BG-2023-145_Forster_RC1 response

Dear Dr Tiano, Referee 1.

Thank you for your stimulating comments. Generally, you will find almost all of the detailed suggestions adopted (see appended pdf). We have met your request for a more intensive discussion of the consequences that a shallow introduction of chlorophyll rich sediment, which is potentially rich in organic matter, may have for biogeochemical processes and distribution/fluxes of related substances.

We have also altered the objectives, since we do realize that the original wording may have led to a misconception. In this context we provide more background.

On the other hand, however, we cannot reduce the information given for Arctica, although you suggested to do so. This is important justification to the calculations leading to the final conclusion: peaks are more likely from fauna than from boards.

Thank you for taking the time to review.

answers to your comments:
In **bold** there is (part of) the original referee's comment,
Our response starts as indicated with >> … including reference to "new lines", which refer to "simple markup" view in MS Word.

(In the context of your comments on line 75, we added some text for clarity in lines 38)

-50: good to mention that Chl-a tracers degrade over time.
>> Done in new L. 52

-55: … it would be good to state or at least give an example of explicit biogeochemical consequences of sediment mixing/movement on biogeochemistry ….
>> now included in new L. 59: "with potential effects on carbon burial and inorganic nutrient release, including potential feed-back of these rate changes on bioturbating macrofauna."

-60: Better to state something about trawling being quite abundant rather than "massive" ….
>> wording has been changed (new L. 66)

-75: With the way the objectives are worded, I am a little concerned that the study may be perceived as having some bias. …
>> Evidence that peaks may be generated by otter boards are indeed not frequent and some are already given in the original manuscript, such as Oberle et al. (2016b). In order to pursue your valuable comment, we added text in new L. 38 in order to introduce more the idea of complex changes in matter, biota and processes in surface sediments. In addition, we altered the objectives in new L. 82 to: "i) mimic the genesis of altered chl-a distributions experimentally, ii) compare these with peaks found in the field and potentially originating from commercial trawling and finally iii) to discuss the likelihood of confusion of these peaks with those generated by bioturbation."

-90: "… a natural, horizontally homogenous vertical chl-a distribution"…
>> … has been changed to: "… reconstruct a horizontally homogeneous, vertically declining chl-a distribution."

-100: I am still wondering if this method of shoveling is truly representative of the effect from trawl doors. For me, I would imagine that trawl doors kind of 'plow' through the sediment continuously rather than shoveling discrete areas of sediment which would essentially turn the sediment upside down in the resulting mound. Are you able to add or rephrase some text to give more confidence in this part of the methodology to show that it is indeed representative of in-situ trawling effects?
>> 'Plowing' is the image we had in mind, when we saw images from board tracks which look much like that in figure 2 in Morys et al. 2021. Slabs or clods of sediment are aligned along the furrow drawn by the dredge or board. Modern plowing on land essentially turn surface soil over in a line of slabs too, however we were not sure that this is what happens in water saturated sediments under water and whether this is the appropriate mental model. In the *ex situ* experiment we tried to keep a defined depth down to which we would impact the sediment. But we failed achieving this in the confined space available by applying some plow-like tool. Therefore, we decided to use the shoveling, which was better defined in geometry, but is less similar to the process that we imagine associated with an otter board.
We rephrased that passage to make clear that this is a surrogate and that we did not have a perfect model impact in the *ex situ* experiment. In the aftermath our results show that whatever the difference was in the two mechanisms, the results are surprisingly similar.

New text (L. 108): "We decided to excavate sediment keeping a defined geometry of depth and width when removing sediment, rather than some plough-like tool which we found difficult to implement. In fact we are not sure about the exact mechanism and geometry of sediment removal and its deposition on the adjacent sediment, therefore the method used in the mesocosm is a surrogate and not necessarily the same as the physical process that might be active *in situ*. We excavated sediment with a flat rectangular shovel, scoop by scoop to about 4 cm depth …"

We also refer to this again in the discussion (new L. 229/230), since we now feel that the difference in mechanisms and the results obtained all the same are noteworthy.

