Thank you for your review – please see below for our responses (blue text).

General comments

The authors present catchment-scale numerical ice sheet modelling simulations designed to make projections of potential future sea level rise contribution from Ryder Glacier. Unlike surrounding glaciers in northern Greenland, Ryder Glacier's ice tongue remains largely intact and has not shown retreat and/or collapse in recent decades, raising the quesion as to why that might be. The authors run a suite of experiments exploring the future of Ryder glacier under two SSP forcing scenarios alongide the sensitivity to calving and basal melt rates. They find the scenario dependence has a greater impact on sea level rise contribution by 2300 than the exact prescription of basal melt rates or an increase in calving rate through time. All of their simulations find the ice tongue to collapse in the future.

Targeted numerical modelling experiments have not been conducted - to my knowledge - on this glacier before, and so the research is timely and important. I think the modelling framework is good and the results are well presented. I do however have some comments to improve the manuscript, including more details on the initialisation and sensitivity to the choice of calving law. These more major comments are detailed in the numbered list below and specific line comments are in the following Section.

Thank you for your helpful review on our manuscript. We appreciate your time and have detailed our responses to your comments below.

1. Abstract: the entire abstract, except for the final sentence is background/method/aim. This needs revising. Normally abstracts would be weighted towards a couple of sentences of background and motivation, the same again for the aim and methods, and then most on the new findings and implications. It does not currently include your main findings, i.e. the range of potential sea level contribution by 2300 nor the scenario dependence stated in the title.

The abstract has been changed to remove some of the background/motivation, and to add in more information on the key results. The new abstract is shown below:

The Northern sector of the Greenland ice sheet contains some of the ice sheet's last remaining glaciers with floating ice tongues. One of these glaciers is Ryder Glacier, which has been relatively stable in recent decades in contrast to the neighbouring Petermann and C.H. Ostenfeld Glaciers. Understanding Ryder Glacier's future behaviour is important as ice-tongue loss could lead to acceleration and increased ice discharge. Meanwhile, it is unclear whether Greenland-wide modelling attempts are able to accurately resolve the influence of fjord/bedrock topography and small-scale variations in ice dynamics for a glacier like Ryder. To fill these gaps, we here conduct targeted high-resolution modelling to Ryder Glacier until the year 2300. We find that mass loss is dominated by discharge under a low emissions scenario all the way to 2300, leading to a sea level contribution of between 0.8 to 2 mm depending on the degree of ocean warming. Discharge also plays a key role under a high emissions scenario up until 2100, after which a strongly negative surface mass balance becomes the dominant driver of mass loss. This negative surface mass balance leads to much higher sea level rise contribution by 2300 of between 44 and 52 mm, with little sensitivity to the range of ocean warming scenarios used in this study.

2. Introduction: The aims at the end of the Introduction do not reflect the title of the paper,

the work of the paper. These are a bit confusing to the reader, because it suggests that you study all three glaciers and provide an answer for why these glaciers are behaving differently. While you can gain insights into that from the work you do here, you are only modelling Ryder Glacier. The aims at the end of Section 2 are a much better reflection of the aims/objectives that are actually addressed in this paper. See the following point, but make sure that the reader is easily able to find the main question/aim of the study in the final paragraph or so of the Introduction.

Thanks for this comment and related suggestions – similar issues were also raised by the other Reviewer. We have removed section 2 (Petermann, Ryder and C.H. Ostenfeld glaciers), replaced the study area figure with one focusing on Ryder glacier. Some material from this section has been incorporated into the introduction and discussion section 'Comparison with other Glacier-Fjord systems'.

3. Section 2: I think this section is unnecessary, and detracts from the main focus of the paper. It leads the reader to think that you will go on to model all three glaciers to make a thorough comparison. Again, as mentioned above, this Section additionally causes confusion because you have one set of aims at the Introduction and a second set at the end of this section. My recom- mendation is, you move some of the material in Section 2 into the Introduction, e.g. lines 95-115 to highlight the importance of studying Ryder Glacier, given how differently it has been behaving to the neighbouring glaciers. You could perhaps make some initial statements as to why that might be e.g. SMB and fjord conditions. Then the rest of the comparison should come in the Discussion, in Section 5.3. There is no need for the level of detail in this section, and a lot of this could be summarised in a few sentences with signposting to other studies.

Please see response to comment number 2.

