Answer to Anonymous Referee #2

This manuscript presents a detailed study of the influence of tides and wind on tidal currents offshore Singapore, which was developed using a series of statistical methods. Careful analysis with solid data recorded (1 year) from fieldwork is presented, and the authors expose this detailed combination of statistical processes to assess the non-stationary behavior of coastal currents.

While I found the authors have done excellent work and the paper is well-written, I still suggest a moderate revision following the comments that I listed below:

We thank the referee for their kind words.

1. The authors need a map or more to show the directions of minor and major axes of the tidal current (e.g., tidal ellipse) at each TCM, the direction of wind to explain the conclusions. As a reader, I found it difficult to figure out the direction of wind stress, tidal currents, and the coastline.

   We have updated Figure 1 to include the bathymetry in the map, as well as Figure 3. Figure 3 now consists of zoomed-in maps of Pulau Hantu and Kusu Island with their wind roses and tidal ellipses of M_2, S_2, K_1, and O_1 at each TCM. The tidal ellipse parameters are calculated by performing harmonic analysis over the entire observation period.

2. The wavelet analyses (CWT and WC) are really insightful in terms of the results. The non-stationary behaviors of coastal currents can be seen clearly. However, in my point of view, the authors should discuss this outcome as a "low-frequency signal". That is more meaningful as the authors also mentioned ENSO in their discussion. For low-frequency signals, as the wavelet indicates some period of 32 days (L266), I suggest the authors explore the linkage to the intraseasonal variability (30 to 90-day) in the tropical atmosphere (e.g., Madden-Julian Oscillation)

   We thank the referee for making this additional point. For a start, we redefined such signals with longer periods as “low-frequency signals” instead of “signals with subtidal frequencies”. We briefly introduce the MJO as a dominant influence of intraseasonal atmospheric variability and expanded on its characteristics and relationship with low-frequency coastal currents in the Discussion section.

3. Lines 275-280: The tidal currents are also distorted by the changes in bathymetry. Can the authors discuss the role of bathymetry more here or provide more information about bathymetry on this study site?

   We briefly described the bathymetry in the Introduction and delved deeper into how the complex bathymetry distorts the tidal currents, as seen from the fluctuating phase relationship with the diurnal wind stress over the entire observation period, in the Discussion section. The deeper depths observed near Kusu Island likely help to explain the unusually weak tidal currents in Kusu South.

4. I need clarification when the authors discuss the seasonal variation of the K1 amplitude of currents. The authors discuss that this might be due to the monsoonal winds. However, the results show that other major tidal constituents (O1, M2, and S2) are also large, but they did not present seasonal variation as K1 (Supp. Material). So why do coastal currents only follow K1? I think the authors need to discuss the interaction between tidal constituents in more detail here (e.g., in Van Maren and Gerritsen, 2012). The characteristics of the tide in the SCS are complicated as the oscillations of standing waves could produce resonance in a semi-closed or complex coastline water body. I suggest the authors put more analyses or explanations in place to make this point more solid.

   Although performing harmonic analysis over the entire observation period ignores non-stationary behaviour, we are nevertheless able to extract additional tidal constituents that could be useful in explaining the seasonal variation. In the Discussion section, we explored the result of the interaction between P_1 and K_1, the former which cannot be resolved using STHA, and discovered that this interaction gives rise to the semi-annual variation of both the strength of alongshore currents and also possibly the K_1 amplitude. This interaction is depicted in Figure 10 of the revised manuscript.

Other minor comments:

- Fig.7: it is really hard to see the arrows on the graph. Also, a legend of a vertical color bar needed to be included.
- We have replaced the diverging colourmap, which in hindsight is better suited for displaying anomalies, with a sequential colourmap for all wavelet analyses. The sequential colourmap highlights regions with higher values yellow, which contrasts well with the black arrows in Figure 7, now relabelled as Figure 8 in the revised manuscript. We have also made the arrows slightly bigger and labelled the colour bar.

- L175: need to make the unit consistent throughout the manuscript
- Thank you for pointing this out. We have changed the unit to cm s^{-1} throughout the manuscript.

- L284 and L288: The phase angles are 0-90 and 0-270, respectively, but why are they converted to a same time lag of up to 6 hours? Are they the same, or do we have a time lag and time lead here?
- We have added extra explanations on the interpretation of the phase angles in sections 2.2 and 3.5, and all phase angles were calculated with respect to the phase. We also specified the leading variable depending on the direction of the arrows.

- Table 1: How much is the percentage of explained variance for PC2 at each site? As they kept PC2 in, they need to provide the percentage as they did for PC1.

- We have added the column of the percentage of explained variance for PC2. For clarity, we have also split Table 1 into two tables (Tables 1 and 2 in the revised manuscript) showing properties of currents and wind separately.

- Table 1: Please check the header: "Direction of mean current speed"? Or just "Direction of mean current?"
- Thank you for spotting this. We have changed the header to “Direction of mean current”.

- L193: As I commented above, the authors need a map to show the direction of the coastline and the currents.
- We have corrected this, please see reply to the above comment.

- Fig 3: I suggest the authors show a graph of monthly averaged wind stress for the study site.
- Good suggestion. We removed the hourly averaged wind from June–Nov and combine all the plots for Hantu and Kusu into one figure (Figure 4a in the revised manuscript). We did the same for monthly averaged wind as well (Figure 4b in the revised manuscript).

- L328-340: I think this paragraph is better to be in the conclusion rather than in the discussion
- Good point, thank you. We have moved the paragraph to the conclusion.