This paper presents simulations of the stratification and overturning circulation in the LGM. The main point is that the AMOC is relatively shallow, despite the stronger tidal dissipation of the LGM, because the stratification also matters. A strong stratification prevents the AMOC from being deep. The strong tidal dissipation does create a stronger production of AABW relative to the present-day. The paper nicely combines simulations of tides, OGCM simulations of the general circulation, overturning, and stratification, and discussions of the literature on LGM conditions, e.g., Adkins et al. 2002. I think that the paper is a nice contribution and should eventually be published. I say this as someone who is a contributor to some of the tidal literature cited here, but who is not at all an expert on simulating the overturning circulation. I hope that some of the other reviewers will be familiar with the latter topic. Below I list a few major points that I think should be improved, as well as some specific points.

We thank the reviewer for their positive and constructive feedback on our manuscript. Here are our point-by-point responses:

Major point 1. First, there are a lot of feedbacks between stratification, ocean tides, and the OGCM, associated with equation (3). I think that both your procedure and the feedbacks could be better described. Equation (3) contains a factor of omega (tidal frequency). So, which frequency did you use? Probably the M_2 frequency, I'm guessing. Please state what you did. Similarly, the formula contains a factor of N^2, the very stratification that you are (presumably) getting from your OGCM simulations, which are affected by the tidal dissipation. And the tidal dissipation in turn is affected by your assumption of N^2. So there are lots of sensitivities here! Again, I think you should describe your procedure and what you did to address these sensitivities.

Authors' Response: Thank you for your valuable suggestions. Indeed, our description of the forward tidal model was not sufficiently detailed. Yes, we used the frequency of the M2 tide equation (3).
Furthermore, as you rightly pointed out, there is indeed an interaction between N^2 in the tidal model and the OGCM: we input the N^2 obtained from the OGCM into the tidal model, and the resulting tidal dissipation in turn affects the N^2 in the OGCM.
To mitigate these effects and sensitivities, we used an iterative process as follows: Taking the LGM simulations as examples, we first input the N^2 obtained from the LGM case (no tidal mixing) into the tidal model, then input the resulting tidal dissipation back into the OGCM, obtaining the experimental result LGM_tidal1. Next, we input the N^2 from LGM_tidal1 back into the tidal model to obtain a new tidal dissipation, and run the OGCM again to obtain LGM_tidal2. The LGM_tidal shown in our manuscript is actually LGM_tidal2.
Following figure illustrates the changes in depth-averaged vertical N^2 during the iterations for both the PD and LGM. It can be seen that during the simulation of LGM, the change from LGM to LGM_tidal1 primarily involves a decrease in N^2 in the Arctic. From LGM_tidal1 to LGM_tidal2, there is almost no change. For the PD simulations, there were no significant changes in N2 throughout. Thus, we have nearly eliminated the mutual influences between N^2 in the tidal model and the OGCM through one iteration.

We acknowledge the need to enhance the description of the forward tidal model in the revised manuscript and will incorporate Figure 1 into the supplementary materials.

