Point by point response to reviewer 1:

Bartl and Thrush conduct experiments to gauge the amount of additional CO2 released from sediment disturbance using incubations of natural sediments sampled from Hauraki Gulf, New Zealand. The authors analyse their results using a machine learning method and find non-linear relationships and interaction effects between additional CO2 release and sediment characteristics. It is concluded that assessments of carbon storage vulnerability must account for sediment heterogeneity.

In general, the paper is well written, and the methodology clearly described. However, I found the current presentation and interpretation of results to be lacking. The fact that sediment heterogeneity needs to be accounted for when assessing carbon impacts is already well-established (and somewhat trivial), and the usefulness of the resuspension assay has already been introduced in the earlier work by Bartl et al. (2025). The results of the BRT model are interesting, but they are presented in a quite condensed manner, and it is not laid out clearly what exactly we can learn from them.

Response:

We thank the reviewer for their thorough assessment of the manuscript which has drawn us to focus more clearly on the purpose of the work presented in this manuscript. With our study we aim to identify sediment types that are at highest risk of producing CO_2 when resuspended and to identify what relationships and interactive effects of highly variable sediment characteristics influence the variability of resuspension-induced CO_2 production. This forms the basis for upscaling the risk of resuspension-induced CO_2 releases across heterogenous seafloor spaces to inform decision makers. We agree that for our purpose of this manuscript we need to explain more clearly what can be learnt from the BRT results in the context of identifying vulnerable sediment types. Modifications of the BRT analysis and results are addressed in the following responses.

I do think the data collected and the experiments done are valuable and useful, but the discussion focuses almost exclusively on the BRT results, which are difficult to interpret, since such ML methods tend to obfuscate possibly straight-forward interactions and relationships. I encourage the authors to dig a bit deeper into their data through additional analyses and/or to present the BRT results in more detail, and to discuss the possible mechanistic explanations for the observed patterns. I list some specific suggestions below, along with other comments.

Response:

Boosted regression trees and other machine learning models are so-called black-box models originally designed for prediction. In marine science, BRTs have been mainly used for predicting the distribution of species, fishing effort, or ecosystem services (Cimino et al., 2020; Lohrer et al., 2020; Soykan et al., 2014). While their application in more biogeochemical contexts is relatively scarce, it has been proven to be a powerful method in relating biogeochemical processes/ variables to environmental factors and identifying types of relationships and patterns (Panaïotis et al., 2025; Rijkenberg et al., 2011; Trost et al., 2025). Critical to applying BRTs in ecological/biogeochemical studies is to extract information of environmental patterns by using tools for interpreting BRT model output. Those are, for example, feature importance (Fig. 2A in the manuscript), partial dependence plots (Fig. 2C-E in the manuscript) and interaction importances (Fig. 2B). We thank the reviewer for highlighting that we need to present our BRT results in more detail to aid interpretation of what the results mean. Upon further reading into interpreting BRT model outputs, we have concluded that it is necessary to exclude highly collinear features from the analysis. While multi-collinearity is not problematic for prediction, it is problematic for the interpretation of BRT results, particularly the ranking of individual feature importance and feature interactions as it can mask the importance of relationships and interactions of other individual features (Boulesteix et al., 2012; Dormann et al., 2013; Lucas, 2020). We checked collinearity between our features (sediment characteristics and water depth) and removed Mud and M-Sand from the analysis as they presented a strong correlation with OM ($r > 0.8$). We decided to keep OM content as feature because it is the substrate for organic carbon mineralization. The removal of Mud and M-Sand does not change the key results of this study and in addition now shows much clearer relationships and interaction effects of the other features which allows for a better interpretation of results. We detail the revised BRT analysis and how it compares to the previous analysis in a separate attachment (Revised BRT_egusphere-2025-3045.pdf). In the revised manuscript we will present the revised BRT results in more detail and clarity in the following way:

1) **SHAP summary:**
   We will present overall feature importance (SHAP summary bar plot) and feature interaction importance (heatmap) based on mean absolute SHAP values (± SD of 50 model iterations) to illustrate which environmental features contributed most to BRT model output, i.e. modelled RCO2P.
2) **SHAP Dependence Plots:**
   We will then go into more detail and present SHAP partial dependence pots of all (not just the top three) individual features. While the SHAP summary gives a general overview of features, SHAP dependence plots show how the model output, i.e. predicted RCO2P, vary by feature value of each individual feature. We will use this to describe how each feature contributes to modelled RCO2P, i.e. with a linear or non-linear relationship. We will also use these plots to identify the intercept at SHAP = 0, i.e. at which feature value we see a shift from a negative to a positive contribution to modelled RCO2P.
3) **SHAP Interaction Dependence:**
   Lastly, we will present individual feature interaction dependence plots to describe in which way two features interact to contribute positively or negatively to modelled RCO2P.

