Reply to RC1

Reviewer 1 attests that this study "is the first published, rigorous and statistically elaborate examination of C stocks and burial rates in the entire shelf sea environment. What is new is the estimates of errors, and this is relevant as an incentive for further dedicated ground-truthing work."

General comments:

Comment: How will this carbon sequestration be partitioned into national shares and accounted for? Will the credits go to the country that provides the storage space, or to the countries that provide nutrients for production of biological material? These accounting questions are clearly not at the focus here, but are highly relevant and offer potential for conflict. They should at least be pointed out.

Reply: As stated by the reviewer, the above questions are not the focus of our study. However, we briefly mention potential implications in the introduction now.

Action: The introduction was updated including a reference to a recent paper on that topic:

[…] Consequently, the question has been raised whether those stocks should be considered as part of national carbon accounting and potential greenhouse gas mitigation projects and subject to management against human-induced disturbance (Avelar et al., 2017). The socio-economic importance of marine carbon storage has recently been assessed in a scenario analysis of increased human and climate pressures over a 25-year period. It was estimated that damage costs of up to $12.5 billion from carbon release linked to disturbance of coastal and shelf sea sediment carbon stores could arise in the United Kingdom (Luisetti et al., 2019). *However, the transboundary nature of carbon flows in the marine environment poses significant challenges for carbon accounting and requires new guidance and governance frameworks to manage these stocks (Luisetti et al., 2020).*

Specific comments:

Comment: Why have only data from the UK and Norway been analysed, although there must be significant stores of data from other countries? Have the authors checked the OSPAR data bases, or PANGEA for easily accessible data sets?

Reply: The data on sedimentation rates were mainly sourced from the EMODnet-Geology data portal. We are confident that this is the most comprehensive collection of measured sedimentation rates for European seas. Any geographic bias is due to the locations of sediment accumulation basins, such as the Norwegian Trough. To address this bias, data from de Haas et al. (1997) were also included to cover areas of low or no net sedimentation. These were collected on a widely spaced grid across the entire North Sea. Regarding organic carbon (OC), we decided to predict OC densities (kg m^{-3}) instead of OC contents (%). As pointed out in section 3.1.2, this has two advantages from a methodological point of view: First, there is no need to transform the response variable as would be necessary in the case of OC contents reported as weight-% or fractions. Second, only one model instead of two needs to be fitted. Especially the second point is relevant here, as fitting two models (one for OC and one for dry bulk density or porosity) would likely increase the uncertainty of the predictions. However, this means that only data sets that report OC contents together with dry bulk density/porosity could be utilised. The final data were carefully selected after screening various data bases and other sources including PANGAEA and ICES.

Action: Section 3.1.2 was rewritten:

Previous studies have predicted OC concentrations and sediment porosity separately to calculate OC stocks (Diesing et al., 2017; Lee et al., 2019; Wilson et al., 2018). Here, we first calculate OC density from concurrent measurements of OC concentrations and sediment dry bulk densities or porosities. This has two advantages: First, there is no need to transform the response variable as would be necessary in the case of OC concentrations reported as weight-% or fractions. Second, only one model instead of two needs to be fitted. *This is advantageous as fitting two models would likely increase the uncertainty of the predictions. Initially, a wide range of data sources were accessed. Ultimately, 373 samples fulfilled the criterion of providing OC content and dry bulk density/porosity measured on the same sample. These samples were collected and measured by the Geological Survey of Norway, the Centre for Environment, Fisheries and Aquaculture Science, Bakker and Helder (1993) and de Haas et al. (1997). The full dataset used for subsequent modelling is shown in Figure 2 and provided as Supplementary Data Table 2.*

Comment: To fill data gaps, the authors employed a technique of placing “pseudo observations” (Hengl et al., 2017 are cited as a reference). Did the authors just say: ”We need a data point here – let0s make it 0.1 wt.% organic carbon and 0 cm/yr sedimentation rate”? This needs to be explained in more detail.

Reply: We believe that it is clear from the manuscript that pseudo-observations were only used in the case of sedimentation rates. No pseudo-observations were employed to model OC density. As explained in section 3.1.1, it was necessary to place pseudo-observations when modelling sedimentation rates due to a strong bias of samples towards accumulation areas. However, large areas of the North Sea are non-depositional or erosional in nature and only those samples from de Haas et al. (1997), less than 20 in total, were taken in these areas. We therefore resorted to pseudo-observations, a practice that has been applied in the past (Hengl et al., 2017). Pseudo-observations were only placed in areas where we were confident that sedimentation rates were 0 cm/yr. The placement was conducted in a random way to avoid human bias. We understand that this method might sound arbitrary at first and that many marine scientists might not be familiar with the concept. As suggested by the reviewer, we are now giving more details in section 3.1.1.

