Reviewer #3

In this study, the authors explore the relation of surface buoyancy forcing and the initiation of the intrinsic oscillations (or threshold) of the AMOC in their earth system model. For this purpose, they conduct ensembles of simulations varying climatic forcing and atmospheric noise in the model. The model reproduces the modern and the LGM AMOC reasonably well. Also it reproduces the occurrence of the intrinsic oscillation of the AMOC under mid-glacial boundary conditions. The sets of experiments with different magnitudes of noise show the effect of noise in increasing the window of opportunity to cause the AMOC variability. Lastly, the authors explore the role of integrated buoyancy forcing (M) in predicting the initiation of the AMOC variability. Particularly, they show that the threshold type behavior of AMOC occurs when "M" approaches to zero.

I'd like to thank the authors for their effort in running so many exciting simulations. Especially, I find the experiments with different magnitude of noise very exciting since it is technically challenging to do so in AOGCMs! Furthermore, the authors are investigating an important question, "What controls the condition of the sweet spot?/Why DO cycles occur frequently under mid-glacial periods", which is of highly interest to the readers of Climate of the Past. Therefore, I think these results should be published. However, while the presented figures are exciting, I feel that this study has lots of rooms for improvements in the writing part. For example, in the paper, "M" is introduced in a heuristic way, but the physical reasoning of why M can be a good index is not fully discussed and it is not also compared to existing literatures. Therefore I would recommend major revision. Below summarizes my criticism.

Best wishes,

Sam Sherriff-Tadano

We would like to thank the reviewer for the valuable comments on our paper, which will help to clarify some aspects of our presented research in a revised version of the manuscript.

General Comments

1. Why focus only over the North Atlantic?

The authors focuses on the role of buoyancy forcing over the North Atlantic building on their previous work (Ganopolski and Rahmstrof 2001). While I agree that the North Atlantic is a very important region, I'm aware that there are quite a few other studies claiming the importance of the buoyancy forcing or density over the Southern Ocean in controlling the glacial AMOC (Buizert and Schmitner 2015, Sun et al. 2020, Oka et al. 2021). Perhaps, for this particular model, the North Atlantic could be the most critical regions, but there are other modeling studies suggesting the importance of both North Atlantic and Southern Ocean. For example, Sun et al. (2020) showed the importance of density contrast between NADW and AABW, rather than the buoyancy flux itself, in controlling the glacial (LGM) AMOC. I think it would be reasonable to point out these previous studies and then explain why this study focuses only on the buoyancy flux over the North Atlantic.

We agree that the AMOC strength is controlled by numerous factors and we will expand the introduction to discuss this in some more detail, following also the suggestions by the reviewer. However, the main purpose of our paper is not to try to explain the strength of the AMOC, but to propose a measure to diagnose convective instability and the associated abrupt AMOC changes. We propose that the integrated buoyancy flux over the northern North Atlantic is a valid measure of that and consequently focus on the surface buoyancy flux over this region. Of course, the buoyancy flux over the northern North Atlantic and its dependence on boundary conditions does also depend on the AMOC state in general and its meridional heat transport in particular, which is at least partly controlled by processes acting outside of this particular region. We will make these points clearer in the revised paper.

2. Comparison with previous studies

I appreciate the authors effort in shorting the Introduction and Discussion, however I think the authors are missing important previous studies that tried to answer similar scientific question "What controls the condition of the sweet spot?/Why DO cycles occur frequently under mid-glacial periods". For instance, previous studies have pointed out the potential importance of Antarctic temperatures (Buizert and Schmittner, 2015; Kawamura et al., 2017), Arctic sea ice (Loving and Vallis, 2005) or changes in surface winds by glacial ice sheets (Sherriff-Tadano et al., 2021a) in initiating the DO-like climate variability frequently during the mid-glacial period. None of the above mentioned studies have explored the role of integrated buoyancy flux over the North Atlantic, but I feel it is beneficial to describe these studies so that the readers can learn some of the history of this research topic.

As suggested also by the other reviewers we will expand the Introduction and Discussion sections by providing a bit more background information and extending the comparison with previous studies.

3. Why is it better to integrate the buoyancy flux over the entire northern North Atlantic?

Here, I'm concerned about the role of sea ice as some of the other reviewers. Previous studies showed the importance of sea ice transport through the Denmark Strait and its melting over the NADW formation region in weakening the AMOC (Born et al. 2010, Vettoretti and Peltier 2018). However, when the buoyancy flux is integrated over the entire region, the spatial heterogeneity in the sea ice-related freshwater flux will be removed. Under this condition, it is not straightforward why M can be a good predictor for the initiation of sweet spot/threshold. Perhaps in this model, I speculate that following two points could be important; 1. Sea ice forms and melts at the same region, hence the sea ice-related freshwater flux is not so important in the first place, or 2. The regional contrast in salinity induced by sea ice formation and melting is removed by advection of salt by oceanic currents. Please discuss why it is better to integrate the buoyancy flux over the northern North Atlantic, rather than focusing over the NADW formation region.

Calculating buoyancy as in Klockmann et al. (2018) over the deep water formation area(s) is of limited use because this flux will be strongly negative until deep water formation continues. This is because most of the heat is released in this area, but only a small fraction of the freshwater flux enters this area through the surface. Therefore, this flux does not provide information about the stability of the AMOC. On the contrary, the buoyancy flux integrated over the entire ocean domain north of 55N, as shown in our paper, provides useful information about the (convective) stability of the AMOC and explains its instability under glacial conditions. Of course, the accuracy of this metric depends on several assumptions, in particular that the southward export of freshwater in the upper ocean through 55N is small compared to the total freshwater balance of the Arctic and North Atlantic north of 55N. The effect of sea ice formation and melt is explicitly included in the computation of the surface buoyancy flux. However, as long as sea ice is formed and melted inside the area of integration of *M* (north of 55°N), the net contribution of sea ice to the integrated buoyancy flux will be small and only due to the non-linear equation of state if sea ice is formed and melted in regions which differ in their sea surface temperature.

4. Bit more discussion on the role of noise?

Fig. 8d and f reminded me of different characteristics of intrinsic oscillations obtained from CESM(Vettoretti et al. 2022)/MIROC(Kuniyoshi et al. 2022) and MPI (Klockmann et al. 2018). This could be very speculative, but if the authors agree, it might be interesting to point out the potential role of noise in causing different shapes of AMOC variability among models.

This is actually a good point, thanks. We will include a sentence mentioning the possible role of noise in explaining the different AMOC response in different models.

Specific Comments

L51: Please describe the climate sensitivity of the model here since it is one of the fundamental metric.

The climate sensitivity of the model is ~3.1°C. We will add this information to the revised paper.

L96-97: Would be suitable to cite Eisenman et al. (2009) and Sherriff-Tadano et al. (2021b) since they discuss the role of changes in atmospheric freshwater flux by ice sheets in intensifying the AMOC.

We will cite these papers in relation to the effect of ice sheets on atmospheric moisture transport.

L209-215: Fig. 10a and b show a threshold type behaviour of AMOC around 160ppm of CO_2 even when the value of M is negative. Is this related to the miss-choice of φ_M? If so, it would be worth discussing it here.

The oscillations for interglacial ice sheets and CO2 around 160 ppm are not reflected in the buoyancy flux because they involve changes in convection pattern that are mostly confined to latitudes south of 55°N, which is therefore not reflected in M. We will discuss this in the revised manuscript, following also the suggestion from Reviewer #1.