Reply 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

Anonymous Referee #3

Received and published: 17 July 2019

General Comments: The authors investigate the relationship between suspended par- ticulate matter (SPM) and bed shear stress (BSS) by means of historic, satellite and model data. They motivate well in their literature review that decreasing water clarity in the North Sea may be linked to increased SPM content. The premise of this work is enticing. It can help to motivate further research and provide an explanation for the long term increase of water turbidity. I find the paper to be well written, language wise, and the motivation and analysis part to be comprehensive, but the analysis needs to be more quantitative. Particularly, I like the message that changing wave regimes should not be neglected in long term simulations with reference to climate change. There are some details and nit-picks that need reworking.

Introduction: The statement of SPM increase possibly exceeding 20% needs to be made cautiously. While the method of Hakanson 2006 is perhaps not ideal to show this, I am more concerned about the vague phrasing. I find no basis for it.

While we believe the original phrasing was cautious in that 20% is within the bounds shown by some empirical studies, we have now removed the 20% reference, as it was not really necessary.

The authors mention several times that tides can be assumed to be free of long-term changes, which is not exactly true, pedantically speaking. There are long period tides (see e.g. Wunsch 1967), which may be negligible directly due to their low amplitude (<1cm) but they play a role in low frequency climate oscillations. Furthermore, sea level rise has an effect on the tidal regime. It is certainly more feasible to neglect them, but then perhaps this should be mentioned.

We have now added a reference to Wunsch, and a second reference showing the potential impacts of climate change.

Methods: There is a vast amount of data used and it would be helpful to expand on the particular choices of data sources and organise it for the reader’s eyes (perhaps in a table or figure). Some data was taken from CMEMS/MetO-NWS-REAN-PHYS to determine if a water column was stratified (section 2.1), but depth-averaged veloc- ities were taken from an FVCOM model, while those same velocities are available at CMEMS as well. It would be helpful to motivate the individual data choices. There may have been easier choices for a unified data set with fewer independent sources.

A table has now been added to make it easier to understand the data used. With hindsight some easier choices could be made, however data choices were in some cases the result of what was available through certain projects., and we decided against simplifying them, given data processing methods were already in place.

Page 3, line 4 (diffusivity): as a physicist, I do understand the role of diffusivity, given that turbulent diffusivity is of course in orders similar to sinking velocities. So perhaps just add the word “turbulent” there.

The word “turbulent” has now been added to clarify the text.

The threshold of 0.5◦C appears arbitrary and needs further explaining. One could e.g. refer and compare to the definition of the mixed layer depth (MLD) used in CMEMS/MetO-NWS-REAN-PHYS or Kara et al. 2000. Alterna- tively, one could just use said CMEMS data of the MLD instead of coming up with a new one (i.e. if the MLD is smaller than the water depth, the column is stratified).

The choice of 0.5 C came from the North Sea Region Climate Change Assessment 2016, who defined stratified waters as those with prolonged periods with temperature differences between surface and seabed above 0.5 C. We believe this is a reasonable proxy for stratification for the purposes of this paper. The aim is to illustrate which months have mixed water columns, not to provide precise quantifications.

Because the ERA-interim and ERA20c are different data sets, they cover a combined period of 1990-2017. It should be made clear that there is no combined data set or otherwise how a potential integration is carried out and bias is made impossible.

The text in the methods section has now made this clearer, and we have stated that two reanalysis were used because the ERA20c reanalysis does not overlap fully with the satellite SPM.

I am unfamiliar with R, but as far as I can see, no tremendously complex statistical operations have been carried out that would require elaboration beyond textbook knowledge and I would know how to achieve the same results in MATLAB. However, it is perhaps helpful to provide some algorithms as flow diagrams in a supplement.

All of the equations used to calculate shear stress are provided in the supplementary materials of Wilson et al., 2018. We have now made this clear in the text. The actual bed shear stress code is written in C++, not R. This has also now been made clearer in the text.

Results: The results section starts off with an explanation of a seasonal climatology of near surface SPM, as well as BSS. This would be more suitable in the methods chapter.

We have now moved the SPM climatology to the methodology, with the BSS climatology still in the results. We have kept the BSS climatology in the results because we have now added a panel showing the relative contribution of waves to bed shear stress.

Figure 1 is the first of several cases where the authors say in the text that there was something to see in the figure which is actually hard to see (in this case the seasonal cycle of BSS, which is noted in the text but not well visible in the image).

We agree that this does not read very well. Because of the very large spatial variation in BSS, we have concluded that it is not possible to map seasonal BSS with the seasonal variations being particularly clear. And so we have just gone with an annual mean. The map of bed shear stress is big picture context for the main results, and is one that has appeared in various other papers. So it makes sense to stick with an annual climatology.

