

Comments on egusphere-2026-108 entitled as “**Spatial heterogeneity of sedimentary organic carbon in fjords around Stavanger, Norway – implications for upscaling**” submitted by Diesing et al.

### **General comments**

This study presented detailed quantifications on sedimentary organic carbon (OC) in Norway fjords. The authors found sedimentary OC is highly heterogeneous, resulting in large uncertainties when propagating local results to regional and global budgets. Finally, the authors concluded upscaling procedures to achieve more reliable results. This work makes sense because recently there are more and more attention on global and regional sedimentary OC budgets. However, I found a few concerns when going through the manuscript, mainly regarding the organization of some parts, a few statements, and unclear expressions. For the current version, a lot of effort has been devoted to summarizing previous work. I understand why the authors do that but it may not be suitable in a case study. It would be better to focus on the regional dataset in this work while introducing unbiased upscaling protocols. My major comments are provided below.

**Reply: Thank you for the assessment. We understand that the main criticism is that a regional study cannot be directly compared with global studies. While we had reasons for doing so, we agree that it might be more appropriate to provide our results as a regional case study, discuss the results in the context of other northwest European mid-latitude fjord systems and carry out an upscaling of organic carbon burial rates to the site and national levels. Finally, it might still be useful to reflect on the implications of our findings for global upscaling studies.**

### **Abstract**

This section started with introduction to the importance of fjords in the global carbon cycle, followed by possible biases, and then, this study to test previous hypothesis. The most significant issue is that direct comparisons between this study and previous global-scale studies may not make sense as they are at different scales (it is not likely to test global hypotheses by regional studies). I would say these hypotheses can only be tested by re-evaluation of global budgets using new methods, instead of regional case studies. It would be better to restructure this section and other sections or provide more convincing evidence on how to test these hypotheses by this study, as the current expressions are not specific.

**Reply: We acknowledge that a direct comparison of results from a regional study with global studies might not be fully feasible. Our main point was, however, that environmental variability appears to be larger in the Stavanger region than in the**

**global datasets, which seems counter-intuitive. Although not a strict mathematical law, it's probably correct to say that the range of values increases with the size of the dataset and the extremes (minimum and maximum) get more extreme.**

**Action: We have made changes to the abstract. In particular, we removed the half sentence about thermally labile organic carbon of global fjords (since we cannot compare our results with the previously quoted numbers due to different methods used) and we are no longer stating that the purpose of the study was to test assumptions that are implicit to global fjord studies.**

## **Introduction**

This section listed a lot but different parts are not well integrated (and also may not be closely linked to results and discussion). For example, OC reactivity was introduced here; however, there was very limited discussion on it in the following sections. Another issue is there were a lot of things that the authors wanted to state in this manuscript, but only with insufficient and indirect evidence. A lot of revisions are needed to reorganize the Introduction section as well as other sections.

**Reply: We will provide a revised introduction in line with the new focus of the manuscript as outlined above.**

**Action: The introduction has been substantially altered to better focus on the objectives of our study, i.e., (1) to characterise the spatial heterogeneity in the study site with regard to substrate type, depositional environment and sedimentary organic carbon and (2) to estimate organic carbon accumulation.**

## **Methods & Results**

It's ok. Please refer to line comments.

## **Discussion**

A lot of effort has been devoted to previous studies (especially in Section 4.3). No need to repeat the findings from previous studies; it is better to focus more on this study and highlight the differences from previous work.

**Reply: Agreed.**

**Action: Section 4.3 has been completely rewritten. We now upscale organic carbon accumulation rates from cores to the study site level. We also illustrate how upscaling using the implicit assumptions of Smith et al. (2015) will likely lead to overestimations of organic carbon accumulation.**

Another possible issue is the thermochemical approach used in this study. Direct comparisons between TGA and other thermochemical decomposition method (e.g., Rock-Eval, Ramped pyrolysis/oxidation) must be cautioned. The methodological definitions of “labile” OC in this study and previous studies are completely different. The methodological differences can lead to biased estimate of OC reactivity. Generally, during TGA, the weight loss from water vapor and inorganic elements can lead to overestimation of OC decomposition. I would therefore not recommend comparing TGA dataset to other datasets (e.g., Ramped pyrolysis/oxidation); but it’s ok to compare two different TGA datasets in different studies given the same definition. Discussion about OC reactivity is also seemingly separate from other parts, which should be streamlined or better integrated with other proxies and results.

