Response to Reviewer (RC2):

Firstly we would like to thank the anonymous reviewer for the time they have taken to read through the manuscript and make their suggestions. However we do have to refute several of their conclusions, specifically about the applicability of the models, details below. An itemised set of responses to their comments is below, beginning with the more substantial points and technical points following.

With respect to the technical issues the reviewer highlights on the numerical models used:

"*OGCM at 1/6° of resolution, which was already run more than 20 years ago, e.g. New et al. 2021 and the new standard for this kind of ocean-only model is rather around 1/20 to correctly solve the eddies, e.g. Hirschi et al. 2020 among many others).*"

While indeed OGCMs have been run at comparable resolutions in the past there are several flaws with the specific comparison the reviewer makes to both New et., al., and Hirschi et.al.

New et. al., 2001 used ⅓ of a degree vs. our ⅙ of a degree, and their setup used fewer vertical layers (20-30 configuration depending vs. 50 as used here). The New et. al., investigation also conducted regional simulations (19°S to 70°N) vs. our global scale simulations. Assuming New et. al.'s zonal range (which is unspecified) is 260E to 360E we find they have a total of 1,602,000 grid points in their 267x300x20 configuration (assuming 20 layers and a uniform ⅓ degree lat-lon grid) vs. the 78,030,000 of our 510x510x6x50 (the 510x510 cubed-sphere configuration). Not only does this represent a significant difference in run-time memory, output data storage, and process interconnect (i.e. message-passing-interface, MPI) traffic requirements from a purely technical perspective, we also have to use a smaller timestep to satisfy Courant–Friedrichs–Lewy (CFL) stability. In summary, this comparison is not accurate nor constructive.

Both New et. al., 2001 and Hirschi et. al., 2020 (the reviewer having specified only those two references we restrict our comparisons to these same references) are contemporary background climate modelling investigations. At present, to the best of these authors' knowledge, the manuscript presented uses one of the highest resolution glacial interval OGCM modelling simulations to-date. This configuration is surpassed only by the AWI-ESM model, as in Shi et. al., 2023, whose ocean model uses a finite-element approach and thus has some regions of higher resolution (depending upon the configuration used). There are significant difficulties in applying GCMs, let alone coupled Earth systems models, configured for present day or preindustrial climates to glacial time intervals as well as different scopes of work (chiefly, the longer millennial scale integration times required for paleoclimate investigations vs. that of centennial scale CMIP investigations) that the reviewer highlights, but then ignores within the context of the current manuscript.

" *Thus, we cannot really say that the tools used are really state of the art.*"

With respect to this statement in the context of the MITgcm, we argue that this has already been recently highlighted (Love et. al., 2021) and refuted in past work using this model configuration in https://doi.org/10.5194/cp-2021-15-AC1 as part of the discussion of Love et. al., 2021. The specific text is reproduced here for clarity and posterity.

"We point out the main focus of this work, the representation of surface transports and features generally regarded as subgrid scale, would not benefit from existing updates to the model as the features of interest are already adequately represented in the version we use. Updates to the MITgcm model appear to largely center around bug-fixes and documentation updates (https://github.com/MITgcm/MITgcm/releases) without substantial effect on the representation of surface transports and eddies. As well, with regards to increasing the resolution of ocean-only simulations, we do note there are some entries in Hirschi et al. (2020) (which for the benefit of those unfamiliar with the work, is a review paper examining the representation of AMOC under present-day conditions from multiple sub 1 degree resolution model simulations extracted from 23 different publications) which are higher resolution. However, only one is a global ocean-only simulation which is above our grid resolution (Moat et al. (2016) which used 1/12 degree). Thus, we contend that the model configuration used in this study is of comparable complexity and resolution to the multi-model ensemble of simulations presented in Hirschi et al (2020). We make this point in the revised submission."

With respect to COSMOS, the authors evaluated the most recent versions of the component models (tagged as MPIESM-P in PMIP4/CMIP6) as part of the preliminary work for this and other parallel investigations. The primary difference between these configurations, barring resolution which was a choice informed by multi-millennial scale simulation requirements and compute cost, is the updated version of the atmosphere component from ECHAM5 to ECHAM6. MPIOM was updated as well, however these updates were primarily 'quality of life' updates addressing I/O difficulties with the model. Having evaluated the additions (and associated computational expense equalling an increase of roughly a factor of 2) to ECHAM6 we found the trade-off in model expense to not be worthwhile for the goals of our investigations. We also point out that the reviewer is incorrect in classifying COSMOS as an AOGCM, as that neglects the land surface model JSBACH. While output of this model component was not included in this investigation (as it will be the subject of future work) we argue that describing COSMOS as an Earth Systems Model is more accurate.

