

Interactive comment on “Tidal variability in the Hong Kong region” by Adam T. Devlin et al.

Anonymous Referee #1

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Comments on ‘Tidal variability in the Hong Kong region’ by Devlin et al. (OSD)

This paper looks at the variability in the semidiurnal and diurnal tides, and in overtidal, around Hong Kong and tries to relate the observed tidal changes to changes over a wider area and in MSL. It is one of a number of papers that have appeared in recent years that have pointed to tantalising associations between changes in tides and MSL that are sometimes enigmatic and always hard to explain. Therefore, the availability of a large data set from a small region such as Hong Kong is to be welcomed. However, as the authors point out, this region has undergone a lot of engineering modifications and it is therefore not the easiest of places to try and separate the impacts on the tides from those modifications from those due to genuine changes in large-scale ocean processes (the NW European coastline is a similarly problematic region given that it has had a lot of dredging etc.). The authors attempt to make that separation by also using data from a small number of sites across the vast area of the South China Sea etc. I found that quite unsatisfactory. The paper seems to me to provide findings which are far from coherent, and so do not lend themselves to easy interpretation. The authors attempt to explain all that diversity by rather (to me) a rambling discussion of ‘maybe’ processes such as reclamation, changes in baroclinicity, changes in rivers, resonance shift etc. You can explain anything away in this way.

-Thank you for your review, and for your constructive comments! We are thankful that you recognize that our study is of interest. We are also thankful to have a critical eye to evaluate our results. As we think is clear to you, we attempted to do far too much in this study! We have been studying these tides gauges for quite a while now and have found quite a bit of interesting behaviour that has piqued our curiosity in many ways. In the course of writing this first draft, we tried to include everything that we had observed, even those things that we were not yet quite sure of (such as the “minor tides” tangent that has now been better elucidated to us by the comment of Richard Ray as an error in analysis approach), or things that are hard to make conclusions about (such as trying to determine anything meaningful about the SCS tides relying solely on the sparse and only historical publicly available Mainland China tide gauge network). We admit that we tried to be far too ambitious in this first attempt. Furthermore, this paper has been waiting for review for a very long time, and while we have been waiting, we have moved forward in our work and found new discoveries and methods that have provided new insights about the data in HK. For example, Richard Ray’s comments led us to the correct way to analyse minor tides such as M_3 and N_2 (i.e., use a 9-year window for analyses) which produced stable results without the 9-year “pseudo-cycle” from constituent contamination. However, this focus on minor constituents, while improved, is not very relevant to the overview of tidal correlations in Hong Kong, which is focused on yearly-scale fluctuations that are not as apparent after performing 9-year analyses. This part of the analysis is saved for a future, global-based study of M_3 based on 9-year analyses.

Based on your comments, Richard Ray’s comment, and the new things we have been working on, we have now greatly streamlined this paper and made a better focus on relevant

and communicable results. The major changes are that we have dropped many things in the first draft and made the focus the Hong Kong local results of the four largest tides.

The relevant omissions are:

- The “minor tide” analyses (i.e., N_2 , K_2 , Q_1 , and P_1) and consequently the delta-HAT-8 analyses.*
- The South China Sea results and discussion. Also, much of the related introduction materials about the SCS dynamics, internal tide generation and propagation, etc.*
- The “historical” vs. “modern” comparisons.*
- The later discussion about M_3 and other minor tidal behaviour (this part was erroneous as pointed out by Ray).*
- Figures related to the above, which has allowed a better resolution to be used without “tiling” the results and making them too small.*
- Removal or downplaying of the suggestion of mechanisms to explain the behaviour, besides some short mentions of the possible importance of engineering projects in HK. This possibility will be explored in an upcoming modelling study using highly accurate DEMs*

-Targeted responses to your individual comments are found below. Many of these comments are not applicable after we removed the majority of things listed above.

-We will do our best to reference our relevant changes in relation to the original text line numbering and sectioning. However, with all the omissions, the form and structure of the text has greatly changed and referring to the old numbering will likely be confusing. Therefore, we will describe the changes in reference to new line numbering where applicable.

“this region has undergone a lot of engineering modifications and it is therefore not the easiest of places to try and separate the impacts on the tides from those modifications from those due to genuine changes in large-scale ocean processes.”

-In regard to this comment, in the new version, we have excluded a lot of the hypothesizing about what is causing the tidal changes (i.e., local vs. regional mechanisms) and instead just mention that coastal modifications have had a long history in HK and may be at least partly to blame via possible resonance and frictional changes. And, as we are no longer including or talking about any of the SCS observations, we don't think it is needed to make any substantial hypothesizing about the regional tidal properties.