**Reply: Agreed, that results from different methods might not be directly comparable. We will take this into account and will better integrate our OC reactivity results.**

**Action: While we show our results on organic matter reactivity using the CRI, we do not compare these any longer with results from the global study of Cui et al. (2022), which is using a different approach.**

My other line comments are listed in the following part.

**L14.** I would say these hypotheses cannot be completely tested, at least in this study, by regional evaluations.

**Reply: We acknowledge that this might not be completely possible.**

**Action: We have deleted the respective half sentence.**

**L19-22.** Indeed, in many fjords there exist significant land-to-ocean gradients, as expressed by carbon isotopes or reactivity. It is better not to confuse two concepts - the uncertainties of OC accumulation rates and lateral gradients of these proxies.

**Reply: We agree that gradients and environmental variability should not be confused. However, our main point was that the environmental variability we see in the Stavanger area appears to be larger than that of global fjord studies, something that one would not expect.**

**Action: As we are reporting results from our case study, we do not see a need to alter this sentence.**

**L22.** How can global assumptions be tested by regional case study? It is better to display more detailed evidence here, instead of only showing the data.

**Reply: We agree that it would be difficult to test such global assumptions. However, it is still possible to point out shortcomings of global studies such as a poorly constrained global fjord area, a likely bias of sampling sites towards areas that have high accumulation rates and the implicit assumption that fjords accumulate sediment and organic carbon over the whole fjord area.**

**Action: We left this sentence unchanged as we think our findings from a local case study add to a growing understanding of fjords being spatially heterogeneous environments, which has important implications for global upscaling.**

**L36.** It is better to state the range of OC burial efficiencies and indicate the representativeness.

**Reply: Agreed.**

**Action: The sentence has been removed as part of a revision of the introduction.**

**L38-44.** This part is like a literature review. The purpose of putting these data here is unclear.

**Reply: As mentioned earlier, we will rewrite the introduction.**

**Action: The introduction has been rewritten to better focus on the objectives of the study.**

**L45-51.** The current structure of Introduction is too lengthy. It has jumped a lot of times from global paradigm to regional studies and then to global fjords again. All these statements can be reorganized and streamlined.

**Reply: As mentioned earlier, we will rewrite the introduction.**

**Action: The introduction has been rewritten and shortened.**

**L52.** Only a minor part of this manuscript is about OC reactivity and it seems separate from others (e.g., OC accumulation rates).

**Reply: We would argue that organic carbon reactivity is an integral part of this study (ch. 3.6, 4.1 and 4.2)**

**Action: None**

**L65.** What global estimates?

**Reply: Organic carbon accumulation rates, reactivities and sources. But as mentioned earlier, we will rewrite the introduction.**

**Action: This sentence has been removed as part of the revision of the introduction.**

**L127.** What is the method used to determine OC content? Combustion or other methods? Whether samples were acidified or unacidified should be clarified.

**Reply: OC content is determined by combustion after acidification with diluted hydrochloric acid. We will update the method description.**

**Action: The sentence has been updated accordingly.**

**L130.** I wonder whether these estimates would be affected by local sedimentation rates or bioturbation. Under settings of high or low sedimentation rates, how reliable are these results. It is better to display sedimentation rates measured in Norway fjords.

**Reply: This part is about stocks and not accumulation rates.**

**Action: None**

**L144.** Is the decomposition (and loss) of other elements (e.g., N) insignificant during ramping process? The range of 200-650 °C may be insufficient to capture the decomposition of all sedimentary OC, especially under N<sub>2</sub> atmosphere (with possible charring).