"*Models with such coarse resolution are now still used since they can allow to perform long transient runs (multi-millennia) to analyse intrinsic AMOC variability during glacial time for instance, but here only simulations of 100 years or less are performed.*"

What the reviewer is unaware of, as it was not communicated in the paper since these are technical details (and in the authors' view outside the scope of the manuscript), is that the COSMOS simulations presented here form only a small subset of sensitivity experiments comprising >10,000 simulation years investigating the roles of glacial runoff and pCO2 in centennial to millennial scale climate variability. Each of the simulations presented have been integrated for several thousand years beyond what is analysed in the manuscript. However, the freshwater forcing fluxes used, which while comparable to the which we can infer from the proxy record and why they were used in Love et., al., 2021, are strong enough to suppress overturning in COSMOS and results in a stadial state. As such, and as discussed in the response to the other reviewer, the resulting climate changes from the stadial state prevent any useful comparison beyond the window presented in the manuscript.

"*Since this study aims at evaluating climate dynamics during paleo-periods, we can wonder if such short-time simulations are really relevant to improve our understanding of climate dynamics as compared to existing records.*"

We note that in-development work within our research group has found that the triggering of large scale (with respect to both spatial and magnitude) centennial to millennial scale climate variability (i.e., Dansgaard-Oeschger-like events) is sensitive to climate perturbations and internal variability on sub-annual timescales. As such, the scales presented here are of at least a temporal scale to be relevant for such investigations.

Abstract: It is quite long and not very clear. I agree that the experimental design used is quite complex and it took me time to understand it, but please try synthesize better what you've done and why (and leave details out of the abstract to focus on the main results).

We will attempt to clarify the abstract.

• *Line 68: you can consider to cite Swingedouw et al. (2022) to substantiate this claim (although this was using an eddy rich model).*

Assuming the statement in question is "This justification for hosing is … important to the transport and mixing of freshwater."

We will cite at least the requested manuscript.

• *Line 75: the link with climate change is not obvious given that the focus here is on glacial time and paleostudies. Please state this more clearly.*

We will add additional, specific, examples of the link between freshwater forcing methodologies (specifically studies reliant on Hosing) and climate change.

• *Line 88: unclear sentence? What was missing? Please be more explicit.*

The statement commented on is "Lohmann et al. (2020) provides a mix of eddy-parametrizing and eddy-permitting conditions through their use of an unstructured mesh approach with the FESOM model."
We are unsure what action the reviewer is requesting here, as subsequent sentences describe and compare the work of Lohmann et. al., 2020 within the context of other work discussed within the introduction. However, we will try to rephrase to increase clarity.

• *Line 108: "cannot" is too strong. The parametrizaiton aims at reproducing that. What you mean here is that they are not doing a good job at this, which I agree, but maybe with better parametrizaiton this can be solve*d.

We will modify the sentence to
"Given that eddy parametrizing models do not accurately simulate the pathway of coastally released meltwater, …"

• *Line 121-132: As far as I understand the use of MITgcm is only for finding the fingerprints that are then used in the coarse resolution OAGCM. This is not stated very clearly here, while the title of 2.1 states implies it but not that clearly. Please rephrase a bit to clarify this.*

We will rephrase the first paragraph of Section 2 while avoiding repeating the first sentence of Section 2.1 which reads "All of the fingerprint generation simulations are performed using the MITgcm …" as reviewer #1 has requested that we remove repetition.

• *Line 134-150: not much is said about the mean state of this MITgcm run. What is the strength of the AMOC, where are located the convection sites. Is the circulation realistic for glacial time?*

The MITgcm run(s) are described in Love et. al., 2021 and this is stated in the manuscript. Given we utilise the MITgcm over a short interval and extract only the salinity anomaly any discussion of the MITgcm's AMOC is of no utility to the reader. As for whether the circulation is realistic, the authors are unaware of any proxy data which can be used in a robust model-data comparison to evaluate the realism of surface ocean circulation during the last deglacial interval. Will will be providing plots showing surface circulation as requested elsewhere. We can at least state that the resulting distribution of glacial runoff for the Mackenzie River outlet compares favourably with previous results discussed in Condron and Windsor, 2012 within the limitations of comparing different experimental setups with the same model.

• Line 165: replace "is" by "might be", since the use of only an eddy-permitting model is really questioning the realism of the results.
We disagree and will not implement this change given the discussion above regarding the relative resolution of the MITgcm configuration presented here vs. the state-of-the-art for glacial ocean simulations as well as the 'proof-of-concept' nature of the fingerprint component of the investigation.

• Line 174: when are those 5 years selected (what time of the simulation).
The line will be modified to read

"... averaged over the last 5 years of each model simulation."

• Line 181: this claim is not supported by any figures I think, so a "not shown" is necessary here.
Will Implement

• Line 198: if there are millennial intrinsic oscillation in COSMOS, how did you change the period of the control simulation? Is the AMOC changing on the long term?

With respect to 'how did you change the period of the control simulation', we are unsure exactly what the reviewer is requesting here. When executing the model we simply specify the year from which the model components are restarted and integrate the model forward. With respect to 'Is the AMOC changing on the long term', there is no long-term secular drift in the AMOC present on multi-millennial scales in the control simulation. If the reviewer is inquiring as to how we selected the control interval we did: we chose the interval with the longest interstadial duration that was at least 1000 years past the change to the background conditions in the model setup.

