

Interactive comment on “Organic biomarkers in deep-sea regions affected by bottom trawling: pigments, fatty acids, amino acids and carbohydrates in surface sediments from the La Fonera (Palamós) Canyon, NW Mediterranean Sea” by E. Sañé et al.

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I enjoyed reading this paper; I think it adds to the literature in terms of the effects of habitat disturbance (by fishing) in deep water. I definitely recommend publication and find little of a substantive nature to criticise in the manuscript in terms of the analysis presented.

However, I think the paper could be stronger by being more focused. To me it is not

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clear if the focus of the paper is on fishing impacts on biomarkers or on the utility of biomarkers. I would recommend the former as the latter has been covered in the literature cited in this paper.

I think it is important to be clear right from the start why only measurements in the upper 5 cm of the sediment are presented. Is this simply a sampling issue or related to the expected effects of the disturbance? I suspect this is only a sampling issue and so the focus of this study is really on surface sediment and changes in biomarkers. Also in the regard, I would add in a line or two indicating what the animals do in the sediment (bioturbation) with respect to mobilising and transforming biomarkers. Given the study focuses on a shrimp fishery, I assume these animals are doing some work on the seafloor.

I am a little disappointed with the referencing in the paper, it would be better to include some of the early work on fishing impacts in soft sediments. I also think citing Collie et al 2000 is inappropriate – this is a case of meta-analysis with no reality check. Simply put how can intertidal cockle populations possibly be the most impacted benthic community by fishing? There are plenty of other reviews that make your point and in fact relate to your study more closely (I have cited the original work in Thrush, S. F. and P. K. Dayton. 2002. Disturbance to marine benthic habitats by trawling and dredging - Implications for marine biodiversity. *Annual Review of Ecology and Systematics* 33:449-473. And Thrush, S. F. and P. K. Dayton. 2010. What can ecology contribute to ecosystem-based management? *Annual Review of Marine Science* 2:419-441.) If you really want to cite Collie and colleagues then this group did update that review – I think Kaiser was the senior author, but I do not have the reference.

Study design, please specify why the control site was selected, is it not fished as there are no shrimps there? Or simply too far from port (an economic refuge)? Pull in ancillary information on how similar the habitats are and lack of disturbance indicated in the control area from acoustic imagery if you have it.

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Statistics, I do not understand the choice of procedure here. It seems to me you have a two way ANOVA design (location and time and interaction term) not two one-way ANOVAs. But actually you would be better to use a generalised linear model to tease out site and time differences in a regression based analysis. The uneven sample size is a problem with the ANOVA approach. When the difference in sample size between sites and times is large the only solution if you analyse this way is via randomly dropping replicates from the data set. Looking at the tables I am not sure this is a major problem but a more gradient based approach would be far better both in terms of the design you have and dealing with normality etc (just use the right error structure). I also suggest discussing the multivariate analysis first – this is your strongest result and I think most interesting analysis. Not all the multivariate analysis discussed in the methods seems to be presented in the results – there is some double up here in the analysis you are doing and its purpose is sometimes lost on me, so I suggest you stick to the important and major findings. This will obviously change the results and discussion. Whatever you do in this regard when you do not see a significant difference you can ONLY say you did not detect one and if you want to go further than that (as you currently imply in the Discussion) you need to perform some power analysis.

There are some very long tables in this paper that I would suggest are better placed in appendices.

Discussion, change the first paragraph to focus on what you found rather than what you did. Assuming this paper is more about results than a methods paper. In the second paragraph there is a lot of detail on what happens to these macromolecules in the water column, unless the water column is different over impacted vs control sites this is not that relevant. If the oceanography is different then you have a problem defining your controls in a non-confounded way. I would focus this more on what is happening on the seabed and expand on the potential role of different organisms in affecting rates of change in macromolecules. Note also the text in this paragraph gets a bit repetitive.

Finally, the conclusion is a bit weak, this will obviously depend on the focus of the paper

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but some more general statements about either changes in sediment biogeochemistry induced by fishing disturbance or something on the broader ecosystem effects and potential impacts on sustainability.

Minor points (mostly suggestions on English) As there are no line number sin the manuscript I will copy in enough text that the authors' can search and find the text I am referring to.

pulling heavy fishing gears – change to gear

Over the last decades, this commercial activity – change to Commercial fishing has. . .

and their surroundings are often targeted – omit and their surroundings

can trigger sediment gravity flows – later in the paragraph you say - altering the morphology of the canyon flanks as a result of chronic reworking – This seems incongruous how can a landslide be a chronic effect (at least on ecological time scales).

the principal factors – change to important factors (organic matter is not necessarily THE most important)

Organic matter indicators have been already used in other works – change to Organic matter indicators have been already used to address

a pool of four biomarkers – delete a pool of four

indicators of labile material (Lee et al., 2004) - this might be useful here too Mayer, L. M., L. L. Schick, T. Sawyer, C. J. Plante, P. A. Jumars, and R. L. Self. 1995. Bioavailable amino acids in sediments : a biomimetic, kinetics-based approach. Limnology and Oceanography 40:511-520.

Univariate and multivariate statistical tests were carried out on all the analyzed samples. – delete

Univariate statistical analyses were performed with the Statistica software v.5.5. – put

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at the end of the paragraph

Multivariate statistical analyses were also performed. In this case, the – Omit

Primer software v. 6 was utilized (Clarke, 1993; Clarke and Gorley, 2006). – move to the end of the paragraph

Before multivariate analysis, data were pre-treated by a transformation to downweight contributions from quantitatively dominant macromolecules – How? What is the transformation?

distances between couples of samples – here and throughout refer to pairs of samples not couples.

favour, in fact, the preservation – change to favour the preservation

the sinking velocities – change to the sinking velocity

interrelated factors, like oxygen penetration – change to interrelated factors, including oxygen penetration, bioturbation, sediment density and grain size.

This agrees with a previous work carried - change to This agrees with previous work carried

Nevertheless, the distribution of LC-FA in the study area suggested high quantities of refractory material in spring and in the untrawled region, letting us doubt again about the utility of refractory compounds (i.e. carotenoids, MC-FA and LC-FA) as biomarkers in the study area, also due to the considerable lateral component of particle fluxes (Martín et al., 2006). – Sorry but I am not sure what you are trying to say here. Do you mean: Nevertheless, the distribution of LC-FA in the study area suggested high quantities of refractory material in spring in the untrawled region. This further limits the utility of refractory compounds (i.e. carotenoids, MC-FA and LC-FA) as biomarkers (Martín et al., 2006).

whereas MC-FA and LC-FA are too refractory to be used as biomarkers – I am a bit

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confused by the text here, sediment grain size has previously been used as a marker of habitat disturbance by fishing and this is far more conservative than a biomarker. I think this need to be reconciled.

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