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Discussions

Interactive comment on "The nodal dependence of long-period ocean tides in the Drake Passage" *by* Philip L. Woodworth and Angela Hibbert

Philip L. Woodworth and Angela Hibbert

plw@noc.ac.uk

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We are grateful for the time that both reviewers spent on this paper. The comments of Reviewer 2 are given below followed by our replies. The page and line numbers refer to the version in OS Discussions.

1. P1 31: 't=0,' =>'t=0. Eq[1] is further modified to;'?

Thanks but we think it reads ok as it is.

2. P6 6-8: Are the daily mean values suitable for resolving Mt (9.13 days)? Using daily means might be one of reasons for large error bars in Fig7. Though there are some discussions on the complications of this analysis approach, it will be better if authors can further discuss(/investigate) the dependence of long-period tides, especially Mt,

on data temporal resolution.

We understand this comment but daily means (Nyquist period of 2 days) should be adequate for study of a cycle with a period of 9 days, irrespective of the small signals and large error bars in Figure 7. It was anyway convenient for us to use daily means which were a product of the Weighing the Ocean project. We don't feel a discussion suggested by the Reviewer's last sentence is warranted.

3. P7 4-7, Fig4 and other places in text: why is non-tidal variability larger in the south side as however there are more eddies in the north as authors mentioned in P12 5? MJO (intraseasonal) is taken as one potential contributor but it seems to me that there are still some significant features at longer timescales (e.g. in Fig4b between 350day and 450day, between 510day and 610 day). A bit more explanations/speculations are suggested to add here.

The Reviewer has misunderstood our purpose in showing Figure 4(a,b). Mf is the largest of the long-period tides discussed here and it varies a lot over the nodal cycle, hence the ratio of tidal to non-tidal variability varies over the cycle. We wanted to include Figure 4(a,b) as examples of that variability, when the tidal component was large and small respectively. The two plots (a,b) were not intended to show north/south differences in variability as such. We have added some words to make that clear.

As we mentioned, there is a lot of non-tidal variability due to eddies etc. in the north (Sheen et al., 2014) but also some in south. This results in spectra parameterised as shown in the Supplementary Material Figure 3 and discussed in Hughes et al. (2018). The MJO was mentioned in particular because the timescale of its variability is not too different from the Mm period. The features pointed out in Figure 4b are indeed interesting – they are presumably associated with rapid (non-tidal) ocean variability of some kind, there are odd features like that in many BP records that we have not investigated in detail. Fortunately, the stationary signals of the tide are fairly immune to such things.

4. Subsection 3.2: It compares the long-period tides derived from BPR and also the tide gauge record. How the power spectral distributions differ between those two kinds of records? If BPR has advantages of resolving long-period tides over tide gauge data, due to less non-tidal variability, one may expect there are more noises close to the 3 constituents' frequencies (Fig3). Is this true? It's good to show this merit.

The Reviewer is right, and this point relates to two by Reviewer 1 (P9 l24 and P9 l25-27). We have added spectra for Vernadsky for comparison. We have also considerably extended the discussion of Vernadsky data in Section 3.2 by using DAC corrections, and have added extra words to the Conclusions. Many thanks for suggesting we do this, which we should have done before.

5. P8 1-3 and Fig5b: It's worth proposing some explanations why such north-south differences are observed here, when this is not expected from the theory.

At this point in the paper, the different phase lags north and south demonstrate spatial variation unexpected from the Equilibrium tide, that (if correct) would have to be explained by dynamical tidal differences. That is what we investigate further in the discussion of Section 4 by making use of the FES2014 model which, if you read on, are explained well by the model.

6. Fig 6&7: small amplitudes and large error bars make it difficult to detect the nodal cycle. It seems to me that error bars are slightly larger in the 1st decade. Is this related to BPR data density used? It'll be good if the data availability (after QC) of such 45 BPR records is provided.

It is true that error bars are slightly larger in the first decade, but it depends which plot you mean. For example, for the amplitude of Mt (Figure 7a) they are much the same, but the amplitudes themselves are larger in the second part which results in the errors on the phases (Figure 7b) being smaller. See also our reply to Reviewer 1 (P8 l33). But in general this comment is right , we have no simple explanation, presumably it depends on the mix of locations (e.g. the F-S line in the early years), ocean variability

changes in time etc.

Most of the BPR records were re-QC'd as part of the Weighing the Ocean project and are available on the PSMSL web site. All of them were inspected for possible glitches in the time series. We have put some wording in the Acknowledgements for anyone who would like copies of the data.

7. Author used FES2014 model to discuss the spatial variation of long-period tide parameters. FES models are assimilated by satellite altimeter data, which to me however have some limitations at high latitudes. Is this a noticeable concern here?

We don't think so. The orbit of TOPEX and its follow-on missions was designed to include the Drake Passage, and there is now over 25 years of precise altimetry, so there has been plenty of data for assimilation. Anyway much of the non-equilibrium dynamics of the long-period tides can be modelled from first principles without assimilation (some references are given in the paper). We suspect that FES2014 is a very good model for the long-period tides. Anyway, it is certainly the best available for use here.

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