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.

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.

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.

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.

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.

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.

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.

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.

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

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

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.

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

L65. What global estimates?

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

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.

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_2 atmosphere (with possible charring).

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

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.

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

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

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.

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

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.

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

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

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.

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.

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.

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.

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.

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.