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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 #2

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This paper discusses the remarkably high temporal variability in lipid and organic matter fluxes in the NW Mediterranean as measured by sediment traps deployed at 200 m. It is rare to find data for samples collected over 6 hour intervals and so the information will be of interest to the marine science community. However, the data sets do provide a challenge as to how best to interpret what is going on. The lipid proxies do provide some useful insights, but they are only part of the picture and the lack of information from other proxies, or even by direct microscopic examination of the material collected, severely limits the interpretation.

Also, as the authors admit, the amounts examined were small and this posed a con-

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siderable analytical challenge. It is unclear to what extent this might have affected the quantitative data, but one must assume that some of the variability is due to this factor. Additional variability could be introduced by the splitting system, by the need to remove swimmers, and simply due to varying trapping efficiency under different flow regimes. Since no data from replicate traps are presented the reader has no way of assessing reproducibility. Perhaps there are data available from other studies that indicate the degree of uncertainty that might apply here.

High variability might be expected if the flux is dominated by short-term feeding events during periods when phytoplankton and grazer populations changed markedly. However, the lipid distributions do not show such dramatic compositional variations and the low amounts of material collected is not consistent with fluxes of large faecal material. I think that the authors are probably correct in their assessment that the organic matter is considerably reworked before collection in the trap, even though they were only deployed at 200 m. The presence of the oxidation product cholest-4-en-3-one is consistent with this view. A number of such lipid degradation products are now known (see papers by J.-F. Rontani) which, if detected, could strengthen this story.

In view of these problems, I feel that the paper will need major revisions before it can be published.

Some aspects of the interpretation of the lipid biomarkers need a closer examination and some I can't agree with:

1. The hydrocarbon distributions are most unusual (e.g. even over odd dominance in some samples) and the occurrence of the compound identified as squalane is highly unusual. The identification of squalene needs to be checked and if confirmed I think that contamination or petroleum residues should be suspected. The latter can easily be assessed by running mass fragmentograms for m/z 217 (steranes) or 191 (hopanes). The fact that there are long-chain n-alkanes present showing a moderate odd-even predominance does suggest that some contribution from higher plants is present, even

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if mixed in with other sources. This is important since it bears on the likely source of 24-ethylcholesterol and 24-ethylcholesta-5,22E-dien-3b-ol.

2. In simple terms, the sterol show a high proportion of compounds indicative of zooplankton (cholesterol and some C26 and C27 sterols), phytoplankton (C28 and C30 sterols) and higher plants (C29 sterols). The authors attempt to go beyond this to propose more specific sources for particular sterols, but the reasoning that just because a certain species isn't present excludes that class of microalgae as a source isn't valid. I am surprised that diatoms are not considered to be the major contributor to the C28 sterol 24-methylcholesta-5,22E-dien-3b-ol given that it is abundant in many genera. If one looks at the alkenone:sterol ratio it is apparent that haptophytes cannot be a major source and this sterol is so rarely abundant in other classes of algae that I think the simplest explanation of a diatom origin is more likely to be correct here. The suggestion that cyanobacteria might be a significant sources of sterols is highly questionable. While it is true that there are many reports (as summarized by Volkman, 1986), it is now apparent that these are probably due to contamination (see Summons R.E., Jahnke L.L., Cullings K.W and Logan G.A., (2001) Cyanobacterial biomarkers: Triterpenoids plus steroids? Eos Trans. AGU, 82(47), Fall Meet. Suppl., Abstract B22D-0184). Even if they might be genuine components of cyanobacteria the amounts reported are very low and could not account for such high abundances of sterols such as 24-ethylcholesterol in these marine settings. While I agree that one must consider algal sources for 24-ethylcholesterol and 24-ethylcholesta-5,22E-dien-3b-ol I think that an origin from higher plants is reasonable in this setting. The authors could check whether their abundances co-vary with each other and with a long-chain alcohol and alkane.

Much of the results section is actually a discussion of the origins of the lipids. I suggest that the results and discussion sections could be combined.

Additional points:

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1. The text needs some attention to the English grammar and choice of vocabulary. I haven't provide a full list here, but some examples are: Page 1231. Line 6. 6-h not 6-h'. Line 9. biomarker not Biomarker; Line 11. causing not ensuing; Line 13. Suggest the proportion of the higher plant inputs increased as shown by the higher export fluxes of long-chain odd n-alkanes. Line 20. Suggest replacing studied situation with region; Line 20. insert of organic matter fixed by primary productivity; Line 23. Exported to; Page 1232. Line 5. aims to understand these forcings in the northwest; Line 9. Replace in this frame with Within this framework; Line 18. replace Simplifying with In simple terms;

2. page 1234. explain oven track mode and write out the temperature program on line 18. Replace atomic mass units with dalton; on line 29. On page 1235, insert temperature between oven and programs. Line 9, replace design with designate. Line 14, replace sight with visual.

3. The terms coccolithophorid, haptophyte and prymnesiophyte are used as though they are interchangeable. The coccolithophorids are a subset of the haptophytes and I would contend that alkenones are synthesized by haptophytes not just coccolithophorids since non-coccolith species also synthesize these compounds. The term prymnesiophytes is now replaced by haptophytes (see the book The Haptophyte Algae). Note that Haptophyceae has a capital H, but not haptophytes or other generic names for phytoplankton such as prasinophytes, eustigmatophytes etc.

4. There are some errors in the sterol nomenclature (page 1238 and elsewhere). The geometry of the side-chain double bond should be specified (it is usually 22E). On line 18 insert a hyphen after 24. On line 19, the sterol should be written as 24-ethylcholest-5-en-3b-ol or as 24-ethylcholesterol. The names of steroidal ketones need checking (e.g. footnote to Table 4). The ketone group cannot be alpha or beta; thus it should read cholestan-3-one and cholesta-4,22E-dien-3-one.

5. I think that there might be some value in presenting a few of the hydrocarbon distri-

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butions as histograms to better illustrate the variation in odd-even predominance.

6. The references need checking for typographical errors (e.g. Sinninghe Damsté not Damsté, Schulte not Sxhulte, Rullkötter not Rülkoter, epicuticular not epicutilar, proper symbol for UK'37, species names in italics, Acta not Ac., Blackburn not BlackBurn, hypereutrophic not hypereuthophic).

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