I read the paper several times and my recommendations are:

(i) to rewrite it to focus only on the local data set from the Hong Kong area which, although may be affected by the engineering changes, does seem to present a reasonably spatially coherent set of findings. And then drop the SCS discussion which is superficial at best for such a large area. A local focus, perhaps with some modelling to provide a sensitivity study, would make for a nice paper.

-Thank you for the feedback and suggestion. We have followed this advice and have now focused only on the Hong Kong results. The SCS discussion has now been omitted, as this

data is sparse and historical. Since this dataset is mainly composed of Mainland China observations, which have not been publicly updated since 1997, studying this data does not really reveal anything useful, even though I really hoped that I could have. We had the best of intention in using this data, hoping that a discussion of these results might help fuel an interest in releasing more data publicly, but we admit this case has not been made.

-As to the suggestion about modelling, we decided to not undertake such an endeavor here. It is believed that capturing the full dynamics of the HK waters is a complex question and will take a lot of careful consideration of details (such as highly-accurate DEM that can simulate the differences in tides under different land reclamation projects of the past) and will hopefully be the subject of future studies. This present study is only meant to be an observational study to identify the interesting tidal observations. While we believe that is highly likely that the coastal modifications have something to do with this, it is also believed that proving this via modelling would be worthy of a completely different study, which we do hope to pursue more completely soon.

(ii) focus only the four main constituents. The smaller ones can indeed be mentioned in passing (e.g. if M4 is changing in an opposite way to M2) but it is the main ones that most people are concerned with understanding at the moment and, as Ray has pointed out in his interactive comment, it is not clear that the authors properly understand the variability inherent in some of the minor tides and/or in the software used to determine them. I would also drop figures 7-10.

-Thank you again for the comments which have focused our scope. We do indeed now only discuss the four major constituents (M_2 , S_2 , K_1 , and O_1), and the delta-HAT based on these four tides. The other four major tides (N_2 , K_2 , P_1 , and Q_1) do not add much to the discussion here, and the delta-HATs based on 8 tides was not too much different from the four-tide rendering, so it is better to focus only on the four most stable tides. We have also removed the old versions of Figure 8-10, which were too noisy and mostly useless, and, as illustrated by Ray, are erroneous in approach. However, we have elected to keep Figure 7 (now Figure 9) which shows the major tidal anomalies witnessed at Quarry Bay and Tai Po, and now have a briefer discussion about these observations in the context of timing with major reclamation projects as a motivation for future modelling efforts.

(iii) drop the division of the data set into historical/modern. I found the discussion of the differences between the two epochs unconvincing.

-Agreed. We were attempting to make something meaningful out of the sparse Mainland China data in relation to the HK data, but this attempt was unsuccessful. Some brief mentions are still included about the fact that there is an obvious difference in tidal variability in early years and later years at the longer records, which was another reason we decided to keep the (old) Figure 7 (now Figure 9).

(iv) try and not include so many numbers in the text which the reader just cannot absorb.

-Thank you for this comment. We agree that too many numerical results in the text can make a boring “laundry list” of data that is too hard to read. We hope that this issue has now been alleviated by the removal of the SCS data and the historical/modern comparisons.

(v) include some mention of changes in tide gauge operations, aside from just whether they were relocated. For example, are some now using radar gauges instead of float gauges? Have any studies been done of the consequent differences in the tide? Or at least flag this as a possible issue.

-Thank you for this comment. The QB gauge is the only one that was ever re-located, and to the best of our knowledge, there are no known discrepancies or errors have been documented at any gauges. All other gauges have had settlement measurements made since 1991, with no significant changes observed. We have been closely working with the data provider, the Hong Kong Observatory, at the senior level, who are well-versed in the history and quality control of the data and can verify the quality of all of data. All gauges are currently radar gauges. We therefore believe that there are no major datum issues, instrumentation issues, or other errors in our set of data used here. We have also added a few ore publications that are old official reports from HKO about early tidal analyses of the HK tide gauge network.

Some detailed comments:

34 - there is no need for a hyphen in mean sea-level. On the other hand there is in e.g. sea-level rise.

-Thanks for the clarification. We have fixed these instances here and elsewhere.

39 - drop 'inter-tidal'

-Dropped, thanks.

44 - define PSI

-Done

48 - well, if you have chaotic results (which are not necessarily the fault of the authors of course), then you can always explain them as a combination of many processes, especially when you have no real data to back up the suggestions. (I know this is a harsh remark, but that's the way this paper reads to me.)

-Thanks for the comment, and no offense taken. We admit this was a bit rambling. We now have focused this better to be applicable to HK, and only suggest the possibility of the frictional/resonance mechanism under rising MSL because of local engineering changes.

84 - start new sentence at Therefore

-Done.