For figure 2, the same criticism applies as for figure 1: the text says that there is a clear positive relationship, but the figure shows dark blue colours on the left panels in several areas where the right panels show bright yellow. It needs to be made clearer what constitutes as a “clear positive relationship”, i.e. a by threshold value or something of the sort and the colour maps need to be modified accordingly. For example, in the text (page 7, line 4ff) it says that the transition to mixed water increases the link between SPM and BSS, which can be seen south west of Ireland in November, but in the figure, it is dark blue there, which indicates a weak correlation. The message that figure 2 carries could be made clearer also by an area correlation, which is more quantitative than a visual comparison.

The original text was poorly explained. We forgot to state that in tidally dominated regions waves will have little influence, so we do not expect the model to explain much. This is illustrated by the new figure 3, which approximates the relative influence of waves. The region South West of Ireland has a relatively low influence of waves. We therefore expect relatively low R^2 values in the regressions. It is notable that the R^2 values are a lot higher in this region in December and January than in November, which is attributable to higher waves.

The monthly stratification was not previously described as climatological, as it is pre- sented here in figure 2. Instead it was written in section 2.1 that the percentage of stratified water columns was taken for each month over a period of 20y. Since the analysis covers 20y and the BSS is assumed to change due to changes in wind stress, the stratification would potentially show trends as well due to changed turbulent mix- ing. Climatological stratification thus makes less sense than monthly means over 20y, unless it can be shown or motivated that the change in stratification is negligible. In the first paragraph of page 7, it says that SPM and BSS become uncoupled in strati- fied regions in summer months. However, with reference to figure 1, the authors claim a seasonality in both parameters. What is the reason for the uncoupling? Can it be explained?

The text in section 2.1 has now been clarified to make it clear we calculated a climatology.

It is true that stratification potentially changed during this time period and also during the 20th century. Analysing this was out of the scope of this paper. However, it is important to quantify this relationship. It is true that there will be complex relationships between bed shear stress and stratification due to the influence of winds. So this ought to be resolved by future work.

In general, the goal of our study was to quantify the influence of bed shear stress on sediment in the water column, not necessarily its vertical profile. This was why the linear regressions in figure 3 were carried out when waters were mixed. Moving towards accounting for stratification is something we will consider in future.

The reason for the decoupling is that variations in the depth and strength of the thermocline etc. appear to be the dominant influence on sediment reaching the surface. This is also potentially complicated by the influence of winds on both waves and stratification.

The description of the methodology for figure 3 belongs in the methods chapter (per- haps 2.4), not the figure caption, and it needs elaboration. It says “For each mapped box and month, grid points with a 20y record of SPM are selected.” Are all grid points within a box selected for which there are 20y of continuous data, or are these random choices? Were the grid points that do not have continuous data coverage neglected?

We only chose grid points with a 20 year record to improve the quality of the data being used in the regressions. The points neglected are essentially those at the northern fringe of satellite coverage. These points have low reliability and we were retiscent to use then. This was an attempt to reduce the amount of variation in the data caused by poor satellite coverage. This potentially could have been developed further. It is noticeable that the R2 of the regression models are notably lower in January than in March.

A sentence has now been added to section 2.4 to clarify this.

Why was a complete area average unfeasible?

Area averaging is problematic because for some regions it can result in temporal comparisons not being apples to apples. Roughly speaking, we are trying to estimate what would happen if, say, bed shear stress doubled in a region. However, to do this the points of comparison must be consistent. The relationship between bed shear stress and SPM varies significantly in space due to variations in sediments and bathymetry. As a result, a strict area averaging will increase the amount of noise due to poor satellite coverage and could lead to sampling bias.

Furthermore, the authors again say that there is a "clear positive relationship", which is easy to see e.g. for box 12, but not so much for e.g. box 1. Regression parameters are in the supplemented tables, but it would be much handier if they were besides the respective plots as well (at least R2).

We believe that the regression results should be interpreted carefully. A major challenge is understanding how much of the variation in SPM is caused by noise within the satellite SPM. This is particularly true for box 1, i.e. the north eastern North Sea. Our regressions show that there is a high p-value for the regressions in January, February and November, but not in March. This is potentially simply down to low light and cloud coverage creating a very noise satellite SPM record that cannot be explained in any detail by bed shear stress. Arguably some of the regressions should be removed because of this, but we believe it is better to caveat that there can be large uncertainties in the satellite SPM.

The parameters have now been moved to the main text.

Also, all boxes are of the same size, but some are only partially covered with water, some cross widely different domains, physically speaking (e.g. box 11 covering parts of the Rhine and East Anglia plume, but reaching close to the Dogger Bank, box 5 covering the Norwegian trench and thus depths from 50-300m). This may be as a minor nit-pick, but it could be argued that a more appropriate choice of boxes could have been made (e.g. as in Capuzzo et al. 2015 or O'Driscall 2014/ICES boxes).

