We thank the reviewer for the useful suggestions for further improvements, which we have implemented for the most part. We also suggest areas of further research, such as implementing this kind of model in deep-sea/margin settings. We also updated the references to what was initially unpublished work throughout the manuscript: Toussaint et al., 2021, and De Borger et al., 2021 have been published during this reviewing process.

Changes have been implemented in the manuscript as track changes, and copied here in our reply. With these changes, we hope to have addressed the remarks sufficiently.

Main comments:

1. **Reviewer comment:** Abstract: Since penetration depth of tickler chain and electric pulse trawl are not significantly different (Fig. 2C), please refrain from using “deeper” and “shallower” descriptions of both gear types in the abstract. Instead, I would suggest simply stating “two gear types with contrasting degrees of disturbance of the seafloor”.

   Reply: We prefer not to alter this wording anymore (it has been previously changed from deep/shallow to deeper/shallower). The different degree of disturbance is specifically due to the differences in depth and variability, so it would be strange not to mention it immediately. Because of the used variance around the mean penetration of both gears, differences may not be statistically significant as implied by the figure, but still depths have been set 50 % lower for the shallower gear type.

2. **Reviewer comment:** Line 118-119: Using the measured DIC flux as the upper boundary organic carbon input flux. How can the measured DIC flux be used to estimate OC input? When you refer to OC flux, do you refer to particulate OC or dissolved OC? This sinusoidal carbon flux was estimated using what data? Is it a real representation of carbon input to the seafloor? Why use an amplitude of 1?

   Reply: Assuming (quasi-) steady state with little to no burial of OC (valid for a lot of areas in the North Sea), we expect the carbon output (DIC) to be similar to the input of organic matter, as in the sediment this organic matter is mineralised to DIC which fluxes back to the water column. The OC flux refers to the particulate OC flux, as in organic detritus arriving on the seabed. Choosing a sinusoidal function is a model simplification which represents the input of carbon to the sediment in shallow North Sea sediments reasonably well, as seen in this example below from Provoost et al., 2013, *Estuarine, Coastal and Shelf Science*. The amplitude of 1 causes a fluctuation around the steady state-mean carbon input, which touches 0 in winter. We have added clarification and explanation of our reasoning to lines 138-141: “*A sinusoidally varying detrital carbon deposition flux, with the model derived carbon flux (Cflux, Table 3) as the annual average, and imposing an amplitude of 1 was used as the upper boundary organic carbon flux (Figure 2 A). The uniform amplitude of 1 for all sites was chosen to simplify temporal variations between sites.*”

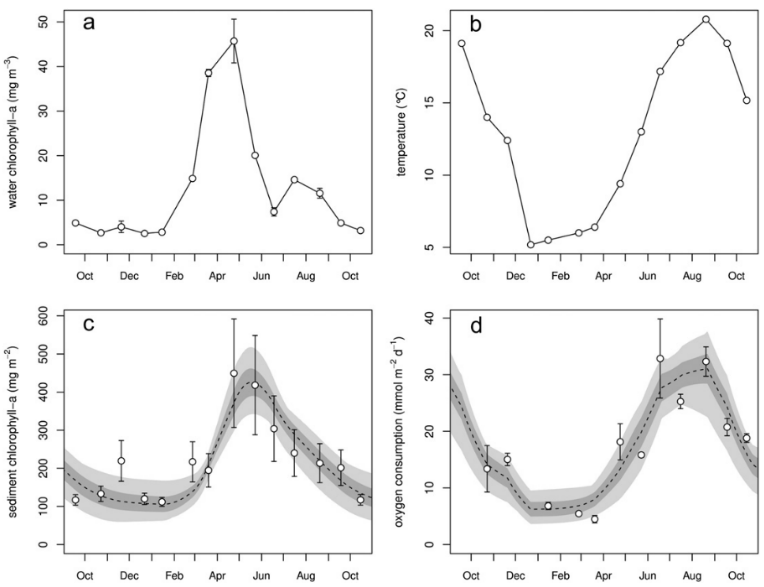

Fig. 3. Measurements of water column chlorophyll-a (a), temperature (b), sediment chlorophyll-a (c) and benthic oxygen consumption (d). Values of water column chlorophyll-a and temperature were interpolated to be used as forcing functions for the model. For (c) and (d) the global model sensitivity is given as well, with light grey indicating the range of model results and dark grey the standard deviation. The dashed line shows the median as well as the best-fit model run, which cannot be distinguished visually.

3. **Reviewer comment:** The varying OC flux in the different stations is not addressed in the methods. In lines 137 – 139. "A sinusoidally varying carbon deposition flux with the model derived carbon flux (Cflux, Table 3) as the annual average, and imposing an amplitude of 1 was used as the upper boundary organic carbon flux (Figure 2 A). This resulted in differing organic carbon fluxes for each location." However, this refers to the temporal variations in OC flux, and not the spatial variations. Could you please specify how you obtained the OC flux stated in Table 3?

