**Response to Commenter**

Firstly we would like to thank the community reviewers/commenters for freely offering their time to read through the manuscript and make their suggestions. An itemized set of responses to their comments is below, beginning with the more substantial points and technical points following.

*The major point of the paper is simple enough. Freshwater released in realistic locations, with realistic circulation and at realistic rates doesn't reduce deep water formation*.

It was very helpful to us to see what the reviewers consider the major point of our paper, since it's not what we intended. We would describe the major point of the paper as, "The salinity anomalies introduced over deepwater formation regions (and thereby potential for deepwater response) by freshwater released from realistic locations at realistic amounts differ depending on the location." Having become aware of this confusion and similar comments by the other reviewers, we've made sure this comes out more clearly in the revised manuscript.

*With respect to the title, it reads as methodology, not as a main finding.*

The title is a WYSIWYG (what you see is what you get) title which explicitly conveys the subject of study. An internal discussion has been had with regards to the title to see if we can convey the same information while broadening the audience and appeal, the result of this discussion to we have chosen to change the title to "Northern Hemisphere freshwater routing in eddy-permitting simulations of the last deglacial"

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*With respect to the abstract, we feel that it is much too long.*

We disagree and find that shortening the abstract any further would result in the loss of useful information.

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*There are many acronyms in this paper. As it is a short paper, we think these should be spelled out more to ease the readability.*

The authors note the only acronyms used in this paper that are not well established in the community are those used for the model runs or features of the model runs (e.g. MAK, GSL, GOM, FEN, CBS, OBS), and those for subjects more readily identifiable by their acronyms by those in the field (e.g. ICE-5G, GSM, MITGCM, PMIP, CMIP). Spelling out each acronym for each occurance would lead to unnecessary bloat of the paper and would thus decrease clarity.

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• *In the introduction, it is mentioned that there are "at least three common experimental design problems", but the authors only expand on two.*

We have clarified the text to reflect the three common experimental design problems better.

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• *It would be nice to have some information in the introduction for why the authors chose the sites they chose. Are these areas known to be the major outflows of freshwater during the glacial? Are there others that are not accounted for?*

The Mackenzie river, the Gulf of St. Lawrence, and the Mississippi/Gulf of Mexico are well established outlets for glacial runoff during the last glacial cycle, these were the most prevalent for liquid flux off the North American ice sheets (this has been clarified with a modified figure 1, including information from supplemental figure 1). The other main outlet, which we do not explore in the manuscript (but is discussed) but was addressed in Condron and Hill (2014) is Baffin Bay, which is primarily solid flux (i.e. Icebergs). This additional information has been emphasized in the introduction as motivation for the sites chosen. We invite the community reviewer to explore

Tarasov, Lev, and W. R. Peltier. "A calibrated deglacial drainage chronology for the North American continent: evidence of an Arctic trigger for the Younger Dryas." Quaternary Science Reviews 25.7-8 (2006): 659-688.

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• *It would also be nice for the introduction to talk more to why an AMOC collapse is thought to have occurred many times in the past. It is implied, but not clearly stated in the introduction. It is also not stated why we might be interested in AMOC collapse today, which may be obvious to the authors but would be worth stating.*

Given that the above would be standard knowledge for anyone in the paleoclimate field, such an addition would detract for most readers. However, to facilitate access for other readers, we have added a few more references addressing the above.

• *Another topic that is not mentioned is the bistability of the AMOC. There has been much work on the existence of "tipping-points" (e.g. most recently Lohmann & Ditlevsen, 2021, PNAS), whereby over a certain threshold of freshwater hosing the AMOC collapses, but underneath that threshold it does not. This is an important concept to include given that despite some significant freshening in your experiments there is little effect on the AMOC.*

We note that the simulations are not of sufficient duration to obtain a robust AMOC signal, and AMOC was provided only for context and comparison to Condron (2012). As such, discussion of bistability and AMOC are best left to other studies, one of which is upcoming, whose timespans can more readily and reliably distinguish these features.