Action: Section 3.1.1 was amended to give a more detailed explanation on pseudo-observations:

The reported sedimentation rate data focussed on accumulation areas like the Norwegian Trough (Figure 2). However, to be able to spatially predict sedimentation rates across the study site it is necessary to include data from areas of erosion and non-deposition, which predominate in the North Sea. Therefore, the data of de Haas et al. (1997) were also included. This provided less than 20 data points of zero net-sedimentation, which was still deemed insufficient. Additionally, pseudo-observations (Hengl et al., 2017) were also included. *Pseudo-observations are ‘virtual’ samples that are placed in undersampled areas and for which the value of the response variable can be assumed with high certainty. Hengl et al. (2017) cite 0 % soil OC in the top 2 m of active sand dunes as an example. Mitchell et al. (in review) placed pseudo-samples in areas of bedrock outcropping at the seabed when predicting sedimentation rates in the Baltic Sea. The placement of pseudo-observations was restricted to* areas of erosion and non-deposition (based on the sedimentary environment layer, as described in chapter 3.2)*, for which* ~~and~~ a sedimentation rate of 0 cm yr^{-1} could be assumed. *The pseudo-observations were placed randomly to avoid human bias.* […]

Comment: Although not explicitly stated, the most important predictors emerging from the analysis are rather intuitive and straightforward (judging from Figure 6): bathymetry, oxygen penetration depth, and energy at the sea floor. Also, OC density and accumulation rates are included as predictors – but aren't these two the parameters to be estimated?

Reply: We are sorry for the confusion, but the parameters in Figure 6 are not the predictors. Instead, Figure 6 shows the variables that were employed in the unsupervised classification (section 4.4). We did not state explicitly in the text, which predictor variables were chosen for modelling. Instead this information can only be found in the Supplements S1 and S2.

Action: To avoid confusion, we now provide information on the selected predictors in the results section.

Comment: The physical predictors are but a few of a long list of parameters that have gone into the analysis (in Table 1). Of these important (there is no measure of statistical impact given for any of them) predictors, bathymetry and energy at the seafloor are not independent (orbital velocities), and the way that oxygen penetration depths are calculated (as a function of mud content) makes one wonder, if they are not linked to bathymetry and energy at the seafloor as well. Instead, one would have expected that OC accumulation and OC standing stock are linked to grain size, for which spatially explicit data sets are available (e.g., Bockelmann et al., 2017) – was there no statistic relationship?

Reply: Again, the information on predictor variable importance is "hidden" in the supplements. We did not intend to discuss the importance of predictors in this manuscript, as a better understanding of the links between predictor and response variables was not the focus of the work. However, it seems that it might be prudent to include such information in the main text. Also, we did not model OC accumulation and OC standing stock; both were calculated from OC densities (see sections 4.2 and 4.3). The OC density model did include mud content as an important predictor, although mud was maybe less important than could have been expected from previous work (e.g. Diesing et al., 2017). This might be attributable to the fact that we predicted OC densities rather than OC content. Muds typically have high OC content but also low dry bulk densities. Consequently, it can be expected that the relationship between OC density and mud content is weaker than between OC content and mud content.

Action: We now include a figure showing the selected predictors and their importance scores.

Comment: If oxygen penetration depth is indeed a function of mud content, why is not mud content the logical predictor? What is the depth of oxygen penetration calculated by the empirical formula used here in the first place? This important piece of information is shown in Fig. 6, but eyeballing the box-and-whisker plot for the turnover zone in the figure and the profiles given in Lohse et al. (1996) suggest to me that the formula used here overestimates the oxygen penetration depth. This will not make much difference in the overall conclusion that not much carbon is stored anywhere except the Norwegian Trough and Skagerrak, but then oxygen penetration depth is probably not a good predictor of OCAR and OC density, because animals influence that depth. There are much more elaborate (model-based) and spatially contiguous estimates for oxygen penetration depths available at least in the southern North Sea (e.g., Luff and Moll, 2004; Pätsch et al., 2018, and probably more recent ones as well), and it might be advisable to use such data instead of the (apparently upublished) empirical relationships used that may be only regionally applicable.

