

# Reply to Editor and Reviewers, re: revision 1 of egusphere-2022-821

Once again, we appreciate the time and interest of the reviewers in carefully reading this manuscript. The reviewers' comments are reproduced below in black, and our replies to their queries are typeset in *blue*.

## Article Validation, Polina Shvedko

Notification to the authors:

The title page of pdf manuscript file must include the full institutional addresses of all authors. However, the address for affiliation #2 looks like the it is not completed. Please enter full information for the next revision.

*Done.*

## Reviewer #1:

The authors have answered and revised my questions and concerns. Therefore, I do not have any major suggestions and comments except two minor comments below that should be clarified before ready for final publication.

1. In the paragraph 175 (Discussion). Could you provide some clues and speculate for the possible reasons why some regions with large delta-Z (i.e. along the coasts of China) but no significant Chl-a concentration, while the tidal mixing from SST (i.e., Susanto et al., 2019) and numerical model (Nugroho et al., 2016) showed strong tidal mixing signatures.

Susanto, R.D.; Pan, J.; Devlin, A.T. Tidal Mixing Signatures in the Hong Kong Coastal Waters from Satellite-Derived Sea Surface Temperature. *Remote Sens.* 2019, 11, 5.<sup>1</sup>

*The analysis of Susanto et al 2019 is concerned with SST signatures in the region around Hong Kong and the Pearl River Estuary. They use spectral analysis to analyze SST data and note variance at the spring-neap periodicities associated with M2-S2 and K1-O1 tides. Their SST analysis is bolstered by in situ data, which reveals the same modulations of currents and vertical shear.*

*The reviewer's comment states that some regions along the coast of China have large delta-Z, but this is mistaken and not indicated by Figure 6b. Perhaps this was a typo in the reviewer's comment. In any case, it is indeed interesting that tidal modulations of SST in that area are evidenced in Susanto et al 2019 and Nugroho et al 2016, but not in our work on Chl-a.*

*We speculate that an explanation for this discrepancy could be the low signal-to-noise of tidal Chl-a modulations along this part of the Chinese coast. Based on the power spectra shown in Susanto et al 2019, the SST peaks at Mf and MSf periods barely exceed the background level (their Figure 5); although, they did pass the significance test for mapping the harmonic analysis which ought to be very similar to the test we used (their Figure 4).*

---

<sup>1</sup><https://doi-org.proxyum.researchport.umd.edu/10.3390/rs11010005>

*Overall, investigation of Chl-a in coastal areas such as the Pearl River Estuary is probably complicated by anthropogenic influences, especially the eutrophication of coastal waters (e.g., see discussion and references in Zhang et al, 2022, “Categorizing numeric nutrients criteria and implications for water quality assessment in the Pearl River Estuary, China”, Front. Mar. Sci.<sup>2</sup> The average nutrient concentration of these waters may be such that the tidal modulations of mixing do not result in modulations of Chl-a, even if they are visible in physical parameters such as SST.*

2. Susanto, R. Dwi, and Richard D. Ray, Seasonal and interannual variability of tidal mixing signatures in Indonesian seas from high-resolution sea surface temperature, Remote Sensing, 2022, 14.<sup>3</sup>

Author response: This reference is now cited in the Results regarding differences in Chl-a MSf between the dry southeast monsoon and wet northwest monsoon.

» Please recheck, I do not see any of this reference in the Results nor entire manuscripts.

*We apologize for the oversight; the reference was added and deleted from intermediate drafts. We feel that it is important to direct readers to that work, because of its discussion of monsoonal modulations of the spring-neap cycle of SST. The reference has now been added to the end of the sentence at line 31.*

## **Reviewer #2:**

This manuscript is much improved! They did a great job revising the abstract and the introduction to set the reader up for what the paper will be presenting. I have a few minor points, listed below, that would improve the manuscript. Overall I feel it is ready for publication now.

*Thank you. Your comments and those of the other reviewer were significant and appreciated. They are now noted in the Acknowledgements.*

Line 41/42; “could explain the association of remotely-sensed Chl-a with spring-neap currents” rewrite as “could explain the association of increased Chl-a with spring-neap currents”. It’s the increase in chl that is significant, not that it is measured by satellites.

*Agreed.*

Or is it changes in chl? The second possible mechanism listed, the resuspension of sediments reducing PAR, would reduce chl. Clarify what is meant here.

*The parenthetical phrase, “(which ought to decrease apparent Chl-a)”, has been added to clarify.*

Figure 1. Consider adding a point to indicate the area of the data analysis shown in Figure 2.

*Done.*

---

<sup>2</sup><https://doi.org/10.3389/fmars.2022.1004235>)

<sup>3</sup><https://doi.org/10.3390/rs14081934>