**Reply: The CRI is a measure of thermal reactivity of organic matter as stated in L. 134. As such other elements might be included. The thermal ranges have been defined by Smeaton & Austin (2022) based on previous studies (Capel et al., 2006). Focusing on the 200-650°C temperature range removes interference from absorbed water and non-organic material (i.e., calcite).**

**Action: None**

**L147.** I recommend keeping consistent when using  $\delta^{13}\text{C}$ .

**Reply: Agreed.**

**Action: We have applied a consistent use throughout the manuscript.**

**L198.** This adjustment is basically reasonable, yet bedrock can contain substantial OC and is dependent on its type (fine-grained bedrock like shales is rich in OC). Just a note.

**Reply:** Good point, but that would be fossil OC, which is not the focus of the paper.

**Action:** None

**L209.** It is recommended to clarify the normalization method (e.g., min-max or Z-score) throughout the manuscript.

**Reply:** The raster layers were centred by subtracting the mean of the raster layer from each pixel value and normalised by dividing the pixel value by the standard deviation of the raster layer. We will provide a more detailed account on the methods used.

**Action:** The text has been updated accordingly.

**L257-259.** How did you ensure observed bioturbation do not significantly bias the estimates?

**Reply:** We did not ensure this; however, based on the reports we received from the dating laboratory we identified which cores showed signs of sediment mixing.

**Action:** We added a sentence for more clarity: To minimise the risk of sediment mixing, sediment cores for dating were only collected from the area mapped as deposition from suspension.

**L273-275.** What criteria were used to select these variables? I mean, OC stocks may be mathematically correlated with these variables, but without any realistic relationship. For example, how would bottom current velocity be related to OC stocks? More explanations or clarifications are needed.

**Reply:** There are two steps involved in the selection of variables. In the first step, potentially important predictors are identified and collated as gridded data covering the area of interest with full coverage. In a second step, we use a data-driven approach to find the combination of predictors that gives the best model performance. This is achieved with the forward feature selection function of the R package CAST (l. 185). It is also noteworthy that we are using an empirical model and not a mechanistic one.

**Regarding bottom current velocities:** These have turned out to be an important predictor in previous studies (Diesing et al., 2017, 2021). Although predictors might not necessarily have a causal relationship with the response variable, this can sometimes be made plausible. In this case, current velocities influence the grain size composition of the sediments, and it is well known that both organic carbon content and dry bulk density, and hence also stocks, are related to the sediment composition.

**Action:** Following a comment from Reviewer 2, we updated the section on modelling and spatial prediction for more clarity. This now explains in more detail how the predictor variables were selected and that we are not using a mechanistic model.

**L291-292.** How did these variables relate to OC reactivity? Actually, bedrock was assumed before to contain no OC.

**Reply:** This is meant to be the fraction of the seafloor that is occupied by bedrock. We will correct this.

**Action:** The text has been updated accordingly.

**L308.** Most of this section 4.1 is like results but not discussion. Maybe better to restructure this part and combine it to Section 3.

**Reply:** We take this comment into consideration when restructuring the manuscript as outlined above.

**Action:** After reconsidering this comment, we think that this section is discussing the results of our study with other studies from northwest Europe and is providing evidence for environmental variability regarding substrate type, depositional environment and sedimentary organic carbon.

**L345.** It is important to distinguish two concepts - spatial heterogeneity and spatial gradients. The discussion based on OCAR and  $\delta^{13}\text{C}$  is seemingly isolated. It would be better to focus on heterogeneous OCAR. Currently these two parts are not well integrated.

**Reply:** We will take this comment into account when re-writing the discussion.

**Action:** Section 4.2 is now focussing on gradients. We have moved the part on OCAR to the next section (4.3), where we upscale the results from sediment cores to the study site and illustrate what might happen when only OCAR data from sites

**selected for contaminant studies might be used instead of a larger less biased sample.**

**L347-348.** What is the range of OCAR after excluding the lowest and highest ones. Extreme values may be related to specific processes.

**Reply: 22 – 73 g C m<sup>-2</sup> yr<sup>-1</sup>, when excluding the minimum and maximum values.**

**Action: It is difficult to include this information in the sentence highlighted by the reviewer. However, we have included it in section 3.3.**

**L387-394.** There is no need to repeat findings from previous researches. It is better to only highlight the differences that are related to the main part of this manuscript. Moreover, all of these are not well supported by the dataset in this study.