• Line 210: since the focus here is on the freshwater release, the way this freshwater is accounted for in the model should be depicted. Is the model rigid lid or free surface? Which kind of parametrization? Is salt conserved with this parametrization?

The freshwater is injected into the ocean model via the same field by which liquid precipitation is passed from the atmospheric model via the coupler. The model uses a free-surface formulation. This information will be added to the manuscript.

• Line 213: still this question about intrinsic variability during glacial time?

We are unsure exactly what the reviewer is requesting here. The sentence in question is 'The control simulation used here has been integrated forward for roughly 1000 years under the current climate con-
ditions prior to initializing the injection simulations.'. This scale of model simulation time is typical in paleoclimate modelling investigations to allow for any adjustment in the model mean-state to the modified boundary conditions.

• Line 249: within those questions, there is an implicit assumption that the response of model to freshwater only depends on their pathway, which is totally false, as shown in Stouffer et al. (2006) for the same design of freshwater release, the model responses can vary by an order of magnitude in terms of AMOC!

The reviewer seems to be implying that in the stating of our research questions we are neglecting that the investigation is a modelling study and presenting results for those models which are used (and introduced in the preceding section).This is a critique that could be levelled against most climate model based studies to date. We will add an explicit caveat on model dependence to freshwater forcing especially with respect to climate response.

• Fig. 5: I do not find this analysis very enlightening: the curves are very messy and not much is said about the differences. Are they due to AMOC response? A correlation of the climate response this AMOC response (with dfferent lags) might be the least to be done. The residual might represent the effect of other processes than change in meridional heat transport due to the AMOC, including atmospheric noise, change in stratification, gye transport, etc.

We will attempt to incorporate some additional statistical analysis regarding the correlation and lag/lead of the presented climate metrics vs. AMOC. If the additional analysis is informative and provides useful results then they will be incorporated into the manuscript, if they are not then this finding will also be summarised. With regards to the lines being messy, the data already has a 10-year running mean applied to it as documented in the manuscript, and the level of variability presented is not atypical of other published modelling investigations using coupled climate models e.g. Klockmann et. al., 2018, Klockmann et. al., 2020, and Zhang et. al., 2021.

• Line 389: unclear sentence.

The sentence in question is “In spite of this result, the use of fingerprints results in a freshwater distribution more dis-similar to the eddy-permitting simulations than the regional injections for all injection sites considered.”

This sentence summarises a small set of comparisons presented in Section 6 of the supplemental materials. We will rephrase.

• Line 404: this is “not shown”, is it?

The site of the GIN seas deep convection is indeed not shown in any of the Figures at present but is mentioned in the model description on lines 207-208. Such figures have been requested elsewhere and so we will cross reference the appropriate figure here.

• Line 411; any correlation to support this claim?

Contemporary investigations such as Cosimo, 2002 and Shu et. la., 2012, have found similar correlations between surface air temperature and sea ice concentration at both poles. We will add additional citations and some physical reasoning to provide additional context to the highlighted statement.

• Line 417-419: unclear sentence.

The sentence in question reads “Comparing regional injections to their respective fingerprint counterparts, injecting freshwater at the outlets results in earlier, but not faster, SIA/SST/SSS changes in the GIN Seas region for the MAK and FEN regions relative to the fingerprint methods.” We will modify to read

“Comparing regional injections to their respective fingerprint counterparts (i.e. MAK-R to MAK-FP), …”

• *Line 435: replace “is” by “might be” since this is a hypothesis at this stage.*

We will modify the text to

“This is likely due to the stronger…”

• *Line 440: “first principles”. What do you mean? Please be more specific here.*

First principles here being: If you inject large quantities of freshwater directly over regions of deep water formation, or directly adjacent to such regions, you will of course reduce the strength of density driven convection. We will rephrase to “buoyancy considerations” for clarity.

• *Line 445-447: please rephrase, I do not get the reasoning here.*

We will rephrase to “This ordering is reversed (i.e., GSL sourced freshwater results in greater impacts than GOM) and results align …”

• *Line 450-451: Fig. S7 should show significant results only! (using a t-test of the difference for instance).*

We will provide plots of the standard deviation of the field in the control simulation over the averaging duration to provide a comparison of the anomalies relative to the variability in the field.

• *Line 465-475: since the site of convection are not shown (among many other things, e.g. barotropic streamfunction, etc.), I think that the analysis is in the end quite poor and not going much into depth of real understanding of the differences in response.*

As discussed elsewhere, we will add plots showing the sites of deep water convection and expand upon this subject in the manuscript.

• *Line 481-483: where is it shown?*

We assume the reviewer is referencing the following on lines 480-481 (as otherwise the reviewer’s comment does not make sense). “The differences are largest at the location of the source of glacial runoff”

We will add references to figures 1 & 2.

References

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