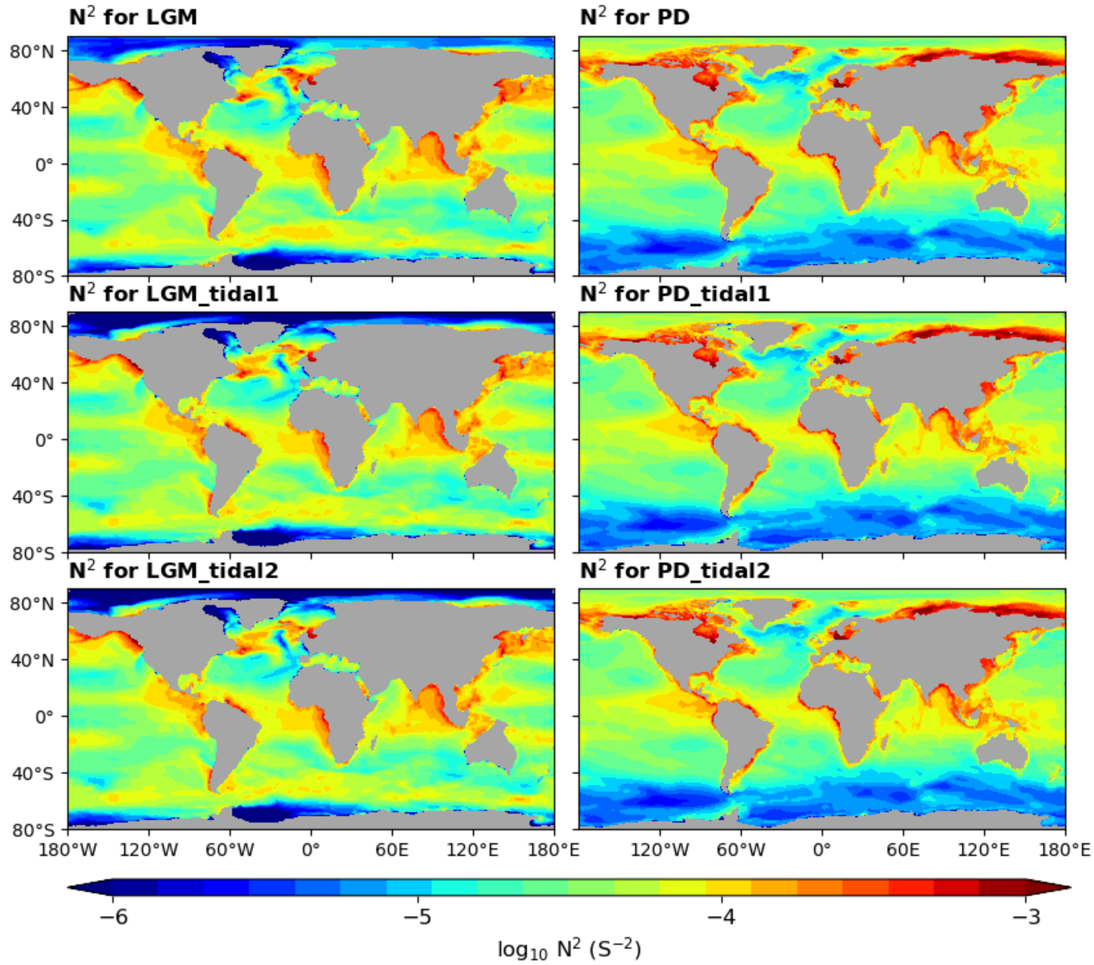

Figure 1. Changes in depth-averaged vertical N² Across Iterations for LGM and PD Simulations.

Major point 2. As far as I can tell, the atmospheric forcing employed here is not described at all. Surely, the wind and buoyancy forcing must matter? Otherwise the authors would be saying that one need change only the bathymetry and tidal forcing to get this dramatically different ocean, which would seem surprising, at least to me. At any rate, it would be very useful to describe the atmospheric forcing, which is always a critical factor in ocean modelling.

Authors' Response: Thank you for your feedback. As you rightly pointed out, atmospheric forcing is indeed crucial in ocean modeling. In our study, without considering tidal mixing, different atmospheric forcings have already resulted in distinct ocean between the present day (PD case) and the LGM (LGM case).

For the PD cases, we employed atmospheric forcing derived from the Reanalysis dataset (JRA55-do 1.4.0) spanning the period from 1958 to 2020. For the LGM cases, atmospheric results from a coupled climate model specifically tailored for the LGM (Zhang et al., 2013) were utilized to drive the ocean model.

Furthermore, the study by Knorr et al. (2021) highlights the variations in the temperature and salinity distributions of the ocean as simulated by different climate models for the LGM period. Not all models successfully capture the crucial characteristics of the glacial stratified ocean during the LGM. This underscores the critical importance of choosing an appropriate atmospheric forcing that can accurately reproduce the oceanic conditions during the LGM. Previous studies (Schmittner et al., 2015; Wilmes et al., 2019) have identified the phenomenon of enhanced tidal dissipation during the LGM contributing to a strong and deep AMOC, attributed to their failure to reproduce the high abyssal salinity and enhanced stratification in the LGM Atlantic.
In light of these findings, we will enhance our manuscript by including a more detailed description of the atmospheric forcing employed and further emphasize its crucial role in our ocean modeling.

Major point 3. As noted below, there are some obvious places (in my opinion, at least) where references should be added. Also, I found some errors in the referencing (such as citing a different paper led by Harper Simmons than the one you intended) even with a fairly casual check of papers that I know very well. So this makes me wonder if the referencing might have some other similar errors. Please should check your references over more carefully to ensure that everything is accurately cited.

Authors' Response: Thank you for your detailed feedback and for highlighting the issues with the references in my manuscript. I apologize for the oversight regarding the errors and omissions in the referencing. I have conducted a comprehensive review of all references cited in the manuscript. I will also add the necessary references in the sections you've pointed out as lacking.

Specific points

Line 41—I believe that this is the first mention of the enhanced LGM tidal dissipation in the main body of the text. This would be a good place to mention that this finding of enhanced LGM tidal dissipation has been found by many authors, beginning with the Egbert et al. 2004 paper that you cite elsewhere, and continuing in other papers (the Griffiths and Peltier papers, the Green 2010 paper, and others, many of which are already in your reference list). On line 199, you could state that your own results of enhanced LGM tidal dissipation are consistent with results from these earlier studies, and cite them again. How exactly you do it is up to you but you should cite this earlier work on this critical point.

Authors' Response: Thank you for your suggestion. I agree that it's important to reference the earlier work on enhanced LGM tidal dissipation when it's first mentioned and to cite these papers again when discussing our own results. I will make sure to include these references accordingly in the revised manuscript.

