**Biogeosciences
Discussions**

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Comment

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 #1

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The manuscript describes the short-term variability of fluxes of various organic compounds in the NW Mediterranean Sea which were used to study the importance of planktonic versus terrigeneous sources. The paper addresses the important issue of the short-term variability of fluxes (hours to days) of organic materials in the marine environment. This variability is far from being understood.

To do so, the authors have measured a large number of lipids in partly small sample amounts which of course is a big challenge. As a consequence, the record for some parameters is incomplete or the results are not reliable due to the detection limits (e.g. for long-chain alkenones, see comments below). I would have used 2/10 of each split (if

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possible) to increase the amount of material available for analysis. If not, I would have mixed two samples together to achieve more reliable results. Even so, time resolution would be 12hours, still exceptional.

The analysis of lipids and their utilization for the interpretation of particle sources on very short time scales has a high potential for an exiting story. However, this can only be achieved by: a) selecting important species- or process-specific compounds out of a series of the available ones, b) by showing and discussing additional important flux parameters (e.g. organic carbon, carbonate, biogenic opal, total mass) or chlorophyll standing stock/primary production and, finally, c) by providing sufficient background information (ocean and atmospheric circulation, river input etc.) so that the lipid data can be discussed in the entire context. Unfortunately, this was not done. Only at the end of the discussion (pages 1246-1248), the findings from lipid studies are discussed with some important other parameters (POC, but not shown) and some background information (chlorophyll, stratification, atmospheric input etc.; nothing shown. I want to give one example to illustrate the major shortcomings of this paper. Some lipids may provide evidence for some contribution of diatoms which makes sense considering the general environment of the study area. This can easily be tested by the determination of biogenic opal or, even simpler, by microscopic analysis. If this is not done, the interpretations from the lipids alone remain highly speculative.

The major aim of this study, to get a least some insight into the short term variability of export fluxes is far from being achieved. Instead, in the discussion, general comments are provided to explain the short-term variability (page 1247: lines 15-16), e.g. about the dynamics of aggregation (nothing has been said about the complex processes of aggregation or disaggregation before). If the short-term daily variability cannot be explained by the lipid and the bulk mass flux parameters combined with the available background data, these data can be used to discuss the longer-term changes, e.g. the summer-fall transition during increasing wind stress and the breakup of summer stratification (samples from ABC series and D series). Even this has the potential for

an interesting story.

The introduction chapter is not an introduction into the theme of the paper at all. What is the state of knowledge with respect to the short term variability of mass/lipid fluxes in comparable environments, what are the problems arising from other findings, what is the aim of this study, which lipid compounds are promising to solve the questions? Furthermore, there is no introduction to the general setting of the study area, no information about hydrography, atmospheric circulation or inputs or the potential influence from rivers. This information is essential to evaluate the terrigenous input into the marine environment.

Section Material and Methods is rather long and provides detailed descriptions of lipid extraction and analysis. Standard methods are at least partly applied and therefore this section could be shortened by providing important citations. Section Results appears to be more a discussion and mixes own findings with results from other investigations. The SST reconstructions provided by the alkenone unsaturation index (page 1237) seem to be partly unrealistic (15-25°C temperature change on rather short time scales). If this is due to low sample amounts, these data should be omitted (page 1238, lines 1-2). Almost all figures contain too much information and are difficult to read. As I mentioned above, some important lipids should be selected, plotted and discussed in detail, others could be omitted. The time scale (y-axis) is difficult to read and seems to be not linear; where are sampling gaps, where are gaps due to low fluxes?

The conclusions also remain vague and unclear (1248: e.g. lines 15-16). What do the authors mean with ....the short term variability of export fluxes depends on physical constraints exerted by carrier particle dynamics?......

In its present form, the ms cannot be accepted for publication in BG. It has to be completely rewritten, reorganized and shortened in some sections.

Minor comments:

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Page 1230, line 25-26: primary production data is not provided, if available, it should

Page 1243, line 25: phytoplanktonic

Page 1246: TC (=total carbon=organic + inorganic carbon) here obviously means POC (particulate organic carbon) which is misleading, the authors should apply the general nomenclature

Page 1248, line 16: strongly

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