Ocean Science
Discussions

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comment**

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

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Discussion paper

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 l 26: 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 l 3: 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 conxtituents using an ocean modeland then analyzing the studies frequencies ? but ocean models might not be enough accurate for such small consititutents . . . 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 l 17: "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 l 16: 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

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