We will then adapt the discussion text to include possible underlying mechanisms for the observed patterns.

General comments:

· The resuspension assay is meant to mimic trawling impacts, but it's not clear what the historical and current trawling intensity in the study area looks like, or what other bottom-disturbing activities (dredging, sand mining, ...) may occur in the study area. Can the authors give some additional information about this in 2.1? Right now, it is only briefly mentioned in the introduction.

Response:

Thank you for pointing this out. We will describe where anthropogenic disturbances (trawling, dredging, sand mining) occur and provide additional information on trawling intensity from the past 5 years based on global fishing watch data (Global Fishing Watch, 2025).

Addition to 2.1:

Anthropogenic impact to the sediments of the Hauraki Gulf have been present for decades and comprise bottom trawling, dredging, and sand mining (Hauraki Gulf Forum, 2020, 2023). Bottom trawling generally occurs at water depths >50 m has been more concentrated in the Colville Channel, north-west of Jellico Channel and east of Great Barrier Island in the past 5 years (Fig S1). Commercial dredging for scallops generally occurred in water depth <50 m, particularly around Hauturu and Coromandel Peninsula, but has been banned since 2022 (Hauraki Gulf Forum, 2020).

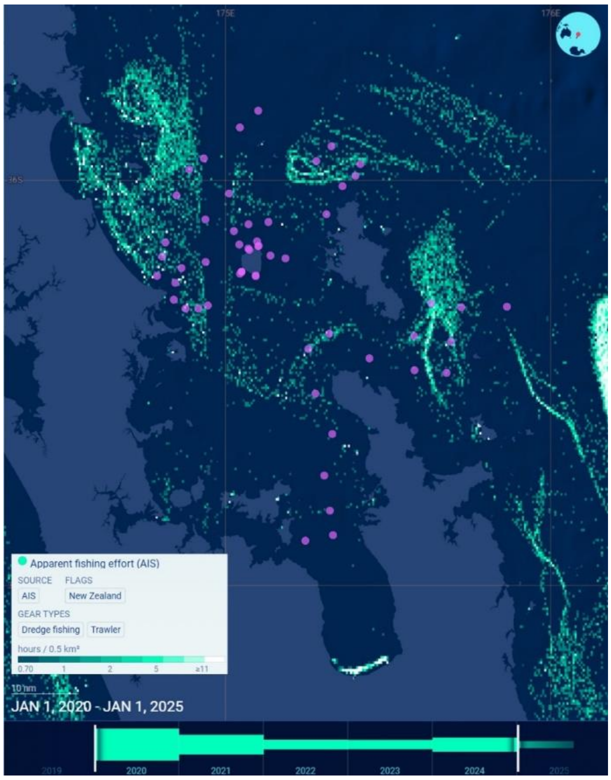


Figure S1: Apparent fishing hours extracted from Global Fishing Watch (Global Fishing Watch, 2025) for 2020-2024 for the gear type trawler and dredge fishing. Pink dots are the sites sampled in February and March 2024.

· The assay is based on SOD measurements, which are then converted to CO2 based on a constant RQ. The authors have justified this choice in Bartl et al. (2025), but I am not convinced that this should hold for this analysis as well. Is the value of RQ=0.9 valid for every sampling location? How can the authors be sure that their measured SOD is due purely to OC mineralisation, as opposed to aerobic oxidation of other species, which has been shown to be a larger oxygen sink in some muddy sediments compared to OC mineralisation (e.g. Kalapurakkal et al., 2025)? Perhaps the authors can give some information on oxygen penetration, redox-depth etc. in their samples to clarify this.