**Reply: Agreed. We will rewrite the discussion as mentioned in the beginning.**

**Action: The whole section 4.3 has been rewritten to focus on upscaling of organic carbon accumulation rates from the 12 cores to the site level.**

**L408-410.** I would respectfully not support this statement. Comparisons can be conducted between two global compilations or researches; but it is not such reasonable to compare a subset from global compilation with a regional case study, as the main focus of the previous studies is not regional OC burial rates.

**Reply: It might be correct that a global study can't be compared with a regional one. However, we would maintain the argument. According to Table S4 in the supplement to Cui et al. (2016), there are six fjords from northwest Europe, two from Sweden and four from Norway. The Swedish fjords do not have information on accumulation rates of terrestrial OC. The average value for northwest Europe must therefore depend on the four Norwegian fjords. The same Norwegian fjords have been classed as terrestrially dominated (Faust & Knies, 2019), while certain fjords in northern Norway are marine dominated (Faust & Knies, 2019) and Trondheimsfjorden and the fjords in our study site show a gradient from marine to terrestrially dominated. Just based on this argument, the average value of 76% terrestrial OC accumulation thus would appear biased. This argument is also supported by Faust & Knies (2019): "These findings indicate that the relative contribution of marine OC to fjords in Norway can be much higher than previously suggested for fjords in NW Europe (24%) and also globally (38-45%; Cui et al., 2016).**

**Action:** The whole section 4.3 has been rewritten to focus on upscaling of organic carbon accumulation rates from the 12 cores to the site level. As a consequence, this part has been removed but we have added a sentence to section 4.1 about the terrestrial fraction of organic carbon: Overall, these results indicate that the terrestrial fraction of organic carbon might deviate substantially from the average value of 76% for northwest Europe (Cui et al., 2016), echoing findings from Faust and Knies (2019).

**L420-421.** Large uncertainties of fjords' area may be a main difficulty when upscaling regional estimates to global level. This may be a main point of this article, which is also related to Section 4.4. However, it seems the discussion (especially in Section 4.3) goes too far from results of this study.

**Reply:** We maintain that the poorly constrained global fjord area is only one limitation in upscaling to the global level. The other limitations are the implicit assumption that fjords accumulate sediment across the whole seabed area (which can be disproved by maps of substrate types (Smeaton & Austin, 2019 and Figure 3 in this manuscript) and sedimentary environment (Figure 4 in this manuscript)) and a likely sampling bias since the best coring sites for deriving sediment accumulation rates are OC-rich muddy sediments (Smeaton & Austin, 2019).

**Action:** None specifically here but we now demonstrate the likely sampling bias based on our own data (section 4.3). We had already demonstrated in the original submission that sediment (and hence organic carbon) accumulates in only half of the area of the study site.

**L425-428.** I don't fully understand why only muddy area is depositional in character. It is more likely almost all the seabed buries sediments with hydrodynamic sorting that leads to heterogeneous distributions of coarse and muddy sediments.

**Reply:** We respectfully disagree with this viewpoint, at least if we are considering longer term (more than a few days or weeks) sediment and organic carbon accumulation. Long term accumulation is limited to the deeper fjord basins where there is limited agitation by waves and currents. Shallow areas under the influence of waves and currents show net erosion and transport of sediment and hence do not accumulate sediment and organic carbon. These areas are characterised by coarse-grained sediments low in organic carbon content. Sometimes, bedrock might be exposed or lag deposits left behind. Overall, such environments are not conducive to long term accumulation of fine-grained sediment and organic carbon. This is reflected in Figure 4, which shows that the deposition of fine-grained

sediment from suspension is limited to deeper areas. Such areas are dominated by fine-grained sediments (Figure 3), and it is well known that organic carbon content correlates with grain size.

**Action: None**

**L430-431.** So how can a dataset be representative of global fjords? This statement is not directly supported by any result in this study. A dataset (with a large number of randomly chosen samples) that covers different latitudes, different fjord types, full land-to-ocean gradients is supposed to be very close to the realistic situation of global distributions.