   Reply: This has been explained in the methodology (lines 117-120): "*Model parameters included both measured concentrations in the bottom water, as well as process rate parameters that were derived following a 2-step steady state fitting procedure (Table 3). Using the measured DIC flux as the upper boundary organic carbon input flux, the O_2 flux and porewater profiles of O_2, NO_3^-, and NH_4^+ were first fitted manually by tweaking a limited set of model parameters.*" The description of how the DIC flux was measured, is in the supplementary information under section S1.2. Please see our previous reply on how we further addressed the varying OC flux.

4. **Reviewer comment:** At the beginning of the discussion (4.1 Organic carbon depletion) you mention that trawling removes OC through erosion, and that less OC is redistributed due to the absence of bioturbation. However, bottom trawling also mixes up sediment, which you consider in section 2.1.3 Disturbance modelling. Please include all of these aspects in this section of the discussion: trawling-derived erosion, mixing, and reduced bioturbation. In your disturbance simulation, the effect of the removal of bioturbators is far greater than the sediment mixing effect of bottom trawlers, which I consider is an important aspect to include in this section. To further add to this discussion, would the sediment mixing effect caused by bioturbators and bottom trawling be similar with an increasing trawling frequency? In certain fishing grounds, bottom trawling often occurs on a daily basis (i.e. Catalan margin).

   Reply:

   We have rewritten the first paragraph to better include the mixing event itself, though the effects of this (injection of O_2, NO_3^- to deeper layers) remain mostly discussed in 4.2. Lines 287-296 now read: "*The main drivers of the biogeochemical changes were found to be the depletion of organic carbon (OC) in the sediment (i.e. the substrate for mineralization itself), the redistribution of this OC nearer to the SWI (Figure 5), and the increasing oxygenation of the sediment. With each trawl pass, a part of the organic carbon rich top layer is removed (Figure 5). This is associated with an* *injection of oxidized reactants from the water column (O_2, NO_3) deeper into the sediment, and a homogenization of OC concentrations in the mixed layer during the mixing phase. Simultaneously, part of the benthos in the sediment is removed, often strongly decreasing the bioturbation rate, affecting the rate at which organic matter is distributed in the sediment (especially after multiple trawling events y^{-1}, Table 4). Sediment mixing alone could potentially increase OC contents at the bottom of the mixing zone, but successive trawling events, and the removal of bioturbators that can transport OC far below the mixing zone, resulted in a redistribution of OC closer to the sediment-water interface in all simulated sediments*."

We further point out that it is too general to say that the removal of bioturbators had a far greater effect that the mixing by the trawling gear. The mixing by the trawling gear clearly affects mineralization processes as well, among others by spiking sediment O_2 or NO_3^- concentrations which affected mineralization process rates up to weeks. However, our bioturbating organisms did have a potential greater effect on the distribution of OC, as has now been put in perspective in the previous paragraph: "*Sediment mixing alone could potentially increase OC contents at the bottom of the mixing zone, but successive trawling events, and the removal of bioturbators that can transport OC far below the mixing zone, resulted in a redistribution of OC closer to the sediment-water interface in all simulated sediments*."

Whereas your last suggestion is interesting, this is not something we have explored in this iteration of the model development, and adding speculation to the discussion might not be meaningful. My personal intuition is that mixing on a daily basis will make the trawling gear by far the dominant mixing process (without any knowledge of the benthos on the Catalan margin).

5. **Reviewer comment:** The effect of grain size and permeability in mineralization pathways are mainly described in section 4.2 Changes to mineralization pathways, lines 345-358. However, these effects are, in my opinion, unnecessarily repeated in section 4.3 Reducing gear penetration depth, lines 389-400. I highly suggest to discuss the effect of grain size and permeability only in section 4.2, and avoid going into detail in section 4.3, since this complicates the comprehension of the latter section. Moreover, I would suggest to change the section title to "Reducing gear disturbance on the seafloor", since it more appropriately describes the content of the section.