Response:

We thank the reviewer for pointing this out. We have double-checked our choice of an RQ=0.9 (Jørgensen et al., 2022) and identified, that for sites shallower than 50 m, the use of RQ=0.9 is correct but for sites deeper than 50 m, we should have used an RQ=0.85 for outer shelfs (50-200m). In addition, we have conducted a full revision of our data quality process and have identified that two sites need to be omitted from data analysis as they do not meet the quality assessment of the resuspension assay method. We apologize for this mistake and have repeated the BRT analysis with the corrected data set. It did not change the key results of this manuscript (Table below, and attachment Revised BRT_egusphere-2025-3045.pdf) and the results and discussion will be revised accordingly.

Metric	Old BRT results	New BRT results with corrected RCO2P (Mud and M-Sand excluded due to collinearity with OM)
R^2	0.57 ± 0.1	0.57 ± 0.1
RMSE	0.56 ± 0.1 mmol CO_2 m^{-2} h^{-1}	0.54 ± 0.07 mmol CO_2 m^{-2} h^{-1}
feature importance by mean |SHAP|	1. OM, SHAP = 0.42 2. Depth, SHAP = 0.13 3. F-Sand, SHAP = 0.12 4. M-Sand, SHAP = 0.12 5. C-Sand, SHAP = 0.12 6. OM:Phyto, SHAP = 0.08	1. OM, SHAP = 0.47 2. C-Sand, SHAP = 0.15 3. Depth, SHAP = 0.12 4. F-sand, SHAP = 0.1 5. OM:Phyto, SHAP = 0.09 6. Chl.a, SHAP = 0.07

It is true, a fraction of SOD may be due to aerobic oxidation of reduced species such as pyrite which can occur at similar time scales to OM mineralisation. Kalapurakkal et al. (2025) incubated slurries of 10 cm sediment depth from a eutrophicated bay where the oxygen penetration depth is <1.6 mm. In contrast, our incubation used the top 3 cm of sediments in an oligotrophic system. Cheung et al. (2024) reports oxygen penetration depth of 3 – 6 mm in sediments of the Hauraki Gulf (30 – 128m water depth, sands and muds) and a nitrate penetration depth down to 12 mm. Together with high TOU/DOU ratios (2.4) that indicate strong macrofauna influence on redox conditions, this suggests minor accumulation of reduced species in our type of sediments. This is supported by Wilson and Vopel (2012) who reported low concentrations of acid volatile sulfides (e.g. FeS, FeS_2 and Fe_3S_4) of 0.02-0.06% dw in muddy sediment (12 m water depth) as opposed to e.g. 0.2-0.3 % dw AVS in eutrophied sediments of Chesapeake Bay (Hantsoo et al., 2023). While Kalapurakkal et al. (2025) mention the analysis of pyrite content in their methods, data was not provided so we couldn't compare it. We will add the above discussion to section 2.2 in the manuscript methods for clarification.

· Only absolute SHAP values are shown in Fig 2A+B even though, according to 169-172, positive and negative impacts of each feature can be distinguished by the sign. For example, I would expect OM:Phyto to have a negative sign. This important information is hidden by showing only the absolute values. Also, can metrics like SD or ranges, (based on the 50 iterations of the model and/or based on the mean of iterations but for all data points) be shown for the SHAP values to get a sense of their distribution/uncertainty?

Response:

We thank the reviewer for highlighting that information is hidden. In our revised results we will describe positive and negative impacts of each feature (including OM:Phyto) in more detail using partial dependence plots which provide positive and negative SHAP values. We will add SD to the SHAP feature importance bar plot to provide insight into model uncertainty. In the partial dependence plot, average, 10th percentile and 90th percentile of LOWESS smoothed functions will further give a sense of the distribution of SHAP values from the 50 model iterations.

· The authors focus their discussion on the results of the BRT model, but the interpretations of the resulting patterns and their mechanistic explanation is lacking. For example, two mechanisms that immediately come to mind are (1) decreased oxygen penetration in muddier sediments that could decrease OM degradation upon resuspension and could explain the disproportionate RCO2P compared to sandier sediments and (2) a decrease of terrestrial OM with distance from coast could explain the pattern in Fig 2D, assuming that terrestrial OM is generally less labile in marine environments compared to marine OM. Terrestrial vs. marine OM is mentioned briefly in 2.1, but it is never discussed afterward, though I assume it should have an impact on the degradability. Can the jumps/non-linearities (Fig. 2) be separated geographically, e.g. between firth, channels and offshore? This could be a straight-forward explanation for the patterns shown in the BRT results. Another question that is not addressed is why M-Sand should have such an important role (though it's not clear why the authors focus on this fraction in the first place, since it seems to be no more important than the other sand fractions; see specific comment).

