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

Anonymous Referee #1

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General comment: This is an interesting paper about the nodal dependence of long-period ocean tides which is a subject not much studied until now. With the increasing quality of ocean tide models and the long-term in situ and altimeters observations now available, such small signals become of importance and can be possibly estimated as demonstrated clearly here. Moreover the analysis proposed is performed in a high latitude region where such analysis has not been done before and where the long-period tides have large amplitudes but the non tidal variability is also strong which is a challenge.

Specific comments:

P2 l31: maybe add the reference Lyard et al. 2006
P4 l25: OK, but in this high latitude region, the ocean response to atmospheric pressure can be significantly different from IB + effects of wind not negligible => might need to use a model forced by the atmosphere (at least a barotropic model for high frequencies) to remove correctly this non tidal variability. Have you done this test?

P4 l29: “low-frequency process” : what are the frequencies concerned ? annual/semi annual only or some other components ?

P6 l7: have you considered the same length of record for each BPR ? if not, can you estimate the impact of the different lengths of record on the harmonic estimation of Mf, Mm, Mt ? this impact is likely not negligible and should be considered in the discussion.

P6 l29: sentence not clear. Please rephrase.

P7 l15 : add ref to eq 4

P7 l33: “28.4 +/-1.4◦” : what about the sign? Do you obtain the same sign as in eq 4 ?

P8 l4: add a sentence like “this N-S difference is likely explained by the dynamic response of the ocean at this frequency” : see the spatial patterns of FES2014 showed in supplementary materials.

P8 l15: add ref to eq 8

P8 l21: have you tried to fit cos or sin ?

P8 l30-31: mean value 0.43 is smaller than in eq 4. Please explain

P8 l33: “which follows from the larger average amplitudes in the second half of the data” : not clear, please explain

P9 l17: “individual uncertainties approximately five times larger than for the BPRs”: how do you explain this point ?

P9 l24: “the superiority of BP measurements” : this point is not clearly demonstrated here. Need a spectrum of TG as in figure 3 + see next point.
P9 l25-27: clearly modelling the non tidal variability should improve the results, you should make the test. You can use the Dynamic Atmospheric Correction (which is a barotropic modelling) to check this impact (the data are available on line on the AVISO website) or use NEMO as in page 12.

P10 l11: add references for FES2014

P11 l15: “typically 1-year long records” : for BP different lengths have been used isn’t it ?

P11 l25-26: comparison is ambiguous: did you choose the 185° contour because this is the closest to the observed average phase lag ? or do you really take the geometrically mid-passage contour ? need to clarify

P11 l29-30: indeed for 92-99, Mf amplitudes are smaller for south deployments . . . is this N-S difference small enough to be not significant ?

P12 l7: “use of 5day values of BP”: is it a running 5 days average ? why not using 1-day as what is done on BP measurements ?

P12 l10-11: “. . . correlations were weaker in the north . . .”: can you explain more ?

P12 l24: why do you use different names for Mt/Mtm ?

P12 l26: same comment as for Mm, see above.

P12 l30: “similar to that obtained above for figure 7a”: the estimation for figure 7a are not shown in the text above ? . . . to add

P12 l32: idem for estimations on figure 7b

P13 l3: likely true for old versions of tidal packages . . .

P13 l28: “should be separable from Mf . . . given a year of data”: have you performed some tests ? using a long time series and then a one year time series to be able to say that ?
P13 l29: you mean removing these small constituents using an ocean model and then analyzing the studies frequencies? but ocean models might not be enough accurate for such small constituents... please clarify.

P14 l3: + this point might also explain the different behaviours of BP and TG?

P14 l8: “our determination of Mm”: why not other components Mf, Mt? please explain

P15 l17: “stacks of records”: please explain

P16 A2: you get these formulae from eq 2 and A1?

P17 l11: how do you choose R=0.414?

P18 l5: It is not clear why you choose to use simplified formulae in this paper? explain please.

P18 l13: R=0.065?

Legend of Figure 5: “one standard error”: please give a bit more details.

Technical corrections:

P1 l16: replace by “while the phase difference for Mm”

P2 l27: replace by “seems to be a good theory”

P4 l18: replace has -> have

P13 l22: replace will -> may