Ocean Science

Discussions

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

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We are grateful for the time that both reviewers spent on this paper. The comments of Reviewer 1 are given below followed by our replies. The page and line numbers refer to the version in OS Discussions.

P2 l31: maybe add the reference Lyard et al 2006

Done

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 ?

We are simply saying here that sea level variability due to air pressure changes are largely compensated for in the BP records by air pressure itself, at the timescales we are interested in, thanks to the inverse barometer. That automatically removes a lot of the non-tidal variability from the records. We believe that is all correct. We agree that there are still other processes in the ocean (caused by the wind or whatever) that result in non-tidal variability – those are investigated using the NEMO model data discussed on page 12 which the reviewer commented on (so, yes, the 'test was done').

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

This was badly worded. Instead of 'primarily a low-frequency process' we now say 'a slow monotonic process'. We have also added an extra reference (Polster et al., 2009) and the three references now given should be adequate to give the reader an idea of the problems of drift in deep pressure sensor data.

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.

This is related to the question for page 11 line 15, see below. Almost all deployments were annual ones apart from those that used the MYRTLE instruments. If the lengths had been very different, then we agree that the lengths and noise contents of each record would determine differently the size of the uncertainties for amplitude and phase (Eq 5). However, we cannot see that being a problem in itself. We have worked with the computed formal errors from each regression and a bigger issue with this approach, as the text discusses, is the assumption of white noise for the computed errors.

P6 l29: sentence not clear. Please rephrase.

The sentence should now be clearer.

P7 l15 : add ref to eq 4

The same references apply as for M2 in Eq [3]. We have referenced Doodson and Warburg (1941) here again.

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

It obviously has the same sign as in Eq 4, as can be seen from Figure 5(a). We have added a few words to say that.

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.

We have not done this. This would be getting ahead of things. The spatial variations (dynamic response) are discussed in detail in the next section using the FES2014 model.

P8 l15: add ref to eq 8

The same references apply as for M2 in Eq [3]. We have referenced Doodson and Warburg (1941) here again.

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

Let us explain what we did again. A first step using the data from each deployment is to find the individual amplitudes and phase lags determined using Eq [5], as explained in Section 2. A second step is that the set of all the values of amplitude are parameterised as a cosine peaking at year 2006.5 when N=0.0, as explained in Section 3.1. For example, that is easy to see for Mf from Eq 4. In the case of Mm the cosine should in theory be upside-down because of the negative sign in Eq 8 – such an upside-down shape was indeed obtained for Mm in Figure 6(a) although its small amplitude meant that it looks almost like a straight line – see the text on page 8 line 16.

For phase lags we fit a sine constrained to be zero at 2006.5 instead of a cosine (see end of page 7) as you can see from Eq 4 that the nodal variation in the equilibrium tide

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for Mf or Mt is expected to vary like sin(N).

We don't understand why the reviewer points to page 8, line 21 in this context as that sentence makes it clear that there is expected to be no nodal variation in phase lag for Mm (Eq 8), and indeed there is no evident nodal variation in the data, so we just show in Figure 6(b) a straight line at the average value of 177.3°.

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

We don't understand the comment. The 0.43 mbar is the mean value of Mt amplitude. If the reviewer is asking why this is smaller than the 1.043 in Eq. 4, then the latter is the nodal factor 'f' which is a dimensionless multiplier of the amplitude in Eq 2 (the lines following explain why that is not exactly 1.0 in this case). Eq 4 applies to both Mf and Mt to a good approximation by the way, the average amplitude of Mf is of course the larger.

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

To be more complete, the text says "which follows from the larger average amplitudes in the second half of the data (Figure 7a)". So please take a look at Figure 7a again – the nodal variation means that the amplitudes tend to be larger in the second half. However, we agree there is maybe a problem with saying 'average amplitudes' which we have changed to 'larger amplitudes on average'.

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

Because the non-tidal background is much larger in a coastal tide gauge record due to air pressures and winds than in a BP record from deeper water – this is the reviewer's own point above referring to page 4 line 25. See the next two points regarding spectra and DAC corrections to the tide gauge data. Section 3.2 has been rewritten and extended.

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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.

The point is demonstrated clearly by the 'five times larger' in the previous comment and from inspection of the error bars in Figures 5 and 8. However, we agree that it is important to include additional spectra for the TG data, we should have done that before, please see our answer to the next point and to Reviewer 2 point 4.

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.

Many thanks for this comment which is similar to that of Reviewer 2 point 4. We have modified and expanded the discussion of Vernadsky data using DAC corrections in Section 3.2.

P10 l11: add references for FES2014

The web site (dated 2018) from which FES2014 can be downloaded is referenced on page 10 as FES2014 (2018) and given in the reference list. This is admittedly a strange way of referring to something, with a date in the model name which is not the date it was obtained. If there is a better way of referencing the model we would be happy to use it.

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

Not really. On page 4 (section 2) we explained that almost all the BPR records are from annual deployments (apart from the MYRTLE records). These redeployments happened at almost the same time of year, constrained by the schedules of the BAS ships, resulting in roughly 1-year records. The exact dates for each one can be found from the PSMSL web site.

