

## Reply to RC2 (*Responses in italics*)

### General comment:

This paper presents data from a 3D coupled numerical model that is used to quantify the major impacts of bottom trawling on organic carbon and macrobenthos stocks in North Sea sediments. The authors simulate six years of trawling activity and consider four management scenarios in which trawling effort is redistributed from areas inside to areas outside of trawling closure zones. Overall, the paper is well written, but in specific sections, the authors' reasoning is difficult to follow because the manuscript lacks of details about how the data has been treated/modelled (please, see some examples in the specific comments). Some of the implications are well sustained by the presented data, but in some sections, the paper becomes speculative. The authors should try to modulate the conveyed messages and not oversell their ideas if they are not well supported by the presented results. Nevertheless, this is a very nice scientific contribution addressing the spatial impacts and the transport and fate of the resuspended C in the heavily trawled North Sea, which could serve as inspiration for similar future modelling exercises - currently lacking in the scientific literature- and that will help us to properly understand bottom trawling impacts at regional scales and to constrain global estimates.

*Thank you for the constructive comments that have helped us to clarify and improve our manuscript.*

*We will extend the methods section to include more details on the model description and adapt the results and discussion sections to communicate the limitations of the study more clearly, and to distinguish the explanations of the model results from the conclusions drawn from those results.*

### Specific comments:

L 95-100: These coupled models account for Hydrodynamics (SCHISM) sediment dynamics (MORSELFE) and for interactions of OC and macrobenthos in the sediment (TOCMAIM), but what about the C remineralization? How it has been addressed? Perhaps something on this regard should be mentioned here, at the beginning of the Methods section.

*Carbon remineralization is included in the TOCMAIM model. We will add more information about this to section 2.1.*

L 109-110: It is weird to me to see the three different OC pools based on their bioavailability and degradation rates (fresh, semilabile and refractory) being considered as sediment classes, at the same level than inorganic particles (sand, silt, and clay), and see afterwards in Table A1 that all three OC pools, in term of their sediment dynamics properties, have been associated to the silt class. This is mentioned later in the manuscript (L 183) but to follow how the model has dealt with the OC resuspension, perhaps it should be clarified first here.

*This information will be added to section 2.1 along with a justification for treating OC as a silt-like fraction. This simplification will be added to the Limitations section (4.5) along with the general discussion of uncertainties of sediment dynamics in the model.*

L 140: Mention here from which period the daily time series of trawling effort from the Global Fishing Watch dataset were extracted. It is introduced later (L 150), but the reader should know it before, otherwise the paper becomes “mysterious”.

*Added (2015-2020).*

L 149: It is unclear why the authors chose the simulation period of 2000-2005 and the daily fields (of GFW trawling effort?) of 2015-2020 averaged; and it is even less clear how the scaling according to the annual historical landings of demersal fish reported in ICES was conducted afterwards. Perhaps it would have been easier and more realistic to simulate the period of 2015-2020 using the GFW trawling effort, since the simulation period of 2000-2005 based on historical landings could be spatially biased (despite the Couce et al. (2020) findings). In any case, this point should be clarified, regardless the reason behind it. Also, later in the paper, the simulation period of 2000-2005 is used as a reference (REF), but perhaps it would be good to mention this already here, and perhaps restate it or in the introduction of the Management scenarios (section 2.4).

*We agree that the choice of time periods should be more clearly motivated and will modify sections 2.1 and 2.2 to clarify our approach and describe the scaling method more precisely.*

*“Reference” was not used to indicate the time period, but rather to indicate the simulations using the actual trawling effort (in contrast to the modified effort used for the management scenarios). This will be changed to avoid confusion (see our response to the comment on L 256 below).*

*While we agree that simulating more recent years would be appealing, there are several conceptual and technical reasons why we chose 2000-2005 as the study period, which we will make clearer in the manuscript:*