   Reply: There was indeed unnecessary repetition of this explanation, we have removed lines 398-403 from section 4.3. The section now flows from the importance of disturbing only a thin surface layer, to changes to sediment structure (Lines 390-394): "… *This indicates that only a thin layer of surface sediment needs to be disturbed to generate significant biogeochemical changes (Dounas et al., 2005). Many biogeochemical processes are mediated by the dynamics of oxygen near the sediment-water interface, which itself is influenced by the composition, and permeability of the sediment. A shift towards fining (an increased proportion of finer grain size classes) has been described in certain trawled areas, with expected consequences for sediment biogeochemistry, such as an increased rate of sulphate reduction (Trimmer et al., 2005). …*"

   We moved the reference to Huettel and Gust 1992 to section 4.2., where lines 355-357 now read: "*Larger pore-spaces in these sediments allow for bottom water to penetrate more deeply into the sediment matrix, bringing oxygen and other reactants into deeper sediment layers (Huettel and Gust, 1992). Cohesive sediments mostly lack such advective transport.*"

   We have also changed the title of section 4.3 to "*Reducing gear disturbance of the seafloor*" as per your suggestion.

6. **Reviewer comment:** Lines 421-422. A better citation to "Site-specific conditions such as rates of biogeochemical recovery and sedimentation rates need to be known to determine the resilience of ecosystems to trawling, and fine-tune management plans" would be Paradis et al. (2021),

Geophysical Research Letters, that addresses the effect of seasonal trawling closures in organic matter content of trawling grounds combined with the sedimentological characteristics.

Reply: Thank you for this interesting reference, we have replaced it.

7. **Reviewer comment:** Code availability. Please upload your code and data to Github or a similar repository.

   Reply: We will put the R-package that contains the diagenetic model on R-forge and will publish the code that performs the simulation in a Github repository upon publication of the manuscript.

Technical corrections

8. **Reviewer comment:** Line 53-54: "Observed biogeochemical changes caused by sediment resuspension can lead to the instantaneous release of nutrients from the sediment into the water column". Please add a reference to this release of nutrients from the sediment into the water column. For instance, one of the findings of the INTERPOL project published by Durrieu de Madron et al., 2005, Continental Shelf Research, studies this.

   Reply: Thank you for this suggestion, we have added a reference to Durrieu de Madron et al. (2005) to the sentence.

9. **Reviewer comment:** In line 225, "In FineL, MudL and MudH the ratio of labile organic carbon (FDET) to semi-labile organic carbon (SDET) increased between 25 and 34 % (Figure S3, supplement)". However, Figure S3 actually refers to FDET / (FDET + SDET). Moreover, in lines 83-84, you define SDET as slow decaying fraction, while here it is defined as semi-labile organic carbon (as well as in line 236). Please maintain consistency in the nomenclature.

   Reply: We have replaced the word "refractory" with "semi-labile" on lines 84 and 124 to remain consistent with that this component represents. Secondly, we updated line 237 to: "*In FineL, MudL and MudH the ratio of labile organic carbon (FDET) as a proportion of the carbon pool (FDET + SDET) increased between 25 and 34 % (Figure S3, supplement).*"

10. **Reviewer comment:** Lines 207-210. Please add the mineralization pathways of the Coarse sediment station, as you do for the remaining stations.

    Reply: The mineralization pathways for the Coarse sediment are given on line 204-205: "*In the coarse sand station (Coarse), the average total mineralization rate was 13.6 mmol C m^{-2} d^{-1}, with 89 % of this due to oxic mineralization, 6 % due to anoxic mineralization, and 5 % was denitrified (see Table S2, supplement).*"

11. **Reviewer comment:** Line 262. "[…] at 4 out of 5 stations". Instead of phrasing it this way, say "at all stations except MudH", or something along those lines. It is easier for the reader to follow.

    Reply: We have implemented this suggestion in line 262-264, which now reads: "*Denitrification rates (base: 0.6, 0.5, 0.1, 0.8, 4.2 mmol m-2 d-1 for Coarse, FineL, FineH; MudL, and MudH respectively) decreased with increasing trawling frequencies at all stations except MudH, with a maximum reduction of 74 % (tickler) and 68 % (pulse) at FineL (Figure 6: P-T).*"

12. **Reviewer comment:** Line 306. Add that these impacts could be far greater in deep-seafloor environments. Similar studies as those explained in this study should be conducted.

    Reply: We have implemented this suggestion as follows (lines 317-319): "*All three of these factors increase the impacts of trawling events on organic matter cycling in the model presented in this* *work, and further modelling work could be useful to investigate the potentially large impact that deep-sea habitats experience.*"

13. **Reviewer comment:** Line 313. Mengual et al., 2016 addresses trawling in shallow environments, and not in the deep-sea.

    Reply: This reference was used in connection to the "fine" aspect, and not as a study of resuspension in deep sediments specifically. We have reworded this sentence to: "*Deep-water species communities are slow-growing and thus recover slowly, organic matter deposition rates are low, and the generally finer-grained sediments found in the deep are easily resuspended following a trawl passage (Norse et al., 2012; Mengual et al., 2016).*"

14. **Reviewer comment:** Line 332. "(, Figure 7)". Erase the ", " when citing the figure.

    Reply: Thank you for noticing this, the comma has been removed.