**Reply:** The issue with global compilations is that they are not a randomly chosen sample. Rather, they are likely to exhibit a sampling bias as coring sites are frequently chosen in a way that they can be dated and accumulation rates calculated. Standard dating techniques, such as the constant rate of supply methodology (Appleby, 2001) require undisturbed core sediments to yield reliable results. This would typically be in the central part of a basin where there is little sediment agitation due to waves and currents and ideally little or no bioturbation. To achieve an unbiased sample would require taking randomly placed samples across the areas where sediment accumulates in a fjord and then calculate OC accumulation for these areas alone (i.e., exclude erosional areas).

We can illustrate the issue based on our own data: Two of the cores shown in Figure 6 were collected as part of a contaminant study (Knies et al., 2021). For such a study type, it is indeed prudent to select coring locations that minimise the chances of sediment mixing. The average OC accumulation rate of these two cores is  $77.7 \text{ g C m}^{-2} \text{ yr}^{-1}$ . The other ten core locations were selected from a stratified random sample as described in sec. 2.3, i.e., in a less biased way. The average OC accumulation rate of these cores is  $37.97 \text{ g C m}^{-2} \text{ yr}^{-1}$ , i.e. less than half of the previous value.

We think that there are good reasons to believe that OC accumulation rate data in global collections is of the former type, i.e., sites were chosen by an expert in a targeted way to minimise sediment mixing, while a representative sample to be used for global upscaling would require a random sample design.

**Action:** We now illustrate how organic carbon accumulation and hence burial rates are likely overestimated in global studies by using data from our own study. (section 4.3).

**L453.** It is very time-consuming, as the authors said, to map the global distributions. The most important issue at present is still to obtain a reliable global repository of fjords' area. Moreover, the authors should clarify why only muddy sediments should be accounted into calculations of OC burial, as well as how can excluding (or including) coarse sediments influence global estimates.

Overall, the discussion part is not well supported by the main results in this study and corresponding wordings should be rephrased.

**Reply: Please see the previous replies.**

**Action: None**

## References

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The authors present a study of a fjord system around Stavanger, Norway, aimed at improving estimates of sedimentary organic carbon stocks and composition. Building on samples collected at randomised stations, they use spatial predictions to scale up to the fjord scale and assess the impact of biased selection of sampling sites. The authors conclude by discussing the implications of upscaling methodology at the global scale, stating that sedimentary organic carbon stocks/burial in fjords are overestimated, and suggest a method for improved estimates.

I very much enjoyed reading this manuscript, which I believe makes an important point. The text is generally well written, yet would benefit from a few clarifications and structural changes.

**Reply: Thank you for the assessment. We provide replies to the detailed comments below.**

Detailed comments:

Section 2.1: Please give a more extensive description of the fjords. What does the catchment area look like? Riverine input? Oxygen conditions (this is only mentioned briefly in the discussion)?

**Reply: Agreed. We will provide a more detailed description of the study site.**

**Action: We have added more information about the study site, in particular Boknafjorden, which is the dominant fjord in the area.**

Section 2.4: Were the bottom-water conditions measured, i.e., oxygen, salinity and temperature?

**Reply: No, we did not measure these parameters. However, we used modelled bottom-water conditions as predictor variables for modelling and spatial prediction as stated in ch. 2.5 (l. 174 – 179) and Table S1. These do not include bottom water oxygen.**

**Action: None**

L123: How was it determined which sites would have cores collected for dating?

**Reply: This is described at the end of section 2.3 (l. 112-113).**

**Action: For more clarity we added that the ten cores were randomly selected from the 15 locations that fell inside the area mapped as deposition from suspension.**

L127-128: Was any inorganic carbon removed before analysis, or do these samples represent the total carbon rather than organic carbon?

**Reply: The samples represent total organic carbon. Total carbon was also measured but is not dealt with in this manuscript.**

**Action: We updated the text to explain that organic carbon was analysed after acidification with diluted hydrochloric acid to remove inorganic carbon.**

L140: The abbreviation “COM” is only used twice in the text; I suggest writing calcium oxalate monohydrate instead.