-160: Is there a good reason why the models were not interpreted further and why model 5 was not investigated (was it just not relevant to the study)? If so, please state that for the reader.
>> (not including model 5 was not intended; it should have read 'models 3 to 5' instead of 'models 3, 4 and 4a')
New L. 172 reads: There is no benefit in trying to differentiate between models of higher complexity (models 3, 4, 4a and 5), since fitting results seemingly differ owing to some horizontal heterogeneity in our data. Also, we lack observations that would allow to differentiate mechanisms like particle injection in physical sediment turnover and compare it to model results. Therefore, we grouped the fits of non-local models into a category "non-local" and do not interpret them further.

-Results: I would personally prefer that certain sentences which describe results from the experiment be in past tense and that more general statements be in present tense. Really not a big deal but consider changing certain words to past tense in the results.
>> We changed this accordingly.

-Fig 4. It would make things much easier for the reader (especially lazy readers like myself) if the ex-situ (top row) and in-situ panels (bottom row) were explicitly stated in the figure itself rather than just

in the caption.
>> the information was introduced into the figure itself

-193: I thought the table was more 'quantitative' model output rather than qualitative (example: muddy vs sandy). >> New L. 207: "We compiled the quantitative information on non-local and local reworking retrieved from mixing.exe by giving the number of best fits in each category (Table 1)."

-217: You show that the results of trying to quantify vertical tracer concentrations create uncertainty for assessing bioturbation, but do they still provide insight on sediment mixing in general which may be caused from either faunal or human induced perturbations? The model results would then be more indicative of general disturbance rather than from only bioturbation.
>> This is exactly the problem; however, the present wording does not seem to capture this sufficiently well. The distribution AND the model results may only indicate general disturbance, but cannot tell us if this is generated by humans or animal.

New L. 233: "The uncertain origin of the observed peaks may affect the assessment of bioturbation intensity, since peaks may only indicate sediment perturbation in general."

-225: Depending on the type of sediment, you can see when previously resuspended sediment has settled on the seabed surface. Were you able to observe this visually in the furrows? If so, you can make the argument slightly stronger about "… sediment sliding back from the mounds". Also, could it not have been from re-settled sediment which was previously resuspended? That could leave smaller particles (typically higher in OM and chl-a) to resettle at a slower rate eventually settling on the seabed surface (with larger particles underneath). This was observed in an experimental study from Tiano et al., (2021) in fine sandy sediments but not in muddy sediment samples (though it may not be representative at all for what happens in this study).
>> We have no observation of settling fluff or sediment in the furrow. We also took care when shoveling to not resuspend or resuspend as little as possible. In situ we did not observe any exceptional Chl concentrations that might indicate resuspension/settling.
We do not intend to discuss this, since it is irrelevant to our observation and therefore do not change that sentence, which is now line 245.

-255: You can probably remove the last sentence of the page (you already state that it is irrelevant).
>> You are right. (remove from what is now new L. 272)

-265: In addition to nets/parts of the gear mixing sediment, Depestele et al., 2016 and 2018 suggest that this mixing occurs in tandem with the removal of surface sediment. This section would benefit with the inclusion of these studies in the discussion and their findings (erosion + mixing rather than these effects in isolation). Furthermore, De Borger et al., 2021 use this erosion + mixing effect as input for a biogeochemical model. Perhaps the findings from the current study suggest a different or at least an additional effect for modelers to take into account when predicting biogeochemical trawling impacts.
>> New L. 279: we have added to the discussion here. The new passage reads:
"The pattern of particle tracers generated by ground trawling, however, may depend on gear and sediment type. Nets are reported to mix surface sediments and reduce chl a content in the top centimeter (Tiano et al. 2019; Oberle et al. 2016b). Depestele et al. (2019) working at a southwestern Frisian Front site in the North Sea found that Tickler-chain trawl affected particle size distribution down to 2-4 cm depth. They also suggested that this trawling caused injection of finer particles into the