- 1. Fishing effort estimated by GFW increases from year to year, but we believe that this increase does not reflect an increase in actual fishing effort. Instead, this effect is caused by the increasing coverage of AIS-based vessel data. This is evidenced by the comparison to the (VMS-based) ICES data, showing a large underestimation in earlier years and subsequent gradual convergence with the ICES effort. Therefore, using the GFW data “as-is” would introduce a strong bias. Our method of averaging and scaling is meant to more closely match the ICES effort.*
- 2. Whereas fishing pressure has remained relatively stable in recent years, the chosen model period 2000-2005 contains strong interannual variations in fishing effort in the North Sea, which allows to more clearly discern the effects of different levels of fishing effort.*
- 3. The atmospheric forcing dataset (Geyer, 2017), with which the model has been validated (Kossack et al., 2023), does not currently extend past 2018. There are other atmospheric models that could be used, but this would require complete re-calibration of the model, as hydrodynamic models are sensitive to changes in atmospheric forcing.*

L 183-185: In the paper, only the silt content is considered to estimate the resuspension rate of trawlers, but, generally, OC content increases in muddy sediments that are finer than silts. What about the clay fraction? Something should be mentioned on this regard.

*The term “silt fraction” is perhaps misleading here, since in the original formula of O'Neill and Summerbell (2016) for trawling resuspension, “silt fraction” encompassed all sediments <63 µm in diameter, including the clay fraction, i.e. the total “mud content”. The mud content is also what we use in our estimate of trawling resuspension, so it is consistent with the resuspension formulation. The phrasing will be modified to clarify this. Regarding OC content, note that it is set independently of grain size according to sediment maps (Bockelmann et al., 2018), though it is true that OC content tends to increase with finer and more cohesive particles.*

L 256: Define here what is exactly the reference simulation (REF). This is the first time that this acronym is used.

*Rephrased for clarification: “Six simulations are carried out for 2000-2005 using different distributions of trawling pressure: a baseline simulation (BASE) using the actual trawling distribution and representing the status quo, serving as a reference to which the remaining scenarios can be compared, one scenario without ...”*

*Note that in accordance with the other reviewer’s comments regarding the potentially misleading use of the term “reference”, we will change it to “baseline“ (BASE).*

L 340-344: The Figure 5, illustrating the change in average trawling resuspension and erosion rates, is hard to follow if previously the corresponding maps of redistribution of trawling for each scenario (similar to the map in Figure 3) are not shown. Otherwise, the authors skip the illustration of one critical step of the computing process, which is the redistribution of trawling effort. The limits outlining the areas of trawling closure zones are hard to discern, and in some maps it is difficult to identify if the areas are inside or outside the lines. Perhaps the areas should be filled with a hatched pattern. Additionally, the time constrain is not mentioned in the figure caption.

*We will replace Fig. 5 with a figure showing the trawling effort for the scenarios and indicate the closure areas more clearly.*

L 390-394: The same as in Figure 5. It would be desirable to see the trawling effort maps before presenting the average differences of changes in OC fluxes in Figure 7, and also the areas of trawling closure zones should be hatched, since they are hard to discern.

*We will indicate the closure areas in Fig. 5 and Fig. 7 more clearly.*

L 430-432: This sentence needs a proper reference to support such a strong statement. This is an example of the speculative sentences found throughout the text.

*This sentence is meant to explain what is occurring in the model, but was not intended as a general claim. Several sentences in the discussion and conclusion sections will be re-phrased to distinguish between explanation of the model results from the conclusions drawn from those results more clearly.*

L 444-445: Macrobenthos biomass responses are shown for the first time in the Discussion section, while they should be included in the Results section.

*The results for biomass will be included in the results section.*

L 516-579: I miss the Model limitations subsection some paragraph dealing with the need to improve the computation of the sediment and C resuspension, transport and re-deposition processes, which have been treated quite simplistically in this modelling exercise. To me, this is a key aspect, since most of the distribution maps and the computation of the C fluxes caused by trawling largely depend on this parametrization. Besides, several of the listed “limitations” on this subsection are not inherent of the models used (SCHISM, MORSELFE and TOCMAIM), but instead they are aspects that could not considered or addressed in the paper using these models. Perhaps the title of this subsection could be renamed as "Model limitations and unaddressed processes/mechanisms".

*We will add a paragraph discussing the simplification of sediment dynamics in the model and associated and uncertainty and renamed the subsection to “Study limitations”.*

L 582: Define the period during which the daily time series were generated.

*Added (2015-2020)*

L 585: Again, the period of the six consecutive years is missing.

*Added (2000-2005)*

L 706: There is no mention to the availability of the code of the MORSELFE model.

*MORSELFE will be added to the code availability section. It is integrated as the sediment module within SCHISM, so it is available at the same source.*