Response to reviewers (reviewers' comments in blue)

Review RC2:

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.

We thank the review for the above assessment.

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.

Our parameterisation of the 18.6-year lunar nodal cycle requires spatial fields for the eight largest tidal constituents. This is because the nodal amplitude is different for each tidal constituent (see table 1 in the revised manuscript).

Note that S_2 and P_1 are pure solar tides so are not directly modulated by the nodal cycle and that M_2 and N_2 are out of phase with the other constituents. So, although M_2, K_1 and S_2 are the most energetic tidal constituents globally. K_1, O_1 and M_f are the constituents with the largest potential (i.e. nodal amplitude times typical magnitude) for 18.6-year modulation of tidally-driven vertical diffusivity.

Although we agree that much of the tidal forcing of deep ocean diffusivity is through the internal tide field, global maps of internal tide variability and dissipation are not available for all the key constituents required to do this sensitivity study. The global maps of internal tide generation and dissipation presented by de Lavergne et al. (2019; data doi: 10.17882/58105) only include the M_2, S_2, and K_1 constituents, plus an extrapolated 'all constituents' field. Similarly, the global maps of tidal mixing used by de Lavergne et al. (2020; data doi: 10.17882/73082) only contains constituent-integrated values.

To arrive at an appropriate spatial field to apply the 18.6-year modulation of tidally-driven vertical diffusivity, it is essential to use a consistent model for all the constituents. In the absence of a multiple-constituent global internal tide model, and given the relatively course 2° horizontal resolution of the ocean in our climate model, we rely on a barotropic tide model with the reasonable assumption that all tidal energy is dissipated locally. However, we acknowledge that some tidal energy does travel further than 2° through the internal tide field. Future work will use the global distribution of baroclinic tidal variability when such maps become available.

Figure 1 is the spatial distribution of the <u>18.6-year modulation</u> of tidally-driven diffusivity that is applied to a conventionally horizontally uniform and temporarily constant vertical diffusivity. Thus, it cannot be compared with maps of direct tidal mixing.

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.

This is a very good observation by the reviewer. Accordingly we have replaced the reference and changed the text to the following:

While the uncertainty in the value is clearly large, its magnitude suggests that it cannot be discounted as a significant driver of multidecadal variability of global temperature, given that, for example, the additional heat uptake into the oceans through the surface during hiatus-type periods is approximately 0.7 Wm^{-2} (Drijfhout et al. 2014).

Figure 4 suggests that the contribution of the lunar nodal cycle should be a global cooling of 0.03C-0.06C C over the period 2020-2029, and a warming of 0.03-0.06 C the period 2030-2039.

See also our reply to review RC4 (1st major comment).

Minor comments:

Line 35: miss-spelt Yndestad.

We have corrected this error.

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

Parentheses have been removed.

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.

We have removed all references to T_g, and replaced with T_{surf}, as this is meant to refer to surface temperature (whether over land or ocean), and not SAT.

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

This has been done with a reference to Gray et al.(2013) (also see response to Reviewer CC1).

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.

The quantity is surface temperature. FORTE2 does not have any model layers at 1-10m above the surface. We have added the following text as a caveat in the Discussion section:

A caveat in interpreting these results is that the sea-ice representation of FORTE2 is simplified, consisting of one slab (Blaker et al. 2021). Future work regarding the nodal cycle in the Arctic should be carried out with a more realistic sea ice model.

Line 110: remove ‘though’

The word has been removed.

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?

The response is statistically significant in the Nordic Seas (see Figure 5), so is a response of the ice cover to the forcing. The caveat introduced into the discussion regarding the sea ice scheme (see above) should also hold for this result.

Line 120: Missing close “)”

Parenthesis closed

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

The order has been switched.

Line 144: insert “a” > “…less of a global…”

“a” has been added.

Check references: missing Blaker et al. (2020)

The reference has been corrected to Blaker et al. (2021) and added.

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

We have removed the wording. In addition we have corrected Figure 4 to note it is as Figure 3, not 2.

Line 279: duplicate “in in”

This has been removed.