sediment at about 4 cm depth, while winnowing the top surface sediment, due to a combined mechanism of sediment removal (decapping) and mixing. The trawling gear with mechanical fish chasing mechanisms used in their study is much larger than the gear used in ours. In our data there is no visible decapping effect in the net area, likely because of the smaller size gear employed. Mixing only may be the correct approach in biogeochemical modelling in contrast to de Borger et al. (2021) who used a combination of erosion and mixing effect to infer consequences for carbon mineralisation and N-cycling. In our study the simulated and real net impact profiles were indistinguishable from respective controls. This implies that sediment mixing appears to be quasi random and similar in both cases. We thus conclude that these net impacts are not detectable in chl-a depth distributions."

-285: Mestdagh et al., (2018) found lower sediment oxygen consumption after deposition of low organic sediment as well as lower macrofaunal densities. They also found higher bioturbation and bioirrigation when low amounts of sediment were deposited and the opposite when high amounts of sediment were deposited. Could be relevant to the part of the discussion about depositional effects.

Lower oxygen consumption (also lower mineralization) would mean less carbon degradation and CO2 release and lower production of inorganic nutrients in the sediment. Furthermore, deposition of sediment with high levels of OM could have the opposite effect and could increase oxygen consumption due to the increased input of OM to the sediment (several unpublished experiments et al.; and it's also logical). Removal of OM from trawling has been attributed to decreases in O2 consumption and mineralization (Tiano et al., 2019, 2022) or increases (van de Velde et al., 2018) due to re-exposure of buried OM/priming. Lower mineralization might decrease fluxes of nutrients (less of them being generated in the sediment) but changing the diffusive boundary in the sediment water interface could increase nutrient fluxes (Warnken et al., 2003). Trawl-induced mixing + erosion can decrease denitrification (De Borger et al., 2021; Ferguson et al., 2020) but might also increase denitrification under conditions which maximize hypoxic areas within the sediment (De Borger et al., 2021). I state all this because I think there is more to be said about what kind of biogeochemical consequences are possible due to your results. Please try to expand a little bit on this section.

>> We did expand and incorporated some of your suggestions in the new lines 306 - 312. Overall there is not much specific information for our experiment (though included), so many thoughts remain speculation. But at least the passage links more to current knowledge.
Furthermore, there is an expansion in the passage on geochemical effects starting with new L. 316:

"Reversal of the top sediment will affect sediment biogeochemistry, since it changes chemical gradients close to the sediment-water interface completely. In the troughs decapping locally exposes anoxic sediments to oxygenated overlying water. Van de Velde et al. (2018) simulated and measured complete homogenisation of a 15 cm surface layer due to mixing by trawling nets as well as dumping of a homogeneous 15 cm layer on top of an existing sediment. They found pronounced enhanced mineralisation dominated by anaerobic pathways and effects on manganese cycling as a consequence of both scenarios. Based on samples from the experiment we report here, Röser et al. (2022) suggest that the coupled Fe-Mn-P cycle reacts very sensitively, as expressed by altered porewater gradients, indicating Mn enrichment in the mound area and Mn loss in the furrow. A disruption of the steady-state biogeochemical distribution is apparent in both cases, although our setting differs considerably from van de Velde et al. (2018) both in sediment height (~5 cm versus 15 cm) and mechanism (decapping/turnover versus mixing). Oxygen consumption was not measured, but it may decrease with

less reactive sediment exposed in the furrow or with the reversed sediment slap forming the mound surface (Tiano et al., 2019, 2022). Oxygen consumption may increase, however, when the board exposes high concentrations of reduced dissolved substances in the furrow or when potentially reactive organic matter such as chlorophyll is buried. The present investigation cannot further explore organic carbon fate after such trawling impacts (Epstein et al. 2021) since we did not generate corresponding data. Transient redox reactions in the sediment initiated by trawling (Bradshaw et al. 2021; Morys et al. 2021; Tiano et al., 2019), however, may differ considerably from redox oscillating know to occur during bioturbation (Forster 1998, Aller 2014, Gilbert et al. 2016)."

