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

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.

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.

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.

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.

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.

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."

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?

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.

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.

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

Correct "Capuzz"

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

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

Page 2, line 6: "SMP"

Page 9, line 5: replace "are" with "is"