Review of Holmes et al. 'Sea-level rise contribution from Ryder Glacier in Northern Greenland varies by an order of magnitude by 2300 depending on future emissions'

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.

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.

2. **Introduction:** The aims at the end of the Introduction do not reflect the title of the paper, nor 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.

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 recommendation 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.

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 $\sigma_{\max}$ 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 $\sigma_{\max}$.

Specific comments

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

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

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

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'

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.

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.

Line 120: Does the domain allow for advance during the relaxation?

Line 136: State the resolution of the velocity dataset.

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.

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.

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.

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.

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?

Line 176: What does a 'high level of subglacial discharge' equate to?

Line 199: I'm not sure what you mean by 'Ryder Glacier's grounded calving front during the relaxation', surely the calving front is floating during the relaxation? make this clearer to the reader.

Line 206: How was the value of $\sigma_{\max}$ varied throughout the simulation? linearly reduced each year?

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.

Figures 4 and 5: units for thickness change.

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.

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.

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?

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'.

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.

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.

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.

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.

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.

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

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.

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.

Section 5.4: I think the limitations on choosing a single sliding law and calving law need to be discussed in more depth here.

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