The boxes were chosen as they could provide an estimate of the spatial variation. As with all choices they have limitations. We agree that alternatives are possible, but was unclear to us that they do not run into similar problems. For example, many of the zones used by Capuzzo et al. often cover a very broad range of shear stress, sediment and bathymetric regimes. Furthermore, our choice of boxes was motivated partly by the desire to choose regions that would have highly correlated wave climates, which makes aggregating bed shear stress over a region reasonable.

In figure 4, the changes in Secchi depth are marked as blue and red, yet ranging from +50 to +50 to positive. Red is presumably negative, i.e. a decline, so it should be -50% there.

Something went wrong when the figure was being tweaked prior to submission. We have now fixed this figure so that the legend is correct.

I really struggle with the sentence p.9, l.5. The evidence indicates an increase, more than it indicates a decline or no change (in that the plotted points are blue, and strongly so). Unless the data coverage is sufficient to make a claim, a claim should not be made. Perhaps a measure of certainty should be given (e.g. through marker size).

We believe that the original paragraph was suitably caveated, and we were clear that these results were indicative. Our key message here was that while the data show that there seems to have been a decline in water clarity across the North Sea, we cannot be sure it was universal. This was the implication of the work of Capuzzo et al. (2015), but there really isn't sufficient data to be confident of the exact spatial details of the changes.

Again for figure 4, the relationship between decline in Secchi depth and bed stress change is hard to see at first. This may be due to sparsity of data, and as the authors stated earlier, the SPM content in the areas south of 53◦N are heavily influenced by river intrusions. Hence, modifying the map by highlighting areas of high river intrusions could help clarify the link between the left panels and the right. Furthermore, a less selective method of data comparison than choosing points with 50km of each other might help here as well.

At p.9, l.8, it says that there was a significant increase in BSS across the entire shelf between 1910-1929 and 1990-2019. This is immensely confusing, because previously (figure S2), a trend of decreasing BSS was shown for the latter period, so it invokes the understanding that the two periods are investigated individually, and not against each other. Perhaps this could be clarified by rephrasing. In the same paragraph it says that the changes are driven by increased significant wave height (SWH), which could be shown in a figure, e.g. by an area correlation. Is there literature as to why the increases in SWH were so variant over space?

The clarifying phrase "between the periods" has been added to the text.

We have amended the text to make it clear that long-term spatial variation in changes in bed shear stress are not simply down to changes in significant wave height, but that the relative importance of waves in determining bed shear stress is also critical. Figure 2 now shows the ratio of wave-only bed shear stress to combined wave and tide bed shear stress. This gives an indication of the regions of tide and wave dominance. Say we have a region where 10% of stress comes from waves, and one where 50% comes from waves. If we simplify the physics and ignore interactions a doubling of wave stress in one region will result in a 10% increase in overall stress, but it would result in a 50% increase in the other. So a lot of the spatial pattern in the 20th century changes really comes down to how relatively influential waves are.

Discussion: In the first paragraph of the discussion, it is argued that there is a decline in primary production (PP), which is attributed to reduced clarity. However, figure S1 shows decreasing trends in SPM. There is a need for elaboration as to why there can be declining SPM as a main contributor to turbidity and reduced PP. The authors later provide this elaboration on page 11, but for easier understanding, the two paragraphs should be interwoven. As a side note: a large number of Secchi depth measurements are taken from near shore stations, e.g. the NIOZ facility on Texel, NL. This will heavily skew measurements in a surrounding area.

We agree. The discussion has now been modified to combine these paragraphs. We further agree with the reviewer that there is great potential for Secchi depth measurements to be skewed in the way mentioned. While some existing studies fail to account for this, we also recognize that it is often very difficult to do so.

The statement in line 13-14 is too strong and needs to either be more strongly moti- vated (quantitatively), or weakened. In line 19 on page 10, it is again said that there would be an expected increase of 20% in SPM. There needs to be a source for this claim and a direct reference as to how this claim can be made.

We have reworded the sentence to say that wave regime was a driver of the decline in water clarity.

In lines 12ff on page 11, biological activity is mentioned as a potential impact on SPM and BSS. Note that before 1950, larger areas of the North Sea had benthic flora (see e.g. Capuzzo et al. 2015), which impacts BSS heavily (and thus also tides, as to my earlier point).

Relevant text has now been added.

Minor comments: A search for typos and grammar mistakes is appropriate. My main points of criticism are with the figures, as stated above.

Page 6, line 1: “Fig. 2 shows the R2 value for the linear regressions of 8-day SPM and bed shear stress and SPM for each month between 1997 and 2017, and stratification throughout the year.” This sentence is hard to follow. Perhaps it should be “. . . the linear regressions of 8-day SPM and bed shear stress [. . .] for each month. . .”?

This sentence has been tidied up

Line 2: “The relationship between bed shear stress shows a seasonal switch.”, seems to be missing that extra “SPM” from line 2. Line 5: “. . . driven. . . ”.

A correction has been added to the text.