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Comment on egusphere-2022-151

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Community comment on "The modelled climatic response to the 18.6-year lunar nodal cycle and its role in decadal temperature trends" by Manoj Joshi et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-151-CC5, 2022

Review of "The modelled climatic response to the 18.6-year lunar nodal cycle and its role in decadal temperature trends" by Manoj Joshi et al.

The paper presents a brief report on the climatic response of a low-resolution OAGCM to the variation in tidal mixing caused by the 18.6 year lunar nodal cycle. A spatial and time varying field of modified ocean diffusion is employed to represent the variation in tidal mixing, including two variants to reflect uncertainty in the vertical. This resulting experiment is simple, but sufficient to draw conclusions that warrant further study.

My impression is that the presented paper does the minimum necessary to draw attention to the potential importance of the 18.6 year lunar nodal cycle in the context of climate projections and hiatus/surge events. The authors propose that parameterisation of the lunar nodal cycle should be implemented in 1D integrated assessment models and decadal-scale forecast systems, and I am inclined to agree.

I have some concerns that I would like to see addressed prior to publication.

Major comments:

The authors create a map of ocean diffusion amplitude modulation based on the geographical distribution of the RMS current velocity and the nodal amplitudes. However, these are the barotropic tides. Around 2/3 of the power input to surface tides is lost in the shallow seas, whilst the remaining 1/3 generate internal tides (see e.g. Ferrari and Wunsch, 2009; de Lavergne, 2019). I believe it is the latter which the authors intended to parameterise in the model, and I therefore have concerns about the spatial distribution given in Figure 1.

The geographical distribution of internal tidal energy dissipation is strongly influenced by bathymetry. The map of tidal dissipation produced by de Lavergne et al. (2018, 2019) clearly shows the influence of bathymetry. This prompts two questions:

- Why did the authors not use such a map in their parameterisation?
- How would the results differ if the dissipation used this sort of geographical distribution?

Such a change in the geographical distribution would likely affect many of the regional results, but it is harder to gauge the impact on the global quantities such as surface temperature and ocean heat uptake.

I believe the importance of the result in the context of the recent hiatus in global temperature and ocean heat uptake is overstated. Hedemann et al. 2017 (cited on line 150) define an ocean surface layer that is 100m thick. Fluxes of heat into the ocean are given as fluxes through 100m, not the ocean surface, and are consequently much smaller. Estimates of increased ocean heat uptake (through the ocean surface) during the 2000s are typically 0.7 +/- 0.3 W m^{-2} (Drijfhout et al. 2014). The average flux you report (~0.07 +/- 0.07 W m^{-2}) is therefore sufficient to explain one tenth of the hiatus.

Minor comments:

Line 35: miss-spelt Yndestad.

Line 89: remove "opposites" given in parenthesis to improve readability. They are unnecessary due to the last sentence in the paragraph.

Line 98 and onwards: refers to "global mean surface temperature Tg", whilst the plot titles in Figure 4 refer to "Tsurf". It is ambiguous what "surface temperature" refers to. In the preceding paragraph I was (I think rightly) taking this to be the "sea surface temperature" (SST). However, I think this and subsequent references might be to "surface air temperature" (SAT; due e.g. to the presence of contours over land in figures 5 and 6). Please clarify throughout.

Line 102: please supply "(vol/sol refs here)".

Line 104: relating to my earlier comment, it is important to determine whether the quantity presented in Figure 4 is SST or SAT. If SAT then the contribution from the land will likely dominate the variability. If SST, does the variability arise from the summer months? In either case, I think a caveat drawing the reader's attention to the simple ice representation in FORTE2 would be advisable.

Line 110: remove 'though'

Line 111: Is the inconsistency in the Nordic Seas caused/dominated by variation in the ice cover, rather than the lunar tidal variation in the experiment?

Line 120: Missing close ")"

Line 125: switch order of the last two sentences in this paragraph.

Line 144: insert "a" > "…less of a global…"

Check references: missing Blaker et al. (2020)

Line 267/8: two mentions of "380 years" which seems to contradict the 760 years mentioned on line 80.

Line 279: duplicate "in in"

References:

de Lavergne, C., Falahat, S., Madec, G., Roquet, F., Nycander, J., Vic, C. (2019), Toward global maps of internal tide energy sinks. *Ocean Modelling*, 137, 52-75. doi:10.1016/j.ocemod.2019.03.010.

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Drijfhout, S. S., A. T. Blaker, S. A. Josey, A. J. G. Nurser, B. Sinha, and M. A. Balmaseda (2014), Surface warming hiatus caused by increased heat uptake across multiple ocean basins, Geophys. Res. Lett., 41, 7868–7874, doi:10.1002/2014GL061456.

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