**Reply: Agreed.**

**Action: COM was replaced with calcium oxalate monohydrate.**

Section 2.5: I find this section hard to follow and suggest it be reorganised. It is currently hard to follow why and how the predictor variables were chosen. Could you expand on the mechanistic reasoning? While the magnitude of oxygen's impact on organic carbon preservation is debated, I would have expected this factor to be included in the analysis. How come it is not?

**Reply: We will reorganise the section and provide additional information for more clarity.**

**Regarding the choice of predictor variables: There are two steps involved. In the first step, potentially important predictors are identified and collated as gridded data covering the area of interest with full coverage. This could mean that a predictor that is deemed relevant might not be included in the case where it is not available for the area of interest. In this study, we were not able to identify a suitable dataset on bottom water oxygen. In a second step, we use a data-driven approach to find the combination of predictors that gives the best model performance. This is achieved with the forward feature selection function of the R package CAST (l. 185).**

It is also noteworthy that we are using an empirical model and not a mechanistic one.

**Action: We have updated the text in Section 2.5. In particular, we explain that the model we used is an empirical and not a mechanistic model. We also explain in more detail how the environmental predictor variables were selected.**

L259-260: Why was sediment mixing so low across much of the area? Are the bottom waters low in oxygen, or is this a normal feature of the benthic faunal communities in these fjords?

**Reply: These statements were based on reports from the dated cores we received from the dating laboratory. Intensely mixed core sediment will lead to sediment accumulation rates that are too high. The purpose of this paragraph was to flag cores where rates are potentially too high. However, some bioturbation might also have occurred in the remaining cores but without leading to unreliable dating results. We suggest rewriting this paragraph for more clarity.**

**Action: We added a sentence for more clarity: To minimise the risk of sediment mixing, sediment cores for dating were only collected from the area mapped as deposition from suspension.**

Sections 3.4-3.6: What is the mechanistic rationale for using different predictor variables for organic carbon stocks,  $\delta^{13}\text{C}$  and organic carbon reactivity? I would expect the three to be linked in general (as suggested by Figure 5). While I understand that different predictors may be motivated, some discussion on this topic would be useful.

**Reply: As stated above, we are not using a mechanistic model, and the selection of the predictor variables is data-driven with the aim to optimise model performance and hence accuracy in the quantitative estimates. This does not necessarily preclude from speculating about potential mechanisms; however, it must be done with caution. In addition, it is also important to consider the purpose of the model. In our case, the purpose was (spatial) prediction. Optimising for prediction might however be suboptimal for other purposes such as data exploration or inference (Tredennick et al., 2021).**

**Action: None**

Section 3.5:  $\delta^{13}\text{C}$  notation uses '13' in both superscript and normal script.

**Reply: We will remove inconsistencies in the terminology.**

**Action: We have removed inconsistencies throughout the text.**

L324-327: Could the authors expand on why there is such a difference in organic carbon stocks between Stavanger and those observed in Scotland and Ireland? What

environmental, hydrological, and bathymetric conditions lead to the much higher organic carbon stocks in the latter areas?

**Reply: We suggest in the following sentence (l. 327-328) that the difference can be attributed to a statistical effect, i.e. a national scale assessment (Scotland and Ireland) might show a larger variability and hence also large maximum values.**

**Action: None**

Section 4.3: While I believe the authors are correct that site selection can skew carbon stock estimates considerably, I wonder whether there is support in previous studies for this. Could the data spread in previous studies be further explored?

**Reply: In section 4.3, we state that organic carbon stock estimates have only be reported for Scotland and Ireland and that the approach taken can be expected to yield realistic results.**

**Action: After reconsidering this comment, we believe that the reviewer was referring to organic carbon burial rather than stocks. We have significantly altered section 4.3 and now provide an example based on our own data how this skewing towards high estimates might happen. We also mention in the introduction that Smith et al. (2015) acknowledged in their supplementary information that the burial rates might be biased towards high values because the sediment cores in their global dataset tended to be located in depositional zones or fjord basins with higher sediment accumulation rates to obtain higher age resolution.**

## **References**

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