Reply: Both mud content and oxygen penetration depth were selected as predictors in the OC density model. All predictors, including oxygen penetration depth, are now included in additional figures. This will give a better overview of the spatial variation of predictors, including oxygen penetration depth. We assume the reviewer is referring to Lohse et al. (1996). In their Fig. 4, oxygen penetration depths > 4 cm were measured on Dogger Bank and of ≈ 2.7 cm off Esbjerg. Painting et al. (2013) report measured mean oxygen penetration depths of 4.8 ± 2.4 cm in the Southern Bight. Likewise, Hicks et al. (2017) report an oxygen penetration depth of 4.6 cm at a permeable sediment site in the Celtic Sea. Taken together, this leads us to believe that our values of up to 2.9 cm in the turnover zone are reasonable and no overestimates. Using the suggested study results (Luff and Moll, 2004; Pätsch et al., 2018) as input data is problematic because they might not cover the whole extent of the study area (as mentioned by the reviewer) and outputs might not be freely available (based on experience), but this is difficult to ascertain without complete references. We therefore believe that using oxygen penetration depth estimated from mud content is the preferred option under the given circumstances.

Action: We now include additional figures showing the selected predictor variables.

Comment: Finally, Zhang et al. (2019) highlight the important role of macrozoobenthos in recycling (and preserving) organic carbon at the sediment-water interface in much of the southern North Sea, an aspect not addressed in the present manuscript.

Reply: We assume the reviewer is referring to Zhang et al. (2019). This is certainly an interesting paper dealing with related aspects of OC modulation by macrobenthos.

Action: We have updated the text in section 6.2 to briefly mention the importance of bioturbation for OC preservation.

Conversely, the seabed of the Norwegian Trough is characterised by water depths in excess of 200 m and experiences very subdued wave and current agitation. Fluid transport in the sediment is therefore driven by molecular diffusion, mediated by bioturbation. *Bioturbation contributes to a balance in the sedimentary OC budget by transporting labile OC to deeper horizons where degradation efficiency is lower (Zhang et al., 2019).* Lack of advective oxidation (Huettel et al., 2014; Huettel and Rusch, 2000) combines with mineral protection (Hedges and Keil, 1995; Hemingway et al., 2019; Keil and Hedges, 1993; Mayer, 1994) and short oxygen exposure times (Hartnett et al., 1998) due to shallow oxygen penetration depths and relatively high sedimentation rates. Collectively, this leads to high OC densities and accumulation rates. This zone might be termed burial zone according to Huettel and Rusch (2000).

Comment: An important issue of course is the origin of OC, stated as an open question in the suggestions for future research. In general, the discussion on why some parts of the sea floor do and others do not accumulate organic carbon (section 6.2) implies that in-situ production and processing dictate OC density and OCAR. Resuspension and transport, and associated exposure to progressive degradation are not discussed. But why then do the hydrodynamic predictors (M2 current and orbital velocity) apparently have such a strong influence? Fine-grained sediment and associated OC buried in the Skagerrak and Norwegian Trough comprise input from rivers, atmospheric input, coastal and sea floor erosion, and primary production that feed into suspended and bed load transport of the North Sea. There are data sets on C/N ratios and even delta13C (see for example the thesis by de Haas, Geologica Ultraiectina 155, 1997), which indicate that about 20% is of terrigenous origin, and an unspecified source is erosion of older strata. Mineralisation in these muds only affects that (relatively small) portion supplied by production in the surface layer (see papers on oxygen consumption measurements by Lohse, Helder, Rysgaard etc.,).

Reply: Hydrodynamics were identified as relevant predictors; however, other predictors were more important. These were, in decreasing order of importance, bathymetry, sedimentation rate, mean bottom water temperature, oxygen exposure time and mud content. These are in good agreement with the expectations, as stated in section 3.2. It should be noted that Figure 6 was derived by an unsupervised classification approach (section 4.4). To carry out this regionalisation of the North Sea regarding processing of OC at the seafloor we chose the six variables displayed in Figure 6. Hydrodynamics (current speed and wave orbital velocity) were included as they play a crucial role in the rapid degradation of OC in permeable sediments (Huettel et al., 2014). Accumulative areas, on the other hand, are characterised by weak hydrodynamics, which favours the deposition of fine-grained sediments. Porewater transport is diffusive and hence much slower than in permeable sediments with advective transport. Together with mineral protection and short oxygen exposure times after sedimentation, this leads to increased OC densities and accumulation rates. We concede that the aspect of resuspension and transport of OC prior to deposition in the main depocentre, the Norwegian Trough, has been left out in our discussion of zones of OC processing (section 6.2.). This has now been rectified. We agree that it would be desirable to address the question of the origin and sources of OC deposited in the North Sea and Skagerrak. We think, however, that a detailed re-analysis of C/N ratios and $\delta^{13}C$ values is beyond the scope of this study. Also, de Haas (1997) states in his thesis: "Variations in $\delta^{13}C_{org}$ in Norwegian Channel sediments cannot be used to explain variations in C_{org} contents as a result of differences in source of the organic matter." Unfortunately, we were unable to ascertain which publications from "Lohse, Helder, Rysgaard etc." the reviewer was referring to.