Response:

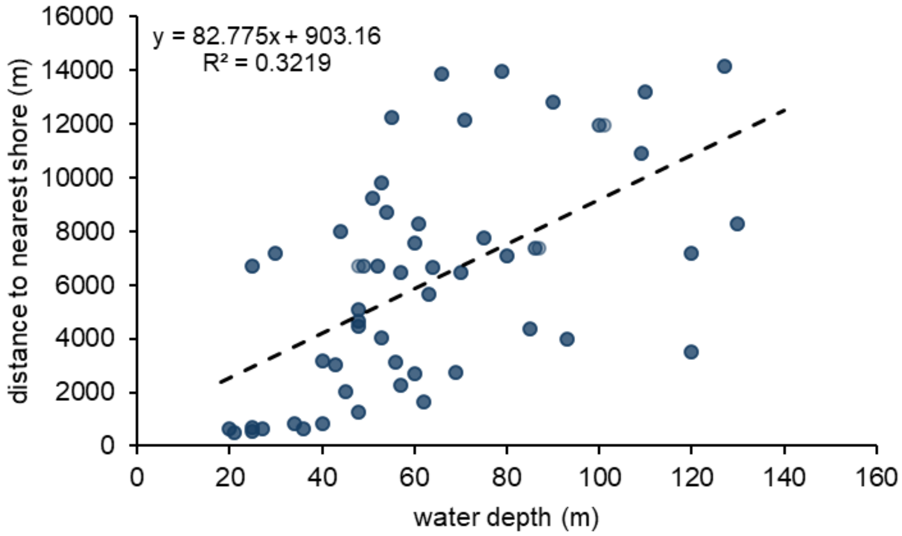
We thank the reviewer for their thoughts on potential mechanistic explanations. As mentioned by the reviewer earlier the critical issue is that the results, i.e. role of most important features, what is driving non-linear relationships, context-dependency of feature interactions, need to be discussed in more detail. We will address this by adding a paragraph on result interpretation in the discussion with the focus on what underlying mechanisms might be implied by the BRT results. As our study was aimed to observe and identify patterns between RCO2P and variable sediment characteristics, discussion around underlying mechanisms can be speculative and we will use our observations to suggest potential scopes for future (experimental) studies.

Regarding the two suggested mechanisms:

(1) Decreased oxygen penetration in muddier sediments decreases OM degradation in undisturbed sediments, as the lack of oxygen prevents the more effective oxic mineralization and allows for accumulation of less degradable OM (Hulthe et al. 1998). However, when resuspended and in contact with oxygen, such sediments have been observed to experience a stimulation of OM degradation as both, labile and older/refractory OM are mineralised Hulthe et al. (1998).

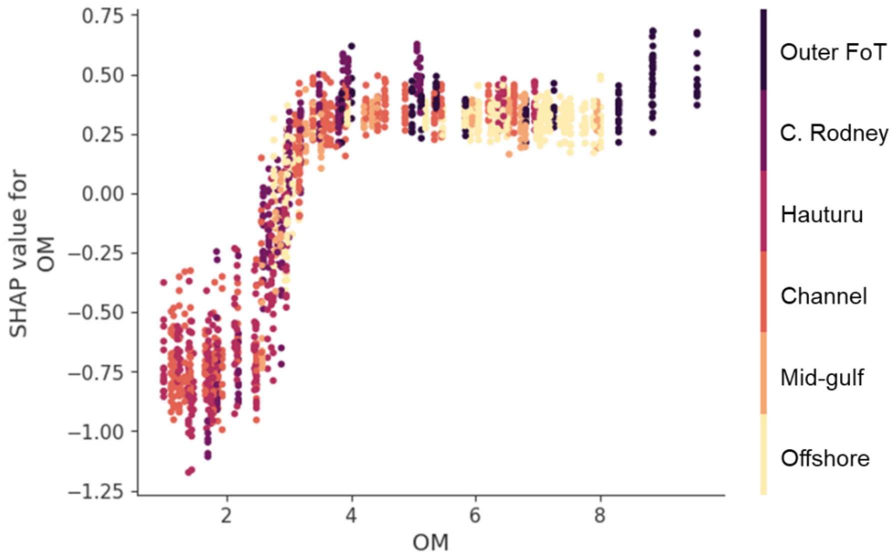
(2) Yes, decrease of terrestrial OM/ increase of fresh marine-derived OM with distance from coast could explain the higher RCO2P with increasing water depth (Fig. 2D). This would assume though, that distance to coast correlates with water depth. We have derived distance to coast values for our sampling sites and while there is a positive trend, the relationship is governed by high variability due to the presence of multiple islands and steep slopes within the Hauraki Gulf (Figure below). Additionally, Sikes et al. (2009) found signatures of terrestrial compounds in OM

