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Biogeosciences Discussions

Interactive comment on "Short-term changes in particulate fluxes measured by drifting sediment traps during end summer oligotrophic regime in the NW Mediterranean Sea" *by* J. C. Marty et al.

Anonymous Referee #1

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General comments For decades, the sea bottom, especially when the deep ocean was of concern, has been considered as a relatively stable environment. In the last 30 years this figure has completely changed as evidences of from seasonal to interannual changes in the responses of benthic ecosystems to the input of material sinking from the upper water column layers have been repeatedly provided in the literature. Most of these evidences were based on sinking rates of organic material within traps measured integrating not less than 7-15 days of collection. Basically, those intervals are chosen to scale-up the sampling of particles to the expected timing of response of the benthos, and as such have helped the scientific community to ascertain relevant patterns of response and important ecological linkages. The topic investigated by

Marty et al. is therefore a new step towards the recognition of the differential ecological importance of downward fluxes of organic particles to the sea benthos. In doing this, they provided some evidence highlighting either that 1) these fluxes exhibit a short-term variability which appears to be higher than the one observed on longer temporal scales, because of different forcing factors (including wind forcing at the sea surface) and 2) the estimates of primary production export to the sea bottom might differ if calculated on different temporal scales. The manuscript is quite well written and of sure interest for the Biogeosciences audience. The results are rather clear and the discussion is aligned with the initial aims of the study. As such this paper deserves to be published on Biogeosciences. However, the authors must acknowledge the presence of additional sources of short-term variability that they did not mention nor measured. These possible biases indeed might change the overall picture they figured out in this study.

Specific comments On one hand, it might be expected that on very short-temporal scales the rates of sedimentation of organic particles in the topmost layers of the water column might change rapidly, as particles with different shape, volume and density sink with different velocities. On the other hand, however, it must be stressed that the identification of a so large short-term variability in the quantity and composition of organic matter fluxes observed by Marty et al. in the NW Mediterranean opens new perspectives in the assessment and interpretation of the responses of the smaller benthic fauna and prokaryotes living on the superficial sediments of the sea bottom. However, the general significance of this work is somehow weakened by the limited attention the investigators paid to other possible concurrent sources of variability that might have influenced the variability of fluxes as measured at the short-term scale they adopted. One of the most challenging trials of environmental sciences in the last decade has been investigating how much of a cause-effect relationship is affected by the different spatial and temporal scales of observation and assessment. This task has found large attention and applicability in intertidal science, because of the relatively easy possibility of performing manipulations and complicate experiments in the field. This, for obvious

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technical and economical reasons, is more and more difficult to do in oceanic studies. Despite this, I would like to draw the attention of the authors and the Biogeosciences audience on the fact that this study lacks of any statistical appraisal of the variability observed. I do agree that the changes observed are quite large and surprising, but, at the same time, the lack of actual replication in the measurement of fluxes weakens the broadening of these results out from the study area, season and period. As far as I can argue, the standard deviations (are they sd?) reported in Table 1 are derived from replicate measurements on different aliquots extracted from the same cup after splitting. As such, the variability of fluxes between deployments is a rough representation of the variability integrating 6-hours of deployments, as one trap only was used. A more correct sampling design - including the synoptic deployment of at least two different traps - would have provided true replicates accounting for a more proper assessment of short-term variability of particle fluxes. Substantially, the variability the authors observed within a trap between deployments might be biased by the splitting of samples at a level that cannot be ascertained with the present data. A replicate trap deployment would have provided likely more robust support to their results (or not), but this possible bias should at least be acknowledged. Another important potential source of variability includes that fact that the variability they observed might be also the result of the trap drifting. In this sense I have a major doubt: if the trap was drifting the variability they observed is possibly determined by “spatial” variability of fluxes instead of only short-term “temporal” changes. To cope with this and to be more compelling to the point, the authors should consider to take into account this source of variability testing for the effect of the actual drifting (i.e. actual distance run by the trap during the deployment) on the variability they observed. I do not know and whether this is feasible or not now, but this should eventually acknowledged and considered in an eventual replication of the experiment. Again, I found to be quite inferential the linkage between the variability of fluxes and the wind forcing at the sea surface. This needs to be more properly accomplished by means of statistical analyses possibly substantiating or disconfirming this.

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Interactive Comment

Technical comments The authors repeatedly refer to temporal changes as to “evolution”. I would avoid using this term specifically because of the “short-term” temporal nature of their investigation. Some figures are not readable at all (e.g. Figure 3: P data are almost unreadable) Throughout the manuscript the authors refer to “quality” of the fluxes , but this term needs to be specified (e.g. nutritional quality?). A comparison between lipids in trap material and those in suspended material is made referring to partly unpublished data. I would avoid this as the linkages/similarities are not appreciable actually by the reader.

Interactive comment on Biogeosciences Discuss., 6, 575, 2009.