Line 49—Arbic et al. 2004a reference should actually be Arbic et al. 2004b.

Authors' Response: Thank you for pointing out the correction regarding the reference. I'll make sure to update it to "Arbic et al. 2004b" in the manuscript.

Line 62—suggest removing "Actually" at the front of the sentence (unnecessary)

Authors' Response: Thank you for the suggestion. I'll remove "Actually" from the beginning of the sentence as it's unnecessary, as you mentioned.

Line 72—I suggest describing zeta_EQ (the astronomical potential) first, and then describing alpha as a factor that corrects for the astronomical body tides (cite Hendershott 1972, which can also be cited for zeta_SAL)
Hendershott, M.C., 1972. The effects of solid earth deformation on global ocean tides. Geophys. J. R. Astron. Soc. 29, 389–402. https://doi.org/10.1111/j.1365- 246X.1972.tb06167.x

Authors' Response: Thank you for the suggestion. I will revise the description of zeta_EQ (the astronomical potential) first, followed by the description of alpha as a correction factor for astronomical body tides. Additionally, I'll cite Hendershott (1972) for both zeta_SAL and the correction factor alpha.

Line 83—"The last 20 days are used for harmonic analysis". You also need to tell us how many days you ran for.
Authors' Response: Thank you for your comment. Upon further inspection, the tidal model was indeed run for a total of 30 days, with the last 20 days being used for harmonic analysis. We will correct this error in the revised manuscript.

Line 84— "Is" should not be capitalized as it is in the middle of a sentence
Authors' Response: Thank you for catching that. I'll make sure to lowercase "Is" as it's in the middle of a sentence, as you pointed out.

Lines 85 and 86—please define what "node number" and "cell number" mean
Authors' Response: Thank you for the clarification. "Node" refers to the vertices of the unstructured triangular mesh, while "cell" refers to the triangles formed by connecting these nodes. I'll make sure to include this explanation in the manuscript.

Equation (5): the error is probably calculated over a tidal cycle; this should be stated. Also, why is there a "2" in the denominator; this is not usually present. Unless you are accounting for the factor of 1⁄2 in the time-average of a squared cosine function. The latter would mean that you are using harmonically analyzed amplitudes in equation (5) rather than instantaneous values; in which case you should say that. Bottom line, you could make this a bit more clear.
Authors' Response: Thank you for your feedback. We acknowledge that the explanation in our manuscript was not sufficiently clear. Here, we are indeed using harmonically analyzed amplitudes (complex notation sinusoids) to evaluate the elevations, which accounts for the "2" in the denominator. We will clarify this methodology and provide a more detailed explanation in the revised manuscript.

Table 1: the errors are reasonable. It would be good to compare them to errors attained by other forward tide models in the literature.

Authors' Response: We agree that it would be beneficial to compare the errors attained by our model to those of other forward tide models in the literature. We will include following comparison table in the revised manuscript. Thanks!

Table. Comparison of M2 RMS error in Forward Tide Models

Model	Deep Water (cm)	Shallow Water (cm)	Global (cm)
Our Tidal Model	4.87	15.14	6.54
Egbert et al. (2004)	< 5	NA	NA
Arbic et al. (2004)	7.26	NA	NA
Griffiths and Peltier (2009)	NA	NA	13.6
Wilmes and Green (2014)	3.86	NA	6.67
Schindelegger et al. (2018)	4.4	14.6	NA

*Note: Differences in the observed models selected across different studies may influence the results.

Line 117: there are two nice papers by Harper Simmons in 2004, and you are citing the wrong one. The paper that followed the parameterization of Jayne and St. Laurent 2001 is Simmons, Jayne, St. Laurent, Weaver 2004, Ocean Modelling, https://doi.org./10.1016/S1463-5003(03)00011-8. So you should replace the Simmons, Hallberg, Arbic 2004 citation in your references with the above reference.

Authors' Response: Thank you for bringing this to our attention. We will replace the citation for Simmons et al. (2004a) with the correct reference Simmons et al. (2004b), in the revised manuscript.

Line 142—you apply five cycles. Five cycles of what? Please explain.

Authors' Response: Thank you for your inquiry. For the PD cases, our surface (atmospheric) forcing is derived from the Reanalysis dataset (JRA55-do 1.4.0), covering the period from 1958 to 2020. We repeatedly drove each PD case with data from this time span five times to achieve simulation stability. We will include this description in the revised manuscript.

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Arbic, B. K., Garner, S. T., Hallberg, R. W., and Simmons, H. L.: The accuracy of surface elevations in forward global barotropic and baroclinic tide models, Deep Sea Research Part II: Topical Studies in Oceanography, 51, 3069-101, 10.1016/j.dsr2.2004.09.014, 2004.

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