Action: The discussion of the "burial zone" in section 6.2 was updated to reflect the origin of the OC deposited there:

De Haas and van Weering (1997) *estimated that only 10 % of the OC deposited in the Norwegian Trough is derived from local primary production and the remainder originates from other sources. A large part of this allochthonous OC is transported into the Norwegian Trough along the Dutch, German and Danish coasts by an anti-clockwise residual circulation* (de Haas et al., 2002)*. This transport is thought to be intermittent, with the rate of transport dependent on the strength of wind-induced waves and currents (de Haas and van Weering, 1997). The OC being deposited in the Norwegian Trough is mostly refractory, as it has undergone several erosion-transport-deposition cycles prior to final deposition (de Haas et al., 2002).*

Comment: Finally, the manuscripts advocates and discusses at some length whether marine protected areas should be established to prevent accumulated organic carbon from being resuspended and remineralised. The manuscript cites an astounding estimate of potential damages arising from mineralisation caused by demersal fishing for the UK as support. Assuming that physical disturbance by demersal fishing enhances sedimentary carbon recycling, the logical sites for such MPAs and fisheries exclusion zones will be the accumulation areas – Skagerrak and Norwegian Trough. It would be interesting to know what the swept area ratio in these deep-water environments actually is and whether there have been monitoring activities (underwater video) in the fisheries sector to establish the extent of sediment reworking there. From the data on composition of OC in the depocenters, de Haas (1997; thesis) concludes that this OC is very recalcitrant – will it be further mineralised at all? Bakker and Helder (1993) showed that oxygen fluxes in the Skagerrak were not related to the total organic carbon (TOC) content of the sediments, and that apparently only fresh organic carbon from sinking primary production was mineralised. This suggests that the old carbon derived from lateral transport across the large submarine catchment is unreactive. An interesting ancillary question is, whether (and if so, how much) a decline in (postulated) remineralisation from artificial disturbance will negatively feed back on primary production and thus will reduce the amount of carbon supplied for burial.

Reply: A spatial comparison of OC densities and the swept area ratio (or other metrics relating to bottom-contact fishing) would indeed be of interest but certainly beyond the scope of this study. We expect that such an analysis would have to include more than simply overlaying OC stocks with swept area ratios. Besides, there is the complication that current OC stocks most likely have already been affected by bottom contact fishing, which complicates the analysis. Our study was also not designed to answer questions on the reactivity of the deposited OC, but we acknowledge that such questions would be relevant in the context of "carbon protection zones".

Action: We briefly discuss reactivity of OC in the Norwegian Trough at the end of section 6.3 now:

Although more research is needed, it is becoming clearer now that seabed disturbance by demersal fishing leads to increased OC mineralisation in cohesive sediments in the short-term (van de Velde et al., 2018) and a general impoverishment in OC in the long-term (Martín et al., 2014). Protecting regional hotspots of OC accumulation from fishing-induced disturbance might therefore be a suitable measure to increase the climate mitigation potential of the seabed. Likely sites that might benefit from protection are to be found in the burial zone (i.e. the Norwegian Trough), while it is unlikely that the turnover zone yields any potential areas worth protecting in this context. Our results could be used jointly with maps showing the footprint of demersal fishing (Eigaard et al., 2016) and other resources to identify potential sites for the establishment of "carbon protection zones". Such management measures that limit the impacted surface area, allowing carbon stocks and faunal communities in the sediment to recover from a disturbance, and resulting in the recovery of carbon burial, might be preferable over technical modifications that reduce the penetration depth of fishing gear (De Borger et al., 2020). *However, such analyses must consider that the OC stocks, as mapped in this study, likely have been affected already by decades of demersal fishing. Our maps therefore do not represent a baseline in a sense of an undisturbed state.*

