Replies to: Interactive comment on “Increasing turbidity in the North Sea during the 20th century due to changing wave climate” by Robert J. Wilson and Michael R. Heath

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General comments The authors relate historical observations regarding water transparency (Secchi-disk depth) with calculations of shear bed stress based on model hindcast simulations. By this, they demonstrate that increased mobilization of sus- pended particulate matter is a major driver for the negative trend in tranparency that was observed in the last century in the area of the North Sea. I really like the concept and the idea of the paper. Although the trend of decreasing overall transparency in the North Sea over the last century is already known (as the authors show well with their comprehensive literature review), the reasons and drivers remain largely speculative to date. In this respect the current work contributes to understand some of the underlying processes. However, in some cases, the conclusions drawn are not always supported clear enough by the data shown. In this respect, I would recommend improvement of the manuscript. Hopefully, the remarks and questions below are helpful in this context.

Introduction: As I understand the linear regression given in Håkanson (2006), it shows a linear relationship between the log values of SPM concentration and Secchi disk depth. Thus, changes in one parameter are transferred logarithmically to the other. Therefore, I would be careful with the “20 % increase in SPM” statement (also in the discussion), inasmuch as it is based on the average decrease in Secchi disk depth Capuzzo et al. found.

We have removed the 20% reference in the introduction, as it wasn’t really necessary. The 20% increase in the discussion was referring to the approximate increase in SPM we would expect in the south eastern North Sea. This was based on the regressions for that region. The original text did not make that clear, so we have reworded it.

Methods: Page 3, Line 4: Does diffusivity play really a role in this context? If so, please elaborate a little bit more on that and/or give a citation.

The influence of diffusivity on the vertical profile of SPM is discussed in Heath et al. 2017. We have therefore moved the reference to Heath et al. 2017 to the end of this sentence, where it is more appropriate.

Page 3, Line 7: What is the rationale behind the 0.5 ◦C difference as threshold for a stratified water column? If this is a common value, please refer to the appropriate literature.

The use of 0.5 C was motivated by the North Sea Region Climate Change Assessment, which used this as the metric for the onset of seasonal stratification. The text has been modified to make this clear, and to reference the North Sea Region Climate Change Assessment.

Page 3, Line 27+: Could you explain why you are using two different datasets for calculating bed shear stress hindcasts? Wouldn’t it be better to use the larger one in terms of being consistent in the data over the whole period (although missing the years 2011 to 2017)?

Ideally, we would use one data set, not two. However, the comparison with satellite SPM was carried out to provide the best available quantitification of the large-scale influence of waves on SPM. The ERA-20c reanalysis is much less appropriate than the ERA-interim for two key reasons. First, it’s temporal coverage is limited to before 2011. Second, ERA-interim is a higher quality data product.

The methods section has now been adjusted to make clearer why two separate wave reanalysis were used.

Page 4, Line 26: What means “Core data analysis” in this context?

This has been reworded to state that we were referring to the R package used for the bulk of the data manipulation.

Results: Page 5, Line 8-9: From my point of view, the seasonal pattern is not readily visible in Figure 1 (right).

After looking at this figure again, we agree the seasonal pattern is not particularly clear. We have experimented with a number of different colour scalings and have concluded that this cannot be made readily visible. Instead we have switched to just showing annual mean bed shear stress.

Page 6, Line 3-4: That the relation is positive is not visible from the R2 values given in Figure 2. Maybe refer also to Figure 3 at this point. Further- more, I would soften the statement “across almost the entire study domain”, because even when the water column is mixed, there are some exceptions (as also stated by the authors). However, beside the two plume regions mentioned, also the English Channel, the Irish Sea, as well as the whole British east coast appear poorly impacted by the shear stress in terms of SPM.

The original paragraph was poorly worded. It has now been amended to make it clear that in tide-dominated regions the R^2 values is low.

Page 7, Line 1-5: If the relation between shear bed stress and SPM is decoupled in the stratified season, what are then the drivers for the Secchi-disk decline in these months? Or is in this season also the decline in Secchi-disk depth lower? If so, the authors could refer to the appropriate literature or show the respective data.

“Decoupled” is perhaps not a totally accurate term. What we mean to say is that when the water column is stratified variations in vertical current shear and diffusivity appear to have a much greater influence on temporal variations in surface SPM. However, this does not mean that bed shear stress does not explain the decline in water clarity during spring and summer during the 20th century. If stratification levels remained the same then bed shear stress likely drove a large part of the decline. However, whether this is true is an open question.

The results section now has a sentence stating that during stratified conditions the influence of the thermocline etc. dominates the vertical profile of SPM.

Page 7, Line 10+: Maybe incorporate the change in the trend into the main manuscript, as it is interesting and contributes to the whole story.

This an interesting part of the story, but we have reluctantly chosen to keep it in the supporting materials. The key focus of the paper is on what happened during the 20th century. Moving the two supporting figures to the main text risks undermining that, as we would have 6 figures on present day conditions, but only one on historical changes.

Page 9: The authors emphasize the strong decline in Secchi-disk depth south of 53◦N (Figure 4, right side), and explain it with an pronounced increase in shear stress across the region. However, according to the left side of the figure, I cannot see that the decrease in Secchi-disk depth at this point correlates to an increase in bed shear stress, which appears to be relatively small in this area (approx. 0-20%). However, as in this area the East Anglian plume as well as the plume of the Rhine is present, I would rather think that the decline in Secchi-disk depth here might be controlled by changes in e.g. river outflow (as stated by the authors before). Nevertheless, for the Northeastern part of the area (53-56◦N, 4-8◦E) the relationship appears to be valid, although the number of data points is comprehensively small.

We have now moved the historical Secchi disk depth data to a separate figure. This was originally placed beside the bed shear stress figure to reduce the figure count more than anything. However, with hindsight this was likely not a good choice. Because the data is very sparse, we can only get an indicative idea of what the spatial patterns of Secchi disk depth changes were. The key issue is whether the big picture stories agree, and they largely do.

Discussion: Page 10, Line 13-14: I think this statement is too strong. Instead I would claim that according to the data available shear bed stress is probably an important parameter in order to explain the transparency decrease in the last century.

The text has been changed to be something more cautious. We have now changed the text to say large reductions in water clarity would have resulted from the bed shear stress changes shown.

Page 11, Line 12-18: Maybe some of the discrepancies could also be explained by a seasonally variable contribution of the organic (e.g. phytoplankton) part of SPM. Turbidity is also influenced by the presence of pelagic phytoplankton.

This should have been stated on page 11 lines 8-13, which referenced Jafar-Sidik who found that satellite SPM potentially mixes up SPM and phytoplankton during summer months. The text has been amended accordingly.

Minor comments

Page 3, Line 14-15: Check the brackets for the reference.

This has been corrected.

Page 6, Line 1: “and SPM” after bed shear stress appears to be doubled.

This has been corrected.

Page 7, Line 7: Maybe replace “bed shear stress and SPM” with “the two parameters” to avoid doubling of the terms with the begin of the sentence.

Agreed. This has been changed.

Page 11, Line 9 + 13: “in situ” instead of “in-situ”

This has been corrected.

Caption Figure 1: In the text is stated that the bed shear stress calculations are calculated after Soulsby & Clarke (2005), but in the caption stated Soulsby (2006). Please explain or correct.

It should have been Soulsby and Clarke in the caption. Now corrected.

Caption Figure 4: “Century” or “century”; please keep consistent

This has now been made consistent throughout the text