Responses to: Review of the manuscript "Increasing turbidity in the North Sea during the 20th century due to changing wave climate", by Wilson and Heath.

General comments

The authors use historic and current proxies for suspended particulate matter concentration (SPM) and bed shear stress (BSS). It is their goal to show that changes in both have been related since 1900. I like the general idea of the manuscript. It has potential and can advance the knowledge on the changes in the North Sea, but it needs significant improvement at this stage, especially to facilitate the understanding of what data are used and what assumptions are made. A large amount of data from many sources is downloaded. The description in the manuscript is very difficult to follow. I strongly advice a table where the following fields are shown: URL or FTP address, short description, date range, spatial and temporal resolution.

In particular, there is an inaccuracy in the satellite SPM data description. What authors are downloading is the CMEMS global optics L4 reprocessed product, which in turn is generated from Globcolour data. Therefore, a proper reference to the CMEMS web page is expected, as well as to the product QUID and PUG. For instance, here is the QUID document of the product they are using: http://resources.marine.copernicus.eu/documents/QUID/CMEMS-OC-QUID-009-030-032-033-037-081-082-083-085-086-098.pdf

A data table has now been added, and Globcolour references have been corrected. We have clarified the use of CMEMS data by providing the approprotitate links to the relevant product user manuals.

In fact, and as a side comment, I am surprised to see a paper led by a scientist from PML download a Globcolour dataset when the CCI dataset could be used instead: http://resources.marine.copernicus.eu/documents/QUID/CMEMS-OC-QUID-009-064-065-093.pdf. This product provides global marine reflectances and has been developed by PML scientists, having a much better characterization of quality and uncertainties. This product contains Rrs(670) which can safely be used as a surrogate for turbidity. Other products like particle backscattering and also newer releases can be checked outside CMEMS, in the CCI website.

The practical reason for this is that the lead author was working at the University of Strathclyde while this paper was written. The original submission erroneously did not provide a double affiliation, but this has now been fixed in the resubmission.

The use of Rrs_670 reflectance was something we considered during the analysis. However, we expect that the paper is targeted at a very broad audience, in particular marine ecologists, and we believe that using marine reflectance, and not SPM will reduce the accessibility of the paper. Furthermore, while there are some benefits to using the OCCCI data products, it is unclear to us if they are superior for our purposes. The critical issue is that we use a product that best captures temporal changes in turbidity. As far as we know there has yet to be an empirical comparison of various products in this regard.

See also a typo in the ftp address provided in the manuscript, "cems". Here, the authors downloaded the global product and resampled for the North Sea instead of directly downloading the product for the European North-West Shelf Seas, for the obvious reason that a REP product is not available for the for that region. Even though this is a complaint and authors did well, it must be stated in the manuscript as a need for service improvement.

We have now removed references to the ftp sites. CMEMS often change where data is stored, so adding an ftp that is likely to become redundant is probably a bad idea.

We are unsure that the paper is an appropriate venue for calls for service improvements for CEMS. If we do this for CEMS, we should do this for the other data providers, who sometimes have a greater need than CEMS to improve services.

The water temperature is another CMEMS product. The URL indicated needs to be specific to that product, as well as the reference to the product QUID and PUG documents.

We have now added relevant QUID and PUB documents.

Though not being a specialist in the physics of the ocean, I understand that a key point in the authors' is that BSS is caused by waves, which is caused by wind. Apparently all the physics is in another paper, but it would be good if authors did a summary of how one physical quantity determines another.

Section 2.4: Methods should be described in a comprehensive for any scientist independently of the software used. So I would prefer a description in terms of equation rather than mentioning R packages.

All of the equations used in the analysis are supplied in Wilson et al. 2018. We have now made this clearer. We believe that is more appropriate to point readers there instead of repeating the equations in new supplementary material.