both in shallow inshore sites and at the outer shelf which suggests that there is considerable redistribution of land-derived OM within the Hauraki Gulf. Therefore, we think that the source of OM is likely not a key driver for the resuspension impacts on RCO2P that we observed.



Non-linearities due to regional differences:

We overlayed different regions (offshore, Channels, Firth, mid-Hauraki, Cape Rodney, Hauturu) of the Hauraki Gulf in the partial dependence plot of OM (most important feature) to see if the non-linearity is linked to different regions (figure below). Overall, the non-linearity cannot be explained by region, but some regions, or the conditions there, might play a contributing role for some sites, e.g. negative SHAP values at low OM linked to sites from the channel and Hauturu Island. We think that the non-linearities and shifts are linked to multiple drivers, such as bottom water currents and redistribution of OM within the Gulf that differ locally within the Hauraki Gulf (line 321 in the manuscript) and we will make this discussion clearer.



Medium sand:

This has changed with the revised BRT method where M-Sand is removed due to collinearity with OM to make BRT results more interpretable.

Specific comments

· 1: The authors may consider adding a larger-scale, regional map to give readers unfamiliar with the area a better sense of where the area is located.

Response: Yes, we will add a larger-scale map.

· 14-15: „…we quantified RCO2P *and it* with measurements of sediment grain size, organic…" missing word?

Response: Sentence corrected.

· 28: I suggest adding references to some other studies to give a sense of the large uncertainty in this number (e.g. Epstein et al., 2022; Hiddink et al., 2023; Zhang et al., 2024).

Response: We added the references and adapted the sentence.

'Globally, bottom trawling is estimated to cause a loss of stored organic C to aqueous CO_2 of 0.58 Gt per year (Sala et al., 2021), with 55-60% released back to the atmosphere, highlighting the considerable contribution of demersal fishing to greenhouse gas production (Atwood et al., 2024). While certainty around those estimates is debated (e.g. Epstein et al., 2022; Hiddink et al., 2023; Zhang et al., 2024), little regulatory action has been taken to account for the vulnerability of marine sediment C storage to disturbance (Porz et al., 2024).'

· 2: Though details may be provided in Bartl et al. (2025), a short description on how the resuspension assay was performed would be helpful (e.g. using only the top 3 cm, gentle shaking, …)

Response: We added more detail (**bold**) to the description of the RA method:

'To quantify the production of aqueous CO_2 through enhanced organic C mineralisation in resuspended sediments, we conducted the resuspension assay according to Bartl et al. (2025). The assay incubates undisturbed sediments in cores ***(Acrylic tubes, inner diameter = 3.4 cm, height = 14 cm)*** *and resuspended sediments in glass bottles and measures the change in oxygen concentration through time to determine the sediment oxygen demand (SOD).* ***For both treatments the top 3 cm are incubated, a sediment depth commonly disturbed by trawling and dredging*** *(Hiddink et al., 2017)****. For sediment resuspension treatments the optimal sediment:water ratios keeping the resuspension mixture oxic, were determined to be 1:8 for sandy sediments (using 250-mL bottles) and 1:17 for muddy sediments (using 500-mL bottles). Test incubations showed optimal incubation time to measure a detectable decrease in oxygen but less than a 30% decline to be 4-6 hours. After adding the sediment to pre-filled glass bottles (0.2-µm filtered seawater, ambient bottom water temperature), resuspension was achieved by gentle inversion for 30 s.*** *At each site three pairs of control cores and resuspension treatments were incubated in the dark at ambient bottom water temperatures* ***which were monitored using loggers (Hobo Pendants, USA). In both,***

