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Biogeosciences Discussions

Interactive comment on "Short scale (6 h) temporal variation of sinking fluxes of planktonic and terrigeneous lipids at 200 m in the NW Mediterranean Sea" *by* L. Méjanelle and J. Dachs

Anonymous Referee #5

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The manuscript investigates the short term variability in lipid fluxes from a drift station at 200m depth in the frame of a BG/BGD special issue on "Short-scale temporal variability of physical, biological and biogeochemical processes in the NW Mediterranean". The authors present an impressive dataset of molecular biomarker composition and fluxes at an unprecedented temporal resolution.

However, the data treatment, discussion and interpretation of the results are somehow in mismatch to the analytical effort and the data obtained. The abstract and introduction promises more than what is discussed in detail.

For example, the authors state that "physical constrains (they mean constraints?) ex-

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erted by carrier particle dynamics" (p.1230, l. 16-17) is the main driver of the short term temporal variability of biomarker export fluxes (no data provided e.g. on the particle composition and fluxes), and that "the coupling between primary productivity" (no data provided) "and biomarker export shows significant changes on scales of days and even of 6h (p.1230, l. 26-28), But, a few sentences above the authors also state somehow oppositional that "biomarkers exported.....record processes averaged over a larger period than the sampling frequency" (p.1230, l.22-23).

The manuscript is introduced in the context of physical and biological forcing as well as hydrographical, environmental and atmospheric variability, but the data presented here are not directly compared to or shown with any of such other data.

Also, I could not find any "comparison of the different markers with organic matter and other bulk indications", although this is promised (p. 1231, l. 27).

Without inclusion of any additional data, the reader will be virtually lost with a presentation and description of individual lipid fluxes.

The authors are aware the fact that they have analyzed samples at the lower detection limit but except for alkenone derived SST there is no discussion (or data) if at least part of the variability could be due to this. This could have been easily checked by a replicate analysis.

It is stated that "recovery of the selected lipid classes was validated using standards" (p. 1233, l.16-17), recovery rates are not provided and there is also no information if the standards where applied at same concentration levels than the target compounds. Although the formalin solution used for sample preservation was found to contain some hydrocarbons, there is no information if a blank of the whole analytical procedure was conducted, which I consider essential when dealing with such samples.

Unfortunately, the analytical procedure did not include a saponification or methylation step for analysis of fatty acid compositions that (a) usually comprise a major fraction

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of particulate and dissolved organic matter and (b) can provide valuable information about phyto- and zooplankton dynamics and trophic interactions.

The site description (section 2.1, p. 1232) is rather deficient and not even includes the year of sampling (2004) and a reference to Table 1 (mentioned nowhere else). This is the place where most of the additional parameters could have been introduced, but this is not the case. Although the authors mention that the sediment traps deployed where drifting, this needs more attention. In fact, the results correspond to samples collected from 31 different stations along 24 days with lags between the sample series A-D and it is unclear if this spatial variability in sample location can account for at least part of the variations observed with the data. The reliability of the used wet suspension divider remains unclear; a duplicate analysis of two subsamples would have done the job.

I could hardly find a rationale for the Figures presented in the manuscript, and some of them are either referred to in the wrong context or not at all. Fig. 2 shows the average composition of sterols (for what?) but is used within the context of alkenones (p.1237, l. 21) and biomarker fluxes (p. 1246, l.11). Fig. 4b and 5a are not mentioned in the manuscript, and Fig. 5a does not include the flux of total C, although mentioned in the caption.

The x-axis of all figures is not a real time axis, because all samples are equally spaced and the lags between sample series are not really obvious.

Table 3 and Fig. 3 (A) present extensive data on the alkenone compositions. However, these data are neither described nor discussed in the text; rather the sum of these compounds is used. I welcome these data, they could hold a potential for an own story, if reliable in face of the low concentrations. But if they are not used for a more detailed discussion, they are meaningless in the present context of the paper and should be reduced to a simple sum. For example, what is the implication of the ratio C37 alkenones/C37:3 alkene provided in Fig. 3(A)?

In its present form, I can not recommend publication of the manuscript. However, the

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data have potential for an interesting story, but the manuscript needs reorganization and rewriting as well as the inclusion of additional but essential background data. I would also welcome to see additional figures (supplement?) with some "real" chromatograms of the analyzed lipid fractions.

Minor technical comments:

p.1230, l. 6: seasonal

p. 1232, l. 14: targeted

p. 1233, l. 15: purchased

p. 1236, l. 11: Sinninghe Damsté et al.

p.1238, l. 10: beginning; l. 14: targeted

p.1239, l. 27: less

p.1240, l. 20: pigment; l. 29: sterols

p. 1247, l. 15 variability

Table 3: (RU) not explained, applies also to Fig. 1, and 3A. Table 4: include the flux of total n-alkanols, diols, sterols and steroidal ketones at the bottom of the table (cf. Table 2). Table 5: provide the units of the given fluxes. Table 6: how is "significant correlations" defined? Nearly everything is bold in this table.

Fig. 3 and 5: insert A and B to the respective subfigure.

References: Volkman et al., 1998 and Wakeham et al., 1991 are not cited in the manuscript.

Interactive comment on Biogeosciences Discuss., 6, 1229, 2009.