4. Model relaxation and calving: I would have liked to see a bit more on how your model performs during the 50-year relaxation period, I assume that the trend must be very similar to observations to justify not having a control run from which to subtract from the perturbed simulations (as is often done, e.g. in the ISMIP experiments, to account for model drift). As part of this relaxation I think more detail on the choice of calving law and what values of σmax were testing to arrive at 200 kPa should be included in the Methods section, preferably with some sensitivity tests and additional figure. Is this calving law really able to replicate the observed changes in front position of Ryder Glacier over the last 50 years, which is characterised by a cycle of slow advance and then a large calving event before re-advance? Do you expect that calving style to continue into the future and does this calving law reflect that? I think all of these warrant some explanation if not in the methods, but as limitations in the Discussion. In addition, while you account for the impact of increased calving (IC) in some of your experiments, you do not explore the low-end member of no calving. I think there is some scope for including a control run, or some exploration of the sensitivity of your results to this choice of calving law and chosen value of σmax.

The entire methods/model description has been re-written, including a new subsection focusing solely on the model relaxation. Here, we have made the aims of our relaxation clearer, provided more information on how the calving calibration was conducted, and compared our relaxation results to observations in a more explicit manner. As part of this reshuffle, the calving law is introduced earlier in the methods section where we have justified its use with reference to several studies which have compared several calving laws (including running simulations into the future with various calving laws). Additional discussion of the choice of calving law has also been added into the Model Limitations section in the discussions.

We have run an additional simulation where we keep all forcings the same as the spin-up. However, we ran the spin-up to match historical mass loss as well as we could (and believe our mass loss trend of 0.9 Gt/yr compares well to the mass loss trend between 2000 and 2017 of 0.96 Gt/yr from Mouginot et al (2019)). This is in contrast to ISMIP6 where Goelzer et al state that '*In any case, representing the historical mass loss accurately was not a strong priority for our experimental set-up, where any background evolution is effectively removed by subtracting results of experiment ctrl_proj'*. In addition, Goelzer et al (2020) state that by subtracting the ctrl simulation SLR contribution from other results, committed sea level rise is not accounted for/included in the projections. One of the benefits of focusing on a single glacier is that we can spend the time matching our spin-up mass loss trends (as well as velocities/thicknesses etc) to observations and so run forward simulations where we don't have to neglect committed sea level rise. However, it is true that there is undoubtedly also some model drift in the results and that the Ctrl simulations provides valuable information on what the difference is between an SSP1-2.6 future and one where any climatic changes were to immediately cease. As such, we have added text relating our Ctrl experiments at several points throughout the results and discussion sections, as well as editing some figures some include this data. Overall, we find that c. 0.3 mm of SLR by 2300 is due to the combined effects of model drift and committed SLR.

We have also run simulations with no calving (one for low emissions, one for high emissions), edited figures 3 and 7 with these results, and added in some discussion of these results. In summary, no calving makes little difference to the high emissions simulations (retreat rates remain similar as the ice tongue/parts of the glacier just melt away from the negative SMB), but leads to an ice tongue which persists until 2300 under a low emissions scenario.

Specific comments

Line 1: how many glaciers with floating ice tongues still exist outside of northern Greenland? if none, change 'some of' to 'all'.

This really depends on whether one assumes Northern Greenland refers to the Northern sector of Greenland as defined by Mouginot et al (2019) and shown in Fig.1, or refers to e.g. the NE, NO, and NW sectors. We have changed the wording to make it clear that we just refer to the NO sector here, as we discuss this sector later in the text.

Line 5: suggest changing 'increased mass loss from discharge' to just 'increased ice discharge'

Changed

Line 33: State some references of studies that have presented these 'observational records'.

References have been added

Line 40: Throughout I would clearly state what you mean by 'stable' i.e. 'mass balance is close to zero' or 'limited change in front position'

Clarification on what we mean by stable has been added to the relevant sentences

Figure 1: See comment above about Section 2. I think this study figure should focus on Ryder

Glacier and there is no need to necessarily include the others. Also the inset currently covers half of Ryder Glacier's catchment. Possibly show the model domain/mesh in this figure and the flowline used in other figures. I think the colour bar should be labelled speed rather than velocity.

We have replaced figure 1 with a figure focusing on Ryder Glacier, with a panel where the entire model domain can be seen.

