary conditions for the regional model. However, we believe that Fig. 15 already provides a very important inside into the major source of uncertainties in simulated glaciers contribution to SLR. Namely, it shows that the uncertainties in the choice of model parameters is likely to be the largest source of the SLR uncertainty. Thus to considerably narrow down these uncertainties, the glacier model parameters have to be better constrained.

Page 11 – line 17: at page 10 (line 8 to 10) is said that also the unforced model drift is calculated. Then this drift is removed by subtracting it from calculated values. This implies that a linear behaviour for glaciers is assumed. I think that this should be properly discussed.

Of course it is known that glaciers response to climate change is nonlinear and we do not assume such linearity. Our modeling approach is based on the assumption that glaciers were in equilibrium at the year 2000. However, to ensure that all glaciers are in the perfect equilibrium with the 2000 year forcing would be required to perform infinitive number of infinitively long spin-up experiments which is not possible even with fast model. This is why we apply as additional constrain, namely, we excluded all model realizations with positive (mass gain) trend and require that the simulated negative trend is significantly smaller than simulated SLR response to climate change scenario. Still, we have to tolerate non-negligible drift in the control runs - otherwise we will be left with to few accepted model realizations. This is why we decided to exclude such drift from the forced run which we believe is still better to bot do it. We will discuss this issue in more details in the revised manuscript.

Page 11 – line 25-27: as above, this implies linearity but glaciers are definitely not *linear systems. This issue is just slightly addressed at page 12 – line 4. Page 12 – line*

We did not assume that glaciers are linear systems. As we explained in the response to the first reviewer, the drift is rather small since we only accepted such model versions in which drift is smaller than simulated SLR in the forced experiments. Of course, zero drift in unforced experiment would be preferable, but this cannot be achieved with the finite computational resources. Therefore we are left with two to options: (i) to leave forced experiments as they are or (i) to exclude unforced drift from the forced experiments. Both options are imperfect but we prefer the second one.

21,22: you attribute the source of uncertainty to Beta, this comes from the fact that Beta is responsible for the imposed melt rate (through the tuning procedure). However Beta is just a model parameter, I think that avoiding the use of Beta (as suggested in he main comments) could also improve this part of the work, it will allow you to relate uncertainties to physical quantities.

As we explained above, there is no physical reasons why the linear plume model should produce correct results with beta=1. See also Beckmann et al. (2018).

Page 13 – line 18 to 20: something is wrong here, an entire sentence is repeated.

The repetition will be deleted.

Page 13 – line 33: same as above. Your results are not affected by CTD/reanalysys
temperature and salinity because the Beta tuning incorporates all the uncertainties.

We agree with the reviewer that colder temperatures in the reanalysis data set in some (but not all cases) can be balanced by a higher beta. We will provide the new table showing which beta values were used for CTD and reanalysis data set.

Page 13 – line 35: "...observational constraints on submarine melt..." as explained in the main comments I think that we should rely on melting formulation as less as possible dependent from a tuning on observations, especially for future projections.

We agree that this would be a nice idea but not at the present level of ice sheet-ocean interaction. As we showed in Beckmann et al 2018, Jenkin's linear plume model does not produce observed submarine melt and therefore should be corrected. Even with this correction believe that our approach is more physically based and therefore more trustworthy than those used in previous studies.

Page 14 – line 4: "and" repeated two times

We will delete.

Page 14 – line 7: "our" repeated two times

We will delete.

Figure 3(a): I think that using white dots is a bit unfortunate, also the red star is not very visible.

Agreed, we will improve Figure 3 for more visible CTD location.

Figure 11: "from" instead of "vom"

We will adapt accordingly.