The paper examines tide gauge data and satellite altimeter data in the East and China Seas in an attempt to determine possible linear trends for the M2 ocean tide. That topic is timely, but unfortunately I do not find the quality of this work high enough to meet the standards of Ocean Science. The analysis is pedestrian (at best), the English rather poor, and the discussion superficial. Below I briefly mention major shortcomings; I do not bother tabulating minor problems. My overall recommendation is that OS rejects the paper.

1. I had hope that a discussion of tide-gauge trends around China might bring forth new data. Alas, the authors use the standard set of old data archived at the University of Hawaii which has not been updated since 1997. (The exceptions are two gauges from Taiwan, which are recent, and Hong Kong and one other site, mentioned below.) Evidently the government of China continues to withhold tide gauge data from scientific study. For this reason, there is nothing new in the tide gauge analysis that was not already published by Feng, Tsimplis, and Woodworth (2015). Moreover, the analysis and results of Feng were superior and more complete than the work reported here.

On this same point of missing modern data: The authors emphasize the large amount of coastal changes now ongoing along the Chinese coast. And yet they use tide-gauge time series that stop 25 years ago. It seems more modern data, and not just from Taiwan, are required to examine properly this problem.

In fact, we mainly want to use satellite altimetry data (Jason and Envisat) to calculate the variation of tidal harmonic constants. The data of tide gauges along the coast are mainly used to verify the results of satellite altimetry solution, although the time ranges of both data collections are different. It has no choice but using these data derived from University of Hawaii, especially since we just collected only one tide gauge Lianxinggang. In addition, the harmonic analysis method combined with an additional time-varying model in this paper is not exactly the same as that used by Feng, Tsimplis and Woodworth. Moreover, what we focus on is China's open ocean, rather than the several places where the tide gauges are located.

We have modified the introduction and discussion of the article, and the language has been re-polished. Please review.

2. There is not a single error bar in the entire paper. There is no way to know if any of the computed trends are significant or not. Error analysis (confidence intervals) are critical to this kind of work. Moreover, a proper error analysis is not trivial -- i.e., one cannot just use uncertainties given by some regression package that assumes white noise. Thus, this critical part of the work is missing.

Yes, we quite agree with you. We have added 95% confidence interval to the calculation results of the tide gauges in table 4. Please review.

3. There is no appreciation for the fact that 18.6-year nodal modulations of tides can potentially impact estimation of tidal trends, if the modulations are non-equilibrium. In fact, an important result from Feng et al. (2015) is that, indeed, the nodal modulations at many of these stations are non-equilibrium. The authors should have considered this point, since it was stressed by Feng et al.

Thanks a lot for your advice. Indeed, the 18.6-year nodal modulations is likely to affect the estimation of the linear variation of M2 tide. In the conclusion of Feng's article, the observed modulations of the M2 and N2 amplitudes are smaller than theoretically predicted at the northern stations and larger at the southern stations. Therefore, this critical part should be taken into account in our discussion. We have modified according to your suggestion and Feng's conclusion. Please review.

4. There is no reference to the work by Bij de Vaate et al. (doi: 10.1029/2022JC018845), who made a much more thorough study of possible tidal trends from satellite altimetry. They reported trends (with uncertainty analysis) in the East China Sea, which should have been compared here. They also found that trends in the South China Sea were not significant, at least at satellite cross-over locations.

Thanks a lot for your advice. It is our fault to neglect this excellent work by Bij de Vaate et al. Their work showed the change rate the main tide constituents in the global sea. Due to the resolution, we can roughly see that the change rate of M2 is quite small in the China Seas, especially in the South China Sea. We have introduced this work in the introduction of the article. Please review the article for detail.

5. There is no appreciation for the possible existence of systematic errors in satellite altimetry which could impact trend estimation. Nor is there discussion of the apparently large errors in M2 (not M2 trends) seen at Jason cross-overs -- 12.8 cm, according to Table 4. It is difficult to see how mm/year trends in M2 could be determined in the presence of such large noise in the mean M2.

Yes, we agree with you that there may be possible existence of systematic errors in satellite altimetry which could impact the M2 trend estimation. In the South China Sea, the RMS of M2 amplitudes at Jason cross-overs is about 12.8 cm, which is of a large order of magnitude. Therefore, we also agree with what you mentioned in point 2 to add error bar. The parameters change rates of M2 is quite small, so the system error is likely to cover the change rate. However, we also hold another view that even if there is a systematic error in it, we calculate the rate of change, the systematic error may not have much effect on the rate of change, because in calculating process, the systematic error may be eliminated by the first difference. Therefore, when calculating the amplitude, phase lag and the corresponding change rate of M2, the influence of systematic error on the former may be more intense. We have added a discussion of the results in table 4. Please review the article for details.

6. Much of the mathematics laid out, especially the large matrix in Eq (4), is not needed. Everybody already knows how to set up a least-squares problem.

Actually, we used the tidal harmonic analysis model of time-varying analysis, which is different from the traditional tide harmonic analysis. We expanded the sine and cosine part of tide constituent into a linear combination, and incorporate the linear change rate and the initial term into the least-square calculation. So we hope to show the detailed calculation process.

7. In both the Abstract and the Conclusions, it is stated that the detected tidal trends in M2 are caused by changes in water depth and coastlines of estuaries. There is no evidence presented that backs up these statements. They are merely assertions.

Indeed, there is no direct evidence to prove the tidal parameters changes caused by the water depth and coastlines of estuaries. Therefore, we just stated that the rapid change in water depth may be one cause. Actually, there are also related literatures that through tide motion numerical experiments, and found that the coastline and water depth variation could significantly alter M_2 amplitude in the Bohai Sea and Yellow Sea (Pelling et al., 2013). In recent years, especially in the past two decades, great changes have taken place in many coastal areas in China, including the depth and coastline. It is on these grounds that we have made this speculation. Of course, we are also planning to carry on relevant experiments, hoping to prove it.

8. As far as I can tell, there is one tide gauge used here that is not from the UHSLC: the gauge at Lianxinggang. In the "Data Availability" statement, the authors give a web site for these data, but the link did not work for me.

Sorry about that, we upload the tide gauges data on the Baidu network disk, and the foreign IP address may not be able to access China's domestic links. We will re-upload the data later, putting them on a publicly downloadable network disk, or submit it directly to the editorial department.

9. I had difficulty with the color scale used in Figures 3 and 4. It is not easy to distinguish positive trends from negative trends, let alone decipher the magnitudes.

Yes, this point was also mentioned in the comments from the reviewer 1, and we have revised the color scale according to the request. Please review.