96 - +/- 5 percent of what?

-This indicates a 5% modification of total sea level due to tides for an arbitrary MSL change. We have tried to make the language clearer.

97 - 65% ditto

-This number is dropped, and the discussion is better focused now.

about 97 - the TAC and delta-HAT acronyms are mentioned here but only explained properly below. It seems to assume the reader has read the other Devlin papers. I would define them a little more fully around here.

-I have tried to explain these metrics better here, or at least enough to be introduced here, with the details better discussed in Methods..

I don't have a problem with the TAC parameter and name by the way, but I really don't like delta-HAT. As I understand it, it reflects the maximum level that would be obtained in a year from the time-dependent amplitudes and phases extracted from the admittance method? But HAT to most people refers to the maximum level that would be obtained by running a set of tidal predictions over 18.6 years. I would find another name for this parameter. Also it has nothing to do with time series as far as I understand it, it is just the sum of the amplitudes for either the 4 or 8 constituents for that year (please clarify if not).

-Thank you so much for this comment. We will try to answer this carefully and explain our logic. Over the course of developing these novel methods in other studies (Devlin et al, 2014; 2017a; 2017b), we wrestled with many different acronyms and names for our metrics of what is now TACs and delta-HATs. For instance, originally, we called them TAT (tidal anomaly trends) in Devlin et al., 2014, but later decided that name was inaccurate, as what we observe is not really a "trend". So, we decided that TAC was better later (Devlin et al., 2017). But I felt a little conflicted about changing the acronym, since it had already been established in my previous study. A similar situation applied to the use of delta-HAT. At the onset, my co-authors and I acknowledged that some people would think of the classical definition of HAT (based on the 18.6-year analysis). We decided that using "delta-HAT" would imply a shorter timescale change in this metric, which could not be revealed from an 18.6 yr analysis; we are interested in yearly-scale fluctuations. However, we have always introduced our method as a "proxy" or "indirect estimate" of the change in HAT. Since this language and acronym has been used in a recent paper (Devlin et al., 2017a), as well as in a new paper that studies the Atlantic using these methods which was recently accepted, we really want to keep the language consistent in the current paper about Hong Kong.

To clarify our methods, we do combine the tidal amplitudes and phases of the top four tides garnered from the yearly admittance values into a single complex time series, and the absolute value is taken to show the highest actual level reached by that combination, which is then detrended and regressed against detrended MSL over the same window.

We therefore want to make the case that we want to keep this name of delta-HAT, but we will also better explain the distinction of it being a "proxy" in the manuscript where applicable. If you still take issue with the use of this acronym, we will relent and try to find a new one, especially if you have a good suggestion.

98 - doubled. With respect to what? Any exceedance level will be with respect to a datum.

-Doubled, as in almost double of the exceedance of MSL alone (above an arbitrary datum).

98 - I would drop the TSL acronym. There is no need for too many acronyms. 'Extreme sea level' would do here just as well.

-We have adopted this format now.

176 - tide gauge records

-Fixed.

189 - website should be the website

-Fixed.

213 - this is true only if the nodal and other low-frequency modulations (i.e. perigean) are the same in the real ocean as in the potential. There are many examples from shallow-water areas of them not being the same.

-Good point, and we have added this caveat to the methods. However, every gauge used has been carefully analysed by eye to identify any instances of "leakage" of low-frequency signals (nodal or otherwise). As Ray has instructed in his comment, the 8.85 yr perigean cycle can still be apparent after admittance methods are applied in N₂ and other constituents. But we have now dropped the N₂ analysis from the paper. However, in the HK region, the low-frequency modulations of the four main tides are not apparent after the admittance method is applied.

223 - state these time series are annual values (presumably)

-We have been more explicit in this paragraph about time-series being annual.

226 - reword: which has previously been shown to be more apparent

-Fixed, thanks!

232 - year-to-year change. (See my comment above about delta-HAT which is bad name)

-We have made this change. However, please see the comment above about the use of the delta-HAT name.

234 - typically 75%

-Fixed

237 - you use the word 'minor' here to refer to N₂, K₂, P₁ and Q₁, but minor is used for a different set below. I would change 'minor' here to 'latter four' or similar.
about 244 - I would add 'amplitude' many times in here and in the figure captions.
For example, you mention 'tidal perturbations' here - perturbations in what? What are

they? I think the problem is the jargon half the time.

-Most of these comments are no longer relevant since I removed a lot of material about minor tides, but I will clarify that we use “tidal perturbations” to indicate the variation of the tidal admittance from the (detrended) mean value.

251-254 - why is this sentence relevant? You don't do any projections into the future.