Additionally, more research on the reactivity of OC is required to better understand the relationships between OC mineralisation and seabed disturbance. The mineralisation of predominantly refractory OC caused by demersal fishing might be limited or even negligible. In the Skagerrak, oxygen microprofile measurements indicated that mineralisation rates were independent of OC content, but related to the input of fresh OC by primary production (Bakker and Helder, 1993). This suggests that preferentially fresh labile OC was mineralised, while allochthonous OC that accounts for 90% of the OC in the Norwegian Trough (de Haas and van Weering, 1997) might be largely unreactive. Conversely, van de Velde et al. (2018) suggested that OC mineralisation is stimulated after sediment disturbance, likely due to the enhanced decomposition of previously buried refractory OC when it comes into contact with labile OC, a process known as priming (Steen et al., 2016). *Another question of interest is to what extent a potential reduction in mineralisation rates due to areal protection of OC stocks might influence primary production and thus supply of OC to the seabed.*

#75: this is only a temporary uptake

Reply: Agreed, but recovery of benthic species due to spatial protection will likely increase the biomass. If protection ensures higher levels of biomass (compared to the impacted state) in the long-term, this will contribute to carbon drawdown.

Action: The sentence was slightly altered to promote clarity:

Establishment of MPAs protecting against demersal fishing could not only facilitate the recovery of benthic species but also promote *longer-term* carbon uptake by seabed ecosystems *through increased biomass*, as well as prevent further loss of OC stored in sediments (Roberts et al., 2017).

106: erosion is an unquantified term and there is reason to believe that a substantial fraction of exhumed OC accumulates in recent depocenters

Reply: It might indeed be preferable to view erosion as the inverse of sedimentation and predict erosion/sedimentation as one variable. However, there is even less information on erosion rates, which renders this approach as currently unfeasible.

Action: No action taken.

#122: which grain density was assumed?

Reply: 2650 kg m^{-3}.

Action: The text was updated to include this information.

#136: water depth = distance to shore? Sedimentation rate according to Müller and Suess is relevant only for deep-sea sediments and vertical transport

Reply: There is a difference between water depth, which is the vertical distance between the seabed and sea level and distance to shore, which is the horizontal distance between a location and the nearest shoreline. Sedimentation rates might be more relevant in deep sea environments. However, at this stage of the 'conceptual' model building it might be wise to include more rather than less variables that might potentially be relevant. Unimportant variables are subsequently identified through an additional step with the Boruta variable selection wrapper (section 4.1).

Action: The first paragraph of section 3.2 was updated:

The initial selection of environmental predictor variables was based on availability and *potential* relevance. *At this initial stage of conceptual model building* (Guisan and Zimmermann, 2000)*, it might be prudent to include a wide range of potentially relevant variables. A selection of variables that are actually relevant for the model will be performed subsequently.*

#140 ff: What use are geomorphology features beyond water depth and orbital velocities? Why not use numerical values for grain sizes (Bockelmann et al., 2017)?

Reply: As per previous reply, it is advisable to "cast the net widely" initially. Regarding grain-size data, we included data on sediment fractions (gravel, sand, and mud content) as mentioned in Table 1.

Action: None.

#145: There are no dated cores in several of these areas (judging from a comparison of Figures 1 and 2)

Reply: For clarity, this sentence was reworded (also to address a comment by reviewer 2).

Action: The sentence reads now:

These potential accumulation areas were critically reviewed in the light of measured sedimentation rates and geological interpretation of sediment cores (de Haas et al., 1997 and references therein).

#148: LSR is 0 in most of the area, and besides the oxygen exposure time appears to be not relevant.

Reply: As mentioned before, it might be preferable to include more potential predictors initially.

Action: None.

#171: Which are these? Include information of statistical weights/relevance as predictor in Table 1

Reply: Information on selected predictors was 'hidden' in the Supplements. This has now brought into the main document.

Action: Information on selected predictor variables has been added to the results section.

#216 ff: Are these the environmental variables explaining most of the variance? O2 penetration depth was estimated by mud content - is that independent of OC density? Table 1 lists many predictor variables, most of which apparently are not crucial. On the other hand, sedimentation rate is a crucial parameters, but how are grain size/Folk parameters etc. linked to sedimentation rate?

Reply: No, as per previous comment it should now be clearer which environmental predictors were selected. The six environmental variables mentioned in section 4.4, which were used as input for the regionalisation, were selected based on their expected impact on OC processing.

Action: None.

#221: Show loadings in a figure or table

Reply: The R Notebook script was updated to provide loadings. However, we would not deem this information so important as to provide it in the main document.

Action: Updated Supplement S4 with loadings.