-325: I find Arctica super interesting. That said, there is too much written about these cool clams and it takes away from the message of this paper. Consider streamlining the Arctica sections.
>> From a biological perspective these information is needed to justify the subsequent back-of –the envelop calculations that yield in the probability of a peak being more likely generated by biology than by trawling. Therefore, we do not agree and would like to keep this information.

-332: The sentence discussing the quantified bioturbation (480 m^{-2} per year non local transport) vs. trawling (0.4 m^{-2} per year) might be one of the most applicable and interesting outcomes from this paper. Consider adding more to this discussion. As the message of this paper suggests that non-local mixing from trawling is similar with that of bioturbation, how does this quantified result relate to the main message of this manuscript (*does it put it in context* etc.)?

Moreover, this is a nice paper with some important results but I think the ending can be improved to showcase why we should find importance in these findings.

>> We have expanded on the final thoughts to bioturbation versus otter boards (see text below).
Maybe there is a typo in your comments or a misunderstanding: this paper ONLY suggests that non-local mixing from trawling is similar to bioturbation, not in the net area. (organic matter injection into below surface layers). The following passage should be more clear on this point.

New L. 371:

"The numbers calculated suggest 3 orders of magnitude higher frequencies for biogenic non-local transport (480 versus 0.4 $m^{-2}\,yr^{-1}$).
The spatial aspects of the particle transport events discussed above are particularly difficult to assess, since patchy occurrence of *A. islandica* and clustering of trawl tracks (Schönke et al. 2022) are frequent. Mound width and thus the area showing peaks generated by otter boards, likely depends on sediment type, steepness of the mounds and is additionally altered by the "bumpy" and discontinuous character of sediment deposition along the furrow (Morys et al. 2021) leading to overestimates. Despite this, with similar assumptions as above and with more uncertainty, we estimate that *Arctica* rework sediment (5 cm diameter circle around one animal, i.e. 19.6 cm^2 x 480 m^{-2} = 0.9 m^2) on a larger area than the area disturbed by otter boards (0.1 m^2).
Thus, we consider it conservatively safe to assume that the majority of peaks detected in Fehmarn Belt stem from *A. islandica* active on a daily scale. Bioturbation by *A. islandica* in Fehmarn Belt should thus be the more frequent particle reworking process when compared to otter board sediment reworking. Therefore, we may continue to interpret chlorophyll peaks as bioturbation traces in this area. The different mechanisms of sediment disturbance bear similarities such as the fresh organic matter injection

below the sediment surface demonstrated here. We cannot elucidate with the present data the biological and biogeochemical effects associated, particularly the spatial magnitude of both mechanisms need better quantification for such a comparison.
Future exclusion of fishery in the area will provide a test field in which the persistence of peaks may be tested and their origin confirmed."

We added literature that you suggested (De Borger et al. 2021; Depestele et al. 2016; Mestdagh et al. 2018; Tiano et al. 2022; van de Velde et al. 2018) and the following citations:

Epstein, G., Middelburg, J. J., Hawkins, J. P., Norris, C. R. and Roberts, C. M.: The impact of mobile demersal fishing on carbon storage in seabed sediments. Glob Change Biol. 2022; 28:2875–2894.

Forster, S.: Spatial and temporal distribution of oxidation events occurring below the sediment water interface. P.S. Z.N.I: Marine Ecology, 17, 309-319, 1996.

Gilbert, F. Hulth, S., Grossi, V., Aller, R.C.: Redox oscillation and benthic nitrogen mineralization within burrowed sediments: An experimental simulation at low frequency. Journal of Experimental Marine Biology and Ecology, 482, 75-84, http://dx.doi.org/10.1016/j.jembe.2016.05.003, 2016.