Response to Reviewer (RC1) :

Firstly we would like to thank the anonymous reviewer for the time they have taken to read through the manuscript and make their suggestions. An itemized set of responses to their comments is below presented in the same order as the reviewer has provided.

"*The authors should synthesize all parts of the manuscript to make it easier to read. There are a lot of repetition throughout. The introduction needs to be re-structured to provide a more logical flow. At the moment, it seems like the introduction includes a summary and a longer description. In addition, the results are a bit difficult to follow. There are a lot of figures (or at least a lot of information in all the figures), and they are not necessarily mentioned when describing the results.*"

We will re-evaluate the manuscript for the repetition and logical flow the reviewer comments on. We anticipate that addressing the specific comments both reviewers provide will be sufficient to address this point.

"*Please be more precise, particularly in the abstract and conclusions. For example, L. 28-35 and 488-498, the broad hosing is compared to the other methods in a very subjective way. The response is faster by how much? The climate response is greater by how much and when? …*"

We will add further details regarding features (e.g., such as the speed of the climate response) to the conclusions of the manuscript and evaluate the suitability of such statements within the abstract (i.e., where brevity is of value).

" *- It would be nice to show a figure in the supplement of the surface currents under LGM/YD conditions and how they differ with the pre-industrial run. This could be compared to a similar figure done with COSMOS.*"

We did not conduct pre-industrial control runs with either of the configurations in the manuscript. While we do see some utility for such a comparison, we no longer have the allocated compute resources with which to conduct such simulations. We will add plots of surface currents for both a MITgcm and COSMOS control simulation into the supplemental materials.

"*- Was salinity restoring used in the simulations performed with the MIT-GCM? If yes, how would that impact your results?*"

Salinity restoring was not used in any of these experiments. A line will be added to the experimental design section for clarity.

"*- I understand that if other modellers want to re-do similar experiments, then the injection rates as shown in Figure 1 are relevant, but I find the salinity anomalies much more useful and would suggest to show the top 30m salinity anomalies (top 100m for GOM?) as figure 1 instead.*"

We disagree as the reviewer has misunderstood the contents of Figure 1, which is not rates but rather the salinity injection distribution derived from the salinity anomaly from the MITgcm experiments. We will attempt to clarify the caption further to alleviate this potential source of confusion.

"*- I don't understand the rationale behind taking a vertical integral to produce the injection rates (L. 179). I can see that for the GOM the freshwater seems to be advected very quickly into the* *sub-surface as no anomalies seem to be visible at 30m but anomalies are visible at 100m… yet without any clear justification salinity in the top ~100m should be shown and used,"*

The rationale behind taking the vertical integral was to ensure that the salinity anomaly throughout the entirety of the water column was captured no matter the resulting vertical distribution (which, as the reviewer comments on, varies depending upon the injection location). Using only the top ~100m as suggested would be explicitly excluding some components of the salinity anomaly without benefit vs. the approach we've taken which incorporates all the salinity anomaly in the water column and then weights this value on a cell-by-cell basis by the (lateral) integral of the field.

We note that the reviewer, in commenting that '*no anomalies seem to be visible at 30m*' highlights that we need to add comments to some of our figures that the scale is clipped (e.g. Figure 1, where -0.25->0 shares the same colour as values >0).

"*- L157: it states that the simulations are run for 22 to 24 years, but on figure 1 the maximum years shown are 20."*

The longer simulations are those presented in the supplement where OBS configurations were used for FEN and GSL. The text will be updated to focus only on those simulations discussed in the main-text.

"*- It is stated that COSMOS exhibits centennial-scale climate variability under the boundary conditions chosen (L. 198-199). Isn't that a problem for your study? Will the internal variability of the model affect the response to the meltwater injection?"*

The centennial scale climate variations are O(500 years) in scale. The interval chosen reflected a multi-centennial warm interval (i.e. analogous to interstadial conditions) to mitigate the impacts of the internal climate variability on the goals of the investigation. As for this behaviour being problematic here: this configuration, and the internal variability exhibited, reflects our current understanding of the climate system during this time interval. In particular the study focuses on the role of glacial runoff (i.e. meltwater) during this interval. Using a model which exhibits a stable climate during an interval which is well understood to be bi-stable would not reflect reality and thus be of limited utility in this regard.

"*-It would be nice to show the main surface currents in the NA in the control state, maybe on top on of the subplot of figure 3. That would help explain the simulated salinity anomalies."*

As with the MITgcm figures discussed previously, the addition of vectors overtop a spatial field would reduce figure clarity significantly. However, given the resolution of COSMOS is much lower than that of the MITgcm configuration used we will investigate this and update the figures accordingly if figure clarity is not significantly impacted.