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• *The ice extents in km2 are quite low given that the record minimum in 2017 was 14.3 million km2 .*

We point the community reviewer to the land-sea mask in Fig. 3, where all of the Canadian Arctic Archipelago is glaciated and sea level has also been reduced, thus reducing the area over which sea ice can form in the model relative to present day conditions.

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• *Line 170 should read ". . down to 200 m depth. . ." [This comes of too much use by scientists of "high" instead of "large"]*

Addressed.

*• Line 186. Direct transport from FEN across the GIN seas is not clear in the figure, the proportion of fresh water shown is very small.*

Unsure how to address that a still figure cannot readily convey motion as was observed by the authors from examining the model output. Hence why this motion was described in the manuscript. Indeed the proportion of freshwater that follows this path is quite small.

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*• Line 191 becomes clear looking at the figure but there has to be meridional transport to get from the Gulf of St. Lawrence to the Gulf Stream and it is curious to read "eastern . . North Atlantic"*

Comment noted, no action required.

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*• Lines 229-231. A curious statement. As though the meltwater is trying not to affect AMOC.*

Comment noted, no action required.

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*• It should also be noted that the authors did not complete a combined experiment where all sites received increased freshwater fluxes at the same time. This might have been sufficient to tip the AMOC into a collapsed state. At the very least, this should be discussed. At most, another simulation should be performed with all four release sites simultaneously releasing freshwater.*

This paper is not a direct investigation of AMOC, the timescales involved in our simulations are neither long enough to sufficiently equilibrate the deeper ocean layers nor to observe a robust AMOC response. The use as a study for AMOC (and other climate effects) instead is the subject of upcoming worth from the authors using a lower resolution model and longer simulations. We constrain our present freshwater injections to that within the realms of reality, a sustained 8dSv into the ocean at a singular outlet is not a realistic flux under any robust deglacial reconstruction if this is what the reviewer is referencing. If the reviewer is instead suggesting a reduced flux (such that it is bounded by a reconstruction like was used in the manuscript) but from all locations at once, this would indeed be interesting but more useful to explore the (non)linear effects of runoff from the outlets. While interesting this is more appropriately explored as a separate investigation where all combinations can be thoroughly examined. However, a related question raised by this comment is whether there is any reason to expect that for the freshwater flux indicated, are the salinity anomalies purely passive tracers or do they feedback on the near surface wind-driven circulation for the order decadal time-scale (ie much shorter than AMOC timescale)? If they are purely passive, then one would expect the routing response to be linear. Though the injection does slightly change near surface density and sea surface height, this is miniscule compared to monthly and yearly variations in windstress (perhaps not the case for the original Condron/Winsor experiment with factor 25 larger freshwater fluxes). Though still unlikely to be significant, a non-linear active response assessment would require fully coupled atmosphere-ocean modelling to properly assess, and therefore beyond the bounds of this study.

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*• We suggest a change in the colour scheme of figures from jet to something more colour-blind friendly.*

We have adjusted all figures using GMT's WYSIWYG scheme (a rainbow-esq colour scheme) to the same color-blind-friendly scheme used in Figure 2.

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*• Figure 1 is confusing and needs more details. We do not know how to interpret it.*

Given this figure has been highlighted as underutilized by another reviewer and the community reviewer requests more details we have added additional text to make use of this figure. As well we have provided some guiding text for those readers less familiar with paleoclimate proxies.

For additional resources and background on the concepts of interpreting δ18O time series/temperature reconstructions from ice cores, and relative sea level , we suggest reviewing:
Shennan, I., Long, A. J., & Horton, B. P. (Eds.). (2015). Handbook of sea-level research. John Wiley & Sons. For foundational information on relative sea level and
Cronin, T. M. (2009). Paleoclimates: understanding climate change past and present. Columbia University Press.
For all other paleoclimate resources (particularly for δ18O, its uses and limitations as a paleoclimate temperature proxy) as well as the latest IPCC assessment report. All of these have proven very helpful for developing a foundation in these subjects and will ought to be very useful for digesting other subject material which make ready use of paleoclimate proxies.