-This sentence did not mean to assume anything about projections. We include this part to clearly indicate that the calculated TACs can be assumed stable and constant over the time window considered (30 years or less), a “pseudo-linear” assumption, if you will. This point needed to be stressed in previous papers, and this window length was employed in all previous works by Devlin et al. so far. Mainly it is matter of consistency (similar to our justification of keeping the delta-HAT moniker). Comparative analyses in other studies have shown that any longer of a regression analysis may obscure changes in the TAC over time. However, we have modified the text a bit to reflect this explanation, and removed the word “extrapolate” which may have been adding to the confusion.

265 - say why you use the last 30 years. Data better?

-Please see the above comment. To reiterate, longer time window may obscure changes in the TAC values, and 30 years has been shown to be a good window to use based on previous studies.

273 - you use the words historical/modern here and early/later lower down which gets confusing. Anyway, as mentioned above, I would drop this aspect.

-These comments should no longer be applicable, as the historical and minor material has been omitted now.

293 - does 'minor' here mean the 4 above? Be clear.

- These comments should no longer be applicable

304 - I am not sure anyone knows where Beibu Gulf is (no offence intended). Perhaps add 'on the south coast of China'.

-Some in America and the Western world would know it as the Gulf of Tonkin. I was trying to use the name that the locals use. However, this comment is now moot since I have removed the SCS discussions.

306/308 - now we have early/later

-No longer included.

325 - you quantify the others but not for Bintulu.

-No longer relevant.

392 - a record can be flat or have zero trend. You can't have a 'flat trend'

-Fixed.

413 - 'minor' here means quite a different set (discussed by Ray)

-No longer relevant.

417 - there is discussion of the perigean dependence of N2 along the China coast in the Feng et al. paper by the way.

-Thanks for the reference. This is no longer relevant to the current paper, but I am doing a new study about the perigean-modified tides based on a world-wide set of data using 9-year analyses, so I will read that paper with great interest!

423 - 'missed'. It looks to me to be there is a little bit.

-No longer relevant; material removed.

425 - why is this interesting? N2 would be in phase wouldn't it in a small area like this?

-No longer relevant; material removed.

456 - 'it is apparent'. It is in figures 7 and 8 ok but not to me for 9 and 10?

-No longer relevant; material removed.

464 - who -> which

-No longer relevant; material removed.

467 - will be -> are

-No longer relevant; material removed.

470 - correlations of what?

-No longer relevant; material removed.

659 - the Conclusions for the reasons for the tidal changes are just speculation. You should start this section by reviewing what the data tells you.

-Thanks for this comment. We have done a lot of work to rewrite the conclusion section with some more focus. We now try to simply discuss the observations, discuss lightly some possible reasons (i.e., harbour changes or regional climate changes), and lay out some future possible steps.

824 - it is hard to see the red and green on top of the dark blue. The caption should say the blue shows depth in metres.

-No longer relevant; maps of the SCS are now removed.

figure 3 and others - I read this paper on A4 paper and I cannot read what's in the legends or even the axis annotations of some of the figures.

-Figure have all been redone in the new version. Without the SCS material, we now use single-panel figures, focusing only on the most relevant observations.

In (b) and (d) there is a red square box for the Hong Kong area not mentioned in the caption. They also have the Egbert model values which are not discussed in the text, so why have them?

-No longer relevant; material removed.

In (c) there are captions for each point like CHC which are unnecessary given Figure 1.

-OK, we now only show station names in Figure 1, and all other figures have names removed.

figure 5 etc. caption - again the word 'amplitude' needs adding whenever you say something like 'detrended (M2+S2+K1+O1)'.

-Thanks, fixed!

figure 7 - I can understand the mean values for the tides but the mean values of MSL require to know the datum.

-All water levels given for MSL are in relation to the "chart datum" as defined by the Hong Kong Observatory. The chart datum is defined as an additional 0.146 m below the Hong Kong Principal Datum (HKPD). The HKPD determined for the years 1965-1983 was approximately 1.23 m below MSL. The HKPD has been recently re-determined using data from 1997-2015 to be 1.30 m below MSL. Therefore, all MSL values are given in relation to the sum of both values, so 1.376 m for the early years, and 1.446 m for the later years (approximately). Please see the following weblink for a full history of the datum in HK: <https://www.hko.gov.hk/blog/en/archives/00000204.htm>. However it should be mentioned that what is shown in current figure, what is plotted for the MSL component is the zero-frequency component of the harmonic analysis (i.e., de-tide MSL) which may have some offset from the full MSL (tides included) as determined from HKO. We now include more material about this history in the text.

figure 9 and 10 - I can't read the information on the right.

-No longer relevant; material removed.

Table 1 - add an extra column for the number of years of data used.

-OK, done!