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

We don't see how the comparison is ambiguous. These lines of text are just mentioning a general comparison of the average phase lag for Mm reported in Section 3.1, and given that there were a similar number of deployments north and south, compared to the average phase lag at mid-Passage from the FES2014 model anyone would conclude by inspecting Supplementary Figure 2(d). We have added '(Section 3.1)' following the mention of $177°$, which makes it a little clearer where the numbers come from. We have also added '(Section 3.1)' where Mf average phase lag is mentioned on page 10, and for Mt following the reviewer's comment on page 12 line 26 below.

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 ?

There is a misunderstanding here. Page 11, lines 29-30 are discussing Mm and not Mf. They make the point that the corresponding Mf amplitudes are not so different to later ones. We suspect that the earlier and lower Mm values referred to are to do with being further east at the south end of the F-S line. This would be another artefact of our having to deal with a data set that has both spatial and temporal dependence.

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 ?

5-day values are a standard NEMO product, see Hughes et al. (2018). Anyway, here we are discussing Mm for which 5-day sampling should be just about adequate.

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

Not really. The weaker correlation with NEMO in the north is almost certainly to do with the higher eddy variability as demonstrated by Sheen et al. (2014). Although NEMO has eddies in it, it is unreasonable to expect a model such as that to explain all the

details of that variability. It does a better job in the south. The different character of variability north and south is demonstrated in Figure 3 of Hughes et al. (2018), see also Supplementary Figure 3 in the present paper. This is already explained in the text.

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

We don't. Mtm is mentioned only once, to point out that the constituent has that name in the FES2014 model. Otherwise we call it Mt throughout. You will often find tidal constituents having different names in the literature when they have been studied by different people through the years.

P12 l 26: same comment as for Mm, see above.

We have added '(Section 3.1)' after the mention of $197°$ to make it clearer.

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

We see the problem here with the word 'above'. We meant a couple of pages above in section 3.1. The wording has been made clearer.

P12 l32: idem for estimations on figure 7b

Ditto

P13 l 3: likely true for old versions of tidal packages : : :

It is not a question of old versions of packages as such. All of them use different sets of constituents depending on the lengths of records analysed etc. The fact of the matter is that the mid-latitude heritage of much tidal research (e.g. Darwin/Doodson/Cartwright) meant that Mt was not normally included in the standard sets, although there is no real reason why it could not have been.

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 ?

There are no tests necessary. We have mentioned the periods of the additional constituents MSf, MSm and MSt and those of our main three Mf, Mm and Mt, and you can check that the pairs are all separable within a year by the Rayleigh criterion. Now, of course, measurement errors can make this procedure more uncertain. So we have added 'In principle' to this sentence.

P13 l29 : you mean removing these small conxtituents using an ocean model and then analyzing the studies frequencies ? but ocean models might not be enough accurate for such small consitutents : : : please clarify.

We have changed the text to read 'ocean tide models' instead of 'ocean models' which may have been misleading. Inference of smaller constituents is a standard procedure in tidal analyses. The details can be left to whoever in the future does the analysis.

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

We are not sure about this. We never combine BP and TG data in any fit, and within the TG fit alone the same 'k' factor will apply.

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

Because Mm is the lowest of the three in frequency and the spectrum is red (or pink). Then please see the previous sentences.

P15 l 17: "stacks of records" : please explain

'Stacks' are when many records are combined into an overall fit. It is a technique often used in seismology, for example, and was used by Trupin and Wahr (1990) to look at long-period aspects of tide gauge records. Anyone interested can read that paper. We are not digressing into a discussion of that here.

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

Yes. That is correct.

P17 l11: how do you choose R=0.414 ?

We did not choose this value ourselves. It is the value of the amplitude of the main sideband in the tidal potential. However, we agree this was unclear and have added the Cartwright and Tayler (1971) and Cartwright and Edden (1973) references again.

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

Because they are quite adequate for the simple nodal variations we are looking at here, especially given the uncertainties in the data. These simple forms are also the ones that normally appear in text books on tides. But there are applications such as Ray and Egbert (2012) where you have to take more complete ones. They are never completely correct, however, as they are trying to provide simple algebraic expressions of combinations of multiple sidebands in the tidal potential.

P18 l13: R=0.065 ?

Yes. Again, this is a value that comes from the amplitudes for the sidebands of Mm in the tidal potential. The Cartwright tables (Cartwright and Tayler, 1971) were already referred to but we have added Cartwright and Edden (1973) to be more complete. We hope this is clearer now.

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

The caption has been expanded.

Technical corrections:

P1 l 16: replace by "while the phase difference for Mm"

Done. Many thanks.

P2 l27: replace by "seems to be a good theory"

Done

P4 l18: replace has -> have

Not done. Bottom pressure (BP) is singular.

P13 l22: replace will -> may

In our opinion, 'will' is better as these other constituents are bound to be present to some extent and there is no 'may' or 'perhaps' about it.

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