

Interactive comment on “Short-term fate of phytodetritus across the Arabian Sea Oxygen Minimum Zone” by J. H. Andersson et al.

Anonymous Referee #1

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Short-term fate of phytodetritus across the Arabian Sea Oxygen Minimum Zone J. H. Andersson et al

Referee comment

The manuscript describes a series of experiments studying the fate of phytodetritus at the seafloor at depths of 140–1850 m across the Pakistan margin and within and below the oxygen minimum zone. The scientific questions addressed in the paper fit well into the scope of BG and the ^{13}C -pulse chase experiments performed are adequate to address the question and state-of-the-art.

Specific comments

To maintain ex situ the very low oxygen concentrations (< 10 μM at 300–1000 m

stations) occurring in situ, a very accurate oxystat system is crucial and this is not a trivial task. However, no information about the system, its performance and limitations is given. Instead, the authors refer to a publication about this oxystat system which however is in review and thus not accessible. Consequently, it is very difficult to assess the significance of the data presented and the manuscript would benefit tremendously if more information about the system and one or two examples of oxygen recordings throughout experiments at very low oxygen concentrations were included.

The introduction led me to believe that the comparison of in situ and ex situ processes was a major goal of this undertaking. I fully acknowledge the difficulties related to in situ work and the resulting smaller number of samples, but was disappointed to see that the authors do not seem to exploit fully the available data from the two successful lander deployments. Fig. 4 gives the amount of ^{13}C respired in the in situ experiments, but for better comparison these two time series should be integrated into Fig 3, which depicts ^{13}C respiration in ex situ experiments. The authors state 'relative differences between results were similar'. But ex situ results were 3-5 times higher than in situ results, and this should be made clear and discussed. Interestingly, the difference is only slightly more pronounced at the deeper as compared to the shallower station, making depth/ depressurisation an unlikely contender as a cause. As sample size differed largely, this could possibly in part explain the deviation? In addition, I have not been able to identify the other results from the in situ incubations, namely incorporation of tracer into bacteria, foraminifera, macrofauna. Why are these data not included into this manuscript? It would be very valuable to compare the carbon flux through the community measured in situ vs ex situ; and possibly allow conclusions with regard to the importance of pressure effects for the different taxonomic/ functional groups.

I was disappointed that the paper remains very descriptive throughout. Setting out to establish carbon budgets I had hoped to see some more quantitative analysis, if not modelling of C flow. I am sure the readers of BG would be a welcoming audience, so perhaps this could be added to a revised version?

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