#243: The mode of transport by OC spiraling is important here - after what time does the material from production sites arrive at the depocenter, and how is the material reworked on the way?

Reply: Agreed, but we suggest discussing this in section 6.2, which seems better placed than in the results section. The reviewer already raised the question about the origin of the OC in the Norwegian Trough as a general comment and we have provided additional information.

Action: See above.

#247: Are these parameters the dominant discriminators?

Reply: No, if the reviewer is referring to the selected predictors. As outlined in section 4.4., these parameters were chosen as input data for the regionalisation due to their expected strong impact on OC processing.

Action: None.

#291 ff: This raises the interesting question on who reports -producer or storage provider?

Reply: Agreed, although this might be beyond the scope of this study.

Action: We added a sentence and refer to a recent publication that deals with this issue.

This would, however, require new accounting guidance and governance frameworks, as carbon removal from the atmosphere and OC accumulation in seabed sediments might occur in different jurisdictions, with the North Sea cited as a prime example (Luisetti et al., 2020).

#297: The difference is that OC in fjord sediments probably is autochthonous (or from land), whereas Norwegian Trough collects OC from a large submarine area

Reply: Faust and Knies (2019) show that the proportion of marine vs terrestrial OC varies greatly between fjords. We therefore think that such a generalisation should not be made.

Action: None.

#312: I assume that these characteristic parameters are the ones shown in Figure 6 (bathymetry, oxygen penetration depth derived from mud content, energy at the sea floor from M2 and orbital velocity)? See comments above.

Reply: Yes, these are the parameters shown in Figure 6, as the regionalisation was based on these.

Action: We added a reference to Fig. 6 to the first sentence of section 6.2.

The regionalisation based on selected characteristic parameters pertaining to OC accumulation *(Figure 6)* and storage has shown that the North Sea and Skagerrak can be divided into distinct zones.

#393: There is always room for improving the data base and suggestions are certainly valid. But when most samples are sands with <0.2 wt.% OC, a standardisation of methods (which effectively has been done in the past on various occasions for the analytical steps) will not change the general conclusions. In the de Haas thesis available on the internet (Mededelingen van de Faculteit Aardwetenschappen Universiteit Utrecht No. 155: Transport, preservation and accumulation of organic carbon in the North Sea), there is information on the delta 13C of OC and C/N ratios.

Reply: We agree that a standardisation of methods has been done in the past, otherwise this study would not have been possible. However, such standardisations were probably not being made with OC budgeting in mind. For example, OC contents have been measured at sediment slices of various sizes, while in terrestrial soil mapping standard depths are used (Hengl et al., 2014). The de Haas thesis does indeed include $\delta^{13}C$ and C/N values, however not in a format that could be utilised (i.e. a table).

Action: None.

#395: Again - who is credited for sequestration of allochthonous carbon in the Norwegian trench? Those who produce the carbon (in their EEZ's), or those who store it in their EEZ's? Productivity is highest in the southern, non-depositional sectors of the North Sea. The best way to enhance sequestration potential is to increase productivity – probably by enhancing eutrophication.

Reply: This point was already addressed twice previously, in the introduction and the discussion. We believe that this should suffice, given that this aspect, despite its future relevance, is not the main topic of this study (as mentioned by the reviewer).

Action: None.

Table 1: Add a measure of statistical weight

Reply: We assume the reviewer is referring to the variable importance measures. This could be done, but we prefer adding this information to the results section.

Action: We now include a figure showing the selected predictors and their importance scores.

Table 2: Reference needed in the caption

Reply: Agreed.

Action: Reference added (Hengl et al., 2017)

Fig. 1: The upper bound of the color scale (51 m) appears to wrong. How were depositional areas selected? What is the diagonal white line from Denmark to the southern tip of Norway?

Reply: Although it might seem wrong, the upper bound is correct. This is due to the relatively large pixel size of the bathymetry grid (500 m). Some 'coastal' pixels will include terrestrial areas and the averaging will have led to positive values. We explain in the text how the depositional areas were derived. The white line between Norway and Denmark constitutes the boundary between the North Sea and the Skagerrak.

Action: If preferable, we could ‘force’ the upper bound of the colour scale to 0 and remove the white line. Additional text was added to the caption to point to the text where the delineation of depositional areas is explained:

Refer to chapter 3.2 for the delineation of areas of sediment deposition.

Fig. 6: Are the parameters presented here as box-and-whisker plots the ones that are dominant, or why were they chosen (see comment above)?

Reply: No. This has been explained previously.

Action: None.

References

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