"*L. 323 and 325: It is stated that 1dSv hosing results in greater reduction than MAK or FEN, but I think that the 1dSv are actually quite close to the MAK and FEN results. Maybe you simply wanted to say 2dSv?"*

The reviewer missed that the text specifically references 'greater peak reduction' and 'maximum GRIP 2m temperature deviation'. With respect to AMOC, as shown in Fig. 4, the lowest AMOC value for 1dSv of Hosing is ~6.5Sv, while for MAK-R and FEN-R the lowest AMOC values are ~8Sv and

~7Sv respectively. While the values are comparable, the statement(s) in the manuscript are correct. However, we will clarify the language in these particular sections to address this potential source of reader confusion.

"*L. 328: Why compare after 10 years?*"

This interval was chosen as, generally speaking, this is when the changes were most interpretable. Once the AMOC has transitioned to a stadial state the 2m temperature anomaly variations reflect the climate transition to the stadial state and not the initial transient impact of the localised freshwater injection. We will add a note to this effect in the revisions.

"*L. 331 and elsewhere: It might be good to make a clearer distinction between the different locations of meltwater input and the time of interest. After 50 years, the MAK and FEN results seem similar to the broad NA hosing. However, the GSL and GOM are quite different until at yr 80-100.*

Does this however mean that if one does not mind an uncertainty of 100 years, then the hosing is fine?"

The statement from the manuscript is "Thus, the deficiencies introduced by Hosing invalidate its use in comparison to regional injection.". While the results for some fields are similar for the various outlets after some time (injection location depending), this work demonstrates that there is no reason to utilise hosing outside synthetic model benchmark comparisons and in doing so the role of glacial runoff/freshwater-forcing is overemphasised in the climate system, even after 100 years. The argument of 'wait 100 years before making observations' also ignores the role of hysteresis and tipping points in the climate system, where the rate of change or the magnitude of impacts are important (e.g., for the simulation of Dansgaard-Oeschger events). These discussion points will be included in the manuscript to address potentially similar queries.

"*5. The impact on the AMOC of the different hosing locations is probably dependent on the location of deep-water formation in the North Atlantic. As such, the locations of deep-water formation in the COSMOS inter-stadial state should be shown. In addition, the strength of the sub-tropical and sub-polar gyres could impact the advection of salinity anomalies the locations of deep-water formation. The caveat associated with the 2 points mentioned above should be discussed.*"

A brief discussion of this, as well as a supplemental figure showing the deep-water formation regions during the interstadial state, will be added to the manuscript.

"*L 429: this is a surprising result, that should be explained. Please also refer to a figure. The more effective salinity crossing is also not obvious from the figures.*"

We will review Section 3.1 in light of the changes already suggested by the reviewers and add additional discussion as required. The 'salinity crossing' was difficult to represent during the investigation, however we will try to add an updated figure to the supplement demonstrating this feature.

"*Throughout: Replace "eddy-parametrizing" with "coarse-resolution" to avoid confusion*"

We will not implement this change as doing so would have the net effect of reducing clarity and precision. 'Coarse-resolution' is a subjective description, which varies significantly depending upon the spatio-temporal scales a given researcher works within. While ‘eddy-paramerterizing’ (i.e., not eddy-permitting or eddying) is a definition tied to the scale of a physical value (i.e., the Rossby radius).

“*Throughout: Please avoid starting sentences with “As well, “*”

We will review the manuscript and reduce the usage of “As well” where possible.

“*Abstract: Please define “AMOC”*”

Will implement

“*L. 39-40: Climate modelling studies should be cited here*”

As the reviewer does not specify specific studies they wish to be included, and as one of the studies cited incorporates modelling elements as well as observational datasets, we will not add additional citations without utility. However, we will modify the following sentence to read:

“A key uncertainty in the understanding of the effect of freshwater injection into the ocean arises from the fact that most models used to examine the impacts are of insufficient resolution to resolve the eddy-scale features that affect transport and mixing (as detailed below).

“*L. 53: References are needed*”

We will cite, at least,

Dokken, T. M., K. H. Nisancioglu, C. Li, D. S. Battisti, and C. Kissel (2013), Dansgaard-Oeschger cycles: Interactions between ocean and sea ice intrinsic to the Nordic seas, Paleoceanography, 28, 491–502, doi:10.1002/palo.20042.

“*L. 56-57: Remove “for a recent temperature reconstruction*””

We will not implement this change. Removing the requested text would imply the Kindler et. al., 2014 was a primary citation for the GRIP record, and citing instead a primary reference for the GRIP ice core record would be of limited utility as the subsequent interpretation thereof is of more utility to the expected audience of this manuscript. However, we will rephrase to incorporate a relevant reference to the original GRIP dataset and clarify the distinction between it and Kindler et. al.

“*L. 105: “direct hosing” needs to be replaced by something more appropriate and precise.*”

Since we have already defined hosing on the previous page we will remove the superfluous word ‘direct’.

“154: Please provide the exact coordinates of the meltwater input locations”

Will implement

“216: ppmv”

We will update the manuscript to switch PPMV and PPBV to ppmv and ppbv as per the CP style guide.