Line 116: Heading 'Numerical model' doesn't really make sense given that this section includes a large part on input data. Perhaps consider changing to Methods and then 3.2 could be 'Model-set up' or similar.

The entire modelling methods section has been re-worked, including changing the section heading to 'Methods' as well as making changes to several of the subsection headings

Line 120: Does the domain allow for advance during the relaxation?
It does, and we have added extra clarification in here

Line 136: State the resolution of the velocity dataset.
We have added in that the resolution of the dataset is 250m

Line 138: I am not totally convinced by this justification for using Budd friction law, is there any reason to suggest that because it works best for Petermann it would for Ryder? I think if not testing different sliding laws in this study, the potential impact of this choice needs to be in the Discussion, especially given that Akesson et al. 2021 show this can have a large impact on future projections at Petermann.
We chose a Budd law in part due it having been found to work well for Petermann, but also as this sliding law has been used for a recent study looking at the whole of Greenland (Choi et al., 2021). As such, we are then able to easily compare our results to the results of these studies. We agree that sliding choice can have an impact on results, but note that other recent studies found other factors to be more important. For example, Barnes and Gudmunsson (2022) found that model results show similar decade-to-century scale predictive power regardless of sliding law choice whilst Carr et al (2024) found sliding law choice to far less import than e.g. SMB. As this study operates on the timescales identified as robust by Barness and Gudmunsson, and focuses on factors such as SMB and ocean forcing, we believe the results to be valid without testing additional sliding laws. We have added extra discussion of this into the 'Model Limitations' section.

Line 145: Is it common to have a spatially uniform viscosity field over the grounded ice? Perhaps mention some other studies that have used this approach. It may also warrant a space in the limitations section of the Discussion.
This approach is relatively common – it is the approach taken in the aforementioned Åkesson et al (2021) and Choi et al (2021) studies, as well as additional studies such as Wilner et al (2023), Humbert et al (2023), and Åkesson et al (2022). We have added all of these references.

Lines 146-156: I think these lines would be better placed in a section 'Relaxation' because it is currently confusing to discuss this before introducing the calving parameterisation Eq. 2 and introducing the SMB forcing in Section 3.2.1.

We have reshuffled the entire methods section, including the creation of a 'Model Relaxation' subsection as suggested

Figure 2: If possible make panel d bigger so it's possible to see some of the detail in the change through time, although I appreciate the geometry is not changing much during this relaxation.

Panel d has been made bigger (stretched the y-axis)

Line 166: More details on these SSP forcing are needed. Which CMIP model was used? Are these an ensemble mean? Also do you apply these as anomalies ontop of the mean SMB field used during the relaxation?
We have specified that the SMB fields are the mean from CMIP6 and that there are independent of the historical SMB field (e.g. not applied as anomalies).

Line 176: What does a 'high level of subglacial discharge' equate to?
We have added in that a high level of subglacial discharge is set to equal 3.88 km3 yr-1 as defined by the Wiskandt et al (2023) study

Line 199: I'm not sure what you mean by 'Ryder Glacier's grounded calving front during the relax- ation', surely the calving front is floating during the relaxation? make this clearer to the reader.
Ryder Glacier currently has two calving fronts; a large floating front and a smaller, grounded front. We have edited the text to state this clearly in the introduction when we first mention Ryder, as well as changing the text in the methods so that readers are reminded of this.

Line 206: How was the value of σ_{max} varied throughout the simulation? linearly reduced each year?
Yes, it was linear and we have added this in

Line 228: when you say 'grounded fronts' do you mean when the ice fronts become grounded during the experiment when the ice tongue is lost? I think a better explanation of what you mean by grounded fronts throughout would be useful.

We have clarified that this applies to any grounded part of the terminus. In combination with the additional text clarifying that Ryder already has a grounded terminus, we hope that the situation(s) in which this is applied are clarified.

Figures 4 and 5: units for thickness change.

This has been added in both figures

Figure 6: Where is this frontal velocitiy taken from? Is this the most useful measure? I would have instead reccomended to show the change in velocity inland of the terminus, to show how changes at the front (thinning/calving) impact inland grounded ice flow.

We have changed this so that the purple lines now show velocity inland of the terminus. We take the mean velocity of all grounded ice areas within 500m of the grounding line, evaluated for each time step.

