

Review of “Shoaled glacial AMOC despite vigorous tidal Dissipation: Vertical Stratification matters” by Chen et al.

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.

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 ω (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.

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 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.

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.

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

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

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>

Line 83—“The last 20 days are used for harmonic analysis”. You also need to tell us how many days you ran for.

Line 84—“Is” should not be capitalized as it is in the middle of a sentence

Lines 85 and 86—please define what “node number” and “cell number” mean

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 $\frac{1}{2}$ 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.

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

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\(03\)0011-8](https://doi.org/10.1016/S1463-5003(03)0011-8). So you should replace the Simmons, Hallberg, Arbic 2004 citation in your references with the above reference.

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