

Interactive comment on “Deep-sea benthic ecosystem collapse and recovery after an intense Dense Shelf Water Cascading event” by A. Pusceddu et al.

Anonymous Referee #1

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General comments

This is a very interesting study on a modern field of marine ecology and geology, tackling also into the field of climate change and its impact on biodiversity and ecosystem function. I believe the manuscript fits nicely within the scope of the journal and will be highly cited in future studies in the field. The data presented provide evidence of a relatively new idea, namely that the influence of dense shelf water cascading events on

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deep-sea ecosystems may be much stronger than previously thought. In the present manuscript particularly, the authors provide, to my knowledge for the first time, an example of how a meiobenthic community collapsed and recovered exactly due to such a major cascading event. The paper is technically sound and the title clearly reflects its content. The abstract provides a concise and complete summary and the manuscript is written with clear and fluent English. However, despite the above positive comments, I have a few reservations, particularly on the way some of the data were analysed and presented, thus I believe that the manuscript could be largely improved if the following specific comments will be taken into account.

Specific comments

My first point of concern is with the way the authors formulated and tested their hypothesis. In particular, as the authors stated in their Introduction, they set out to test that the cascading event affected the biodiversity and functioning of the deep-sea benthic ecosystem. However, while they provide enough data and discuss in detail how the cascading affected the biodiversity, they don't provide any data nor do they discuss the second part of their hypothesis, namely how the cascading affected the functioning. For this, it would be necessary to provide an independent measure of functioning in the canyon and the deep basin. I strongly suggest to either include, if of course available, such data and analysis in their Results and Discussion or, in case this is not possible, reformulate their hypothesis and adjust the Discussion by excluding the part on the effects on ecosystem function.

An other part where the manuscript could be largely improved is the part on the effects on nematode diversity. There is still great potential in the dataset, which, in my opinion, has not been fully explored. More specifically, while the authors provide convincing evidence for a significant decline in the number of major taxa and nematode species

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and a consecutive recovery, they do not discuss how the cascading affected the nematode community structure. I think with a little more effort, this can be easily achieved since the data on the nematode community structure are available. It would be very interesting to know how the cascading event shaped the nematode community structure, which particular species were the most tolerant or vulnerable to the disturbance, which species re-occurred after the recovery, if these species that re-occurred were the same as those that were extinct and if any of these patterns was consistent along the depth gradient. All these are important ecological questions which, if discussed, would greatly improve the manuscript.

A final point of confusion is the fact that the authors present and discuss their results in light of the cascading event that occurred in 2005 (i.e. all graphs, analyses and comparisons are performed focusing on the values found in April 2005 and comparing these values with the values found just before and after that time point). However, as the authors state on Page 17858 and Line 5, according to Palanques et al. (2012), there was another cascading event in 2006 in a nearby canyon. Taking into account the proximity of the two canyons, this event in 2006 should have equally impacted the CCC canyon. Thus I wonder why the authors don't consider their 2006 sampling point as another cascading event as Palanques et al. (2012) suggests? If indeed there was a cascading event in 2006, then one would expect that a similar to the 2005 event collapse and recovery would appear just before and after this sampling point as well.

Technical comments

1. (Page 17863, Lines 5-6): Here the authors say that the calculated b-diversity between sites, sampling times and depths but later on Page 17864 and Lines 2-3, they say that turnover diversity (which, if I understand this correctly is practically the same thing) was estimated between sampling times and depths only.

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2. (Page 17865, Line 3): I believe that (Fig. 2c) should read (Fig. 2b,c) since it refers to the bioavailable C and bioavailable fraction respectively.
3. (Page 17865, Lines 12-14): This statement is not correct for biomass since, as can be seen in Fig. 3b, the biomass in Apr. 2008 is significantly lower compared to all other sampling events.
4. Statistical analyses: The PERMANOVA analysis is either incorrect or the authors did not explain well what they did. In a two-way PERMANOVA, similar to its two-way ANOVA analogy (see the PERMANOVA manual), when the interaction term is significant, the usual advice is that you should not test the effects of the individual factors. For example, it would be misleading to examine the individual factors and conclude in page 17865, and lines 25-26 that "nematode biodiversity, expressed either as species richness, expected species number [ES(100)] or as Shannon's (H) index, is significantly lower". As all statistical textbooks advise, what you can do, if the interaction term is significant, is look at each factor separately, using a one-way ANOVA (or PERMANOVA in our case).
5. (Page 17865, line 26). I think Table 3 should read Table 4. Same on the next page, Table 5 should read Table 4?
6. (Page 17867, line 6). "...to the deep-sea margin ..." should read "... to the deep-sea basin ..."

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