The Secchi disk analysis is a weak point of the paper that undermines the analysis of the historic trends. A rigorous trend analysis must be made and the significance of such trends must be made. Also it is likely that authors missed many samples by forcing samples before and after 1905 to be close in space. Here I advise the authors to divide the North Sea in areas to cluster the Secchi disk measurements. Then the corresponding time series would be decomposed in seasonal, long-term and irregular trends using an approach like the X11. In open areas where data is expected to be more scarce but also less horizontal variability is expected, regions would be of greater size than coastal areas.

We have now moved the Secchi disk analysis to a separate figure, which shows changes by season. The aim of this analysis was to give a prelimenary, and admittedly indicative, view of the spatial changes in Secchi Disk depth. The approach taken runs into the problem that in many regions there is sparse data, and the langugage used in the original paper was perhaps not cautious enough. However, we are skeptical that alternative approaches are available. The aggregation methods of Dupont and Aksnes (2013) and Capuzzo et al. (2015) do not effectively deal with spatial sampling bias. For example, Dupont and Aksnes (2015) control for bottom depth and distance to the coast. However, bed shear stress and sediment properties are likely to be more important covariates. Given that there are huge differences in the spatial coverage in Secchi disk samples pre- and post-1950 we are concerned that the aggregation methods used in other studies do not result in partly spurious trends.

The paper as it is now seems to effectively prove a link between SPM and BSS for the satellite era (Figs. 1-3) but I cannot say the same based on the historic period (Fig. 4). Assuming the Secchi disk analysis correct, which I am not sure about, I am unable to appreciate any relationship between the left and right panels of Fig. 4, and the related discussion in Fig. 4 seems misleading. I particularly disagree with the sentence "Over the longer term, spatial patterns in changes in bed shear stress between 1910-1929 and 1990-2009 correspond with spatial patterns in differences between Secchi disc depth pre- and post-1950."

We agree the original paper overstates the ability of the reanalysis to recreate the historical trends in the original figure 4. In reality, because of the sparseness of the historical Secchi Disk depth data we cannot say with huge confidence what the spatial patterns were. However, while the big picture – North Sea water clarity declined – remains in in place, there are some indications this was not universal across the North Sea. The paper's text has now been reworded to be more cautious.

Regressions in Fig. 3 lack their statistical parameters. How were they calculated? What weight is given to outliers? What do a similar slope but different intercept mean in terms of physics?

In the original manuscipt the statistical parameters were given in the supplementary materials. These have now been moved to the main text.

The analysis left as a supplement may be of interest for the main manuscript if is accordingly treated. Here, authors seem to find a reversal of the long-term water darkening, accompanied with a corresponding decrease in BSS. As commented above for the historic period, trends have to be rigorously calculated and tested for significance.

"Our analysis shows that changes in wave energy have been a key, and probably the dominant driver of changes in water clarity in the North Sea." That is a very strong statement and I would like authors to spend some time explaining the physics behind.

This statement in the original text was probably too strong. We have now reworded it to make it clear that we have shown that changes in the wave regime caused large increases in bed shear stress. Because of the spatial sparsity of the Secchi Disk depth data, there is a lot of uncertaintity in the actual changes in Secchi Disk depth, so we should have been more cautious in interpreting the results.

The key result of the paper is that the wave changes shown by the wave reanalysis would have resulted in large reductions in water clarity. The original placement of this result side by side with the Secchi disk depth data result probably provided a misleading impression. The new figures arrangement is more reflective of the key results in the paper.

Minor comments

The page numbering restarts at every page, which I am not sure is due to journal format, but a unique numbering for the whole manuscript would help.

Page numbering restarts in each page as part of the Copernicus default template.

The words "disc" and "disk" are found in the manuscript. Authors might unify the grammar choice.

This has now been changed to "disk" throughout.

Correct "Capuzz"

This has now been corrected.

The reference Jafar-Sidik et al. (2017) is not found in the reference list.

This has now been added.

Page 6, line 5: "drive" should be "driven"

This has been corrected.

Page 2, line 6: “SMP”

This has been corrected to “SPM”.

Page 9, line 5: replace “are” with “is”

This has been corrected.