undisturbed cores and resuspension treatments, *SOD (mmol m^{-2} h^{-1}) was calculated from oxygen concentration measurements (OXROB10 probe and FireSting GO2 oxygen meter, Pyroscience, Germany) at the start and end of the incubation period and normalised to incubation time and area of the sediment core.* ***The calculation assumes a linear decrease of oxygen during the incubation time which was validated through preliminary incubations (Bartl et al. 2025). Four samples had an oxygen decrease of > 30% decrease from initial to final measurement, and 5 samples had an oxygen decrease of > 30% decrease from before and after shaking in the resuspension bottles and were removed from further data processing (Bartl et al. 2025).*** *From SOD, organic carbon mineralisation to CO_2 was estimated using a respiratory quotient of 0.9 for inner shelf sediments (Jørgensen et al., 2022). Resuspension-induced CO_2 production (RCO2P) was then calculated as the difference of CO_2 production in the resuspension treatment and the undisturbed sediment of each sample pair. The factor increase in CO_2 production in the resuspension treatment was calculated by dividing it with the CO_2 production from the undisturbed core.'*

· 117-119: The naming of the combined sand size classes is a bit unfortunate as it could create confusion, so I suggest introducing the acronyms "F-Sand" and "C-Sand" here and using those whenever referring to the combined classes, rather than "fine sand" and "coarse sand".

Response: We renamed the sand size classes as: F-Sand for sand grain sizes of 63-250 µm, M-Sand for sand grain sizes of 250-500 µm, and C-Sand for sand grain sizes of 500-2000 µm.

· 137+231: remove comma after „Both"

Response: Removed

· Tab 1: I don't see Phaeo:Chla in the table, though it's defined in the Table description.

Response: Apologies for this confusion. We have removed this from the table description.

· 175-177: Can the authors be more specific here? Which features could be omitted for the prediction? And doesn't this clustering imply that the interactions are not so important, and that OM itself is already a quite good predictor?

Response: Based on the other review comments we will revise the results text to provide more clarity on feature importances, collinearity and interaction importances.

· 177-178: M-Sand does not seem to be more important than the other sand classes according to Fig. 2a. Why do the authors choose to focus on M-Sand, and how can the high interaction score of M-Sand with OM be explained? The short discussion in 249 ("…other environmental factors play a role for the reactivity of sediment OM and thus RCO2P") is quite unspecific.

Response: This has changed with the revised BRT method where M-Sand is removed due to collinearity with OM to make BRT results more interpretable.

· Fig. 2C-E: It may be worth drawing a zero-line in these plots

Response: Zero-line added to the plots.

· 211: The authors may consider removing the linear regression formula here, since it disrupts the flow for the reader and is already included in the description of Fig. 3.

Response: Removed

· 240-241: Which range is being compared here to arrive at 2-88 mmol/m2/d? The range of undisturbed CO2P from Table 1 (0.1-1.0 mmol/m2/h or 2.4-24 mmol/m2/d) is different.

Response: The range of RCO2P is compared because the maximum potential oxic organic C remineralization rate from Porz et al. (2024) is used as proxy for organic C mineralisation due to trawling/sediment resuspension. Sentence has been clarified.

'The range of modelled oxic remineralisation rates reported by the authors (0.1 - 100 mmol C m^{-2} d^{-1}) is similar to our RCO2P measurements (2 – 88 mmol C m^{-2} d^{-1}), supporting the assay's applicability for resuspension assessments.'

· 248: "median" should be "medium"

Response: Corrected

· 250: Looking at the supplemented maps, it doesn't seem like grain size is always decreasing along the depth gradient. Maybe the authors can rephrase to clarify what is meant here.

Response: Rephrased.

'The heterogeneous distribution of larger grain sizes and OM in the Hauraki Gulf can be a result of physical forces such as the Gulf's circulation and tidal currents (Black et al., 2000; Manighetti and Carter, 1999; Zeldis et al., 2004).'

· 254: I don't think it's fair to say that the study has found any "interactive dynamics" at this stage, but rather relationships; the dynamic process understanding needs to be explored further.

Response: Agreed and removed "interactive dynamics".

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