Comment on gmd-2020-378
Anonymous Referee #2

Referee comment on "Turbidity maximum zone index: a novel model for remote extraction of the turbidity maximum zone in different estuaries" by Chongyang Wang et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-378-RC2, 2021

General comments

This paper describes a new approach to identify turbidity maximum zone using an index (TMZI) that combines observations of Chla and turbidity (TSS), and applies the index to 3 estuaries in Guangdong, China. A key hypothesis is that TSS affects primary production and is thus highly correlated with Chla, which is mostly valid. The manuscript is very poorly prepared and importantly, I have some serious reservation on the claims, and recommend rejection.

Major comments

As the authors alluded to, estuarine TMZ’s vary greatly in different estuaries, and I think this site specificity is for good reason. As the definition of TMZ suggests, it’s not the absolute values of turbidity but local maxima (attributed to physical or biological processes) that lead to TMZ, and the latter should be site specific and potentially not comparable across systems. I suggest they first give a rigorous definition for TMZ, as this underpins the significance (or lack thereof) of the claims. Phrased in another way, how can one measure the accuracy of any method that quantifies TMZ (as they repeatedly use ‘accuracy’, ‘good consistency’, ‘good performance’, ‘more natural’, ‘agreed better with reality’ in the texts)? Unless this key issue is addressed, there is no way to assess if the new method is actually better than previous approaches. I found the ‘validation’ sections have a lot of hand-waving claims, and lack rigor for scientific journal. Better-than-previous study-results is not sufficient (not to mention that ‘better’ is ill defined here).

Short of a rigorous metric to measure accuracy, an alternative would be to use pattern recognition technique to quantify the ‘better performance’, but we still need a definition of ‘ground truth’.

It’s also not sufficient to demonstrate that TMZI works for 2 other estuaries in the same
province. A variety of estuaries with different physical and biological characteristics is needed to truly support the claim. There are systems that other organic matters than Chla are dominant.

**Minor comments**

TSS usually includes CDOM and Chla, so there may be auto-correlation between TSS and Chla, especially for systems dominated by organic matters. This needs to be explored.

Can’t Eq (1) be simplified, as exp and log cancel out?

There are also very extensive syntax errors and confusing sentences throughout the texts, and below is an incomplete list. The authors should go over the text very carefully. There are also mentions of geographic names (Neilingding etc) that should be illustrated in plots.

Ln 54: ‘within limits’; ln 58: ‘progress’; sentence on ln 84; ln 88: ‘latent’; ln 133: ‘famous’; ln 168: expected; ln 217: while; ln 224: referring to; ln 235: ‘null’ (near zero is different from null); ln 242: read; ln 256 (sentence); ln 275: extracting; ln 279: season; ln 289 (sentence); ln 298: similar; ln 307: indicated; ln 386-7 (sentences)....