Figure 7: This Figure needs a bit of a rethink, the lines in panels a, b, i, j are impossible to see any trend in and are too small. I think the sign of the red line in b) is wrong if you are expressing discharge as a loss.

The main idea behind the line graphs was to show the overall trend of mass loss in different simulations, for example by highlighting how much mass loss accelerated after 2100 in the high emissions scenarios/ how much discharge increased once low emissions simulations reached a bedrock depression. Unfortunately, the visuals of these very large increases meant that the y axis had to cover a large range of values, making the lines sometimes hard to see. Although we had tried to ameliorate this issue by showing a zoom in of the lines on panel a, we can understand how the lines remained hard to distinguish. As such, we have removed all the line graphs from this figure and just kept the pie charts.

Section 4.3: was the integrated SMB calculated over the grounded area only? and how was discharge calculated? across the grounding line? or a defined flux gate?

The integrated SMB across the model domain at each timestep is available as an output from the model. In additional, the total mass loss at each timestep is available as an output. As such, the discharge can then be calculated as the difference between these two. We have added in information on how the discharge was calculated.

Line 286: I find 'discharge losses' to be a bit awkward to read, I suggest changing it throughout to 'increases or decreases in ice discharge'.

Changed

Line 298: Surely the fact that the ice tongue was lost quite quickly in the high-end simulations is one of the reasons for the submarine melt rates not having such an impact in these simulations. Perhaps worth mentioning.

After the ice tongue is lost, we still have submarine melt; it is just applied to the grounded front instead. As overall discharge still remains higher in the high emissions scenarios (and continues to grow with time), the lack of sensitivity to ocean scenario is more likely due to the fact that the SMB just becomes so negative that it dwarves discharge (see Fig. 7, where the high and increasing levels of discharge in the HE simulations can be seen in the pie charts). We have, however, added in some information on how the overall magnitude of submarine melt changes upon ice tongue loss.

Lines 302-315: All of this could be shortened and summarised in a few sentences. Also if making a comparison between your study and other Greenland wide results I suggest including the ISMIP6 results.
We have removed some content from this section to streamline the discussions, but also added in several sentences comparing our results to Goelzer et al (2020).

Line 321: See comment above, surely the reason there is less sensitivity to ocean forcing is because at some point there is no ice tongue left? Worth mentioning this.
See response above

Line 337: The impact of the melt-elevation feedback may not be straightforward. Consider adding a reference to Delhasse et al., 2024 - TC who showed in coupled simulations the positive melt-elevation feedback may be mitigated.
We have added in a sentence addressing this point, along with a reference to the Delhasse et al study

Line 380: 'total ice-tongue collapse' to me suggests instantaneous collapse, i.e. the timeframe of C.H. Ostenfeld ice tongue collapse, whereas as far as I understand in your experiments the removal of the ice tongue is gradual due to the calving law? in which case I'm not sure have replicated an entire/immediate tongue collapse and it would be worth discussing this.
This is true – we have changed the word 'collapse' to 'loss' to remove the connotation that the loss of the ice tongue was sudden

Line 392: see comment above, I suggest using 'increased ice discharge' instead of 'discharge losses' throughout.
Fixed.

Lines 426-432: These lines are an exact duplicate of Lines 377-382. I suggest a careful proof read of the entire Discussion making sure there is no duplication and that the information presented is as concise as possible.
We have removed the first instance of this duplication, thank you for noticing this.

Line 455-460: This section on topography is important, but I do wonder about mentioning that loss of the ice tongue doesn't appear to initiate a runaway retreat of the grounding line further inland.
True – we have added a sentence that makes it clear that topography/bathymetry can also stabilise the glacier (something we see in the simulations once a prograde slope is reached).

Section 5.4: I think the limitations on choosing a single sliding law and calving law need to be discussed in more depth here.
More discussion of the sliding law has been added in (see response further up to your initial comment on the sliding law). The 'model limitations' section already contains discussion of how calving characteristics may change in the future but we have now additionally included a line specifying that different calving laws would be beneficial. However, it is not possible within the computation time allocated to run all of the simulations with several different sliding and/or calving laws and we believe that choosing a calving law based off recent comprehensive comparisons is a valid decision.

Line 505: This sentence would benefit from rephrasing. State the pathway instead of using 'latter'. 'greatly reduced negative societal impact' reads awkwardly as well
This sentence has been rephrased.