Biogeosciences Discuss., 6, S61–S65, 2009
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Biogeosciences
Discussions

Interactive comment on “^{234}Th measured particle export from surface waters in north-western Mediterranean: comparison of spring and autumn periods” *by* S. Schmidt et al.

Anonymous Referee #3

Received and published: 5 February 2009

This paper describes 234Th derived particle export at the DYFAMED site in the Mediterranean Sea during the spring of 1995 (previously published and discussed in Schmidt et al. 2002) and the fall of 2004. During the fall cruise, five dissolved and particulate 234Th profiles were collected over a four week period impacted by strong wind induced mixing events. Total 234Th measurements were limited in three profiles to the upper 60 m, with the remaining two profiles obtaining total 234Th measurements down to depths of 200-300m.

I admire the efforts put forth by the authors and believe that understanding particle export during storms is of substantial interest. Unfortunately, the new data is rather minimal. Given the quality of the measurements and model assumptions, it is further difficult to place the results in context of other particle export studies. As such, it is not clear to me that the data presented here is enough to merit publication. At the very least, errors on the calculations must be included, as well as a more thorough discussion of assumption weaknesses. I have several major concerns:

1) Data and models used: Dissolved 234Th samples were purified at sea using anion exchange chemistry. Unfortunately, the chemical recoveries were poor, only 20-60 precent. Particulate 234Th activities were also low and difficult to measure. Combined, this suggests that errors on total 234Th are quite high, likely to be in significant excess of the 5-15 percent reported by the authors for dissolved Th alone. Given the similarity in U and Th activities, and their associated errors, it is impossible to obtain even a 1-D Th flux model with any accuracy. Calculated 1D fluxes are some of the lowest that I have ever seen (even though integrated to only 60 m). No errors on these flux measurements are provided. Thus, it doesn't make sense to go through the trouble of developing a NSS model (which is integrated over 80 m?? why different depths?). Furthermore, when the NSS model is applied, it is only applied for the Sept 17 and Oct 3 profiles, even though data exists for Sept 24-25. No clear reason is given as to why. Is it because sampling between time periods is very short, and thus very close to error limits (see Savoye et al. 2006)? Finally, it is assumed that physical processes are minimal. With such low fluxes, and a clear indication that intermittent deep mixing occurred, this assumption is poor. I would argue that the low fluxes calculated with the 1D model are minimized, as it is likely that a significant amount of deep water in U-Th equilibrium was upwelled into surface waters (again - see Savoye et al. 2006). Can the authors constrain this input?

2)Derivation of POC fluxes. Again, no errors are provided. I also could not replicate what C/Th and what SS and NSS deficits were used in the POC determinations (Table 1 is incorrect in several areas??). In one case, I believe that Th:U disequilibria over the upper 60 m was converted into POC fluxes using an average POC/Th ratio found in

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suspended particles (> 0.45 um). This is significantly different than that typically used (see Buesseler et al. 2006). In fact, the C/Th ratios of 11.9-24.1 umol/dpm (averaged over the upper 60 m) are about a factor of 2-4 higher than those collected at 150-200 m using sediment traps. The authors also use sediment trap C/Th ratios of 2.6 to 4.2 umol/dpm, but it is unclear if they use this ratio with the 60 m or the 80 m SS or NSS deficits from U:Th. I would argue that that the authors are probably ok in doing this later application (sediment trap ratios with 60-80 m integrated Th deficits) as there is such little disequilibrium in the surface waters, but this needs to be made very clear. A caveat should also be added that this second method may miss deep chlorophyll maxima, which would add to the Th flux (the authors mention DCMs at 50-60 m and 90 m - are fluorescence data available? They should be shown). This entire section needs to rewritten and tables added for clarity.

3)Comparison of Th derived POC fluxes to traps: Comparing Th derived POC fluxes to the sediment traps are interesting, but this data is not presented, so validity is difficult to judge. Much is made of the differences found. I would argue, however, that the differences are not significant given the errors associated with all of the above assumptions. This section could therefore be considerably shortened.

4)Discussion. The authors do surprisingly little to compare the spring and fall events 8211; and why they differ. It is therefore not clear to me that the May data needs to be included? The authors could expand on this discussion (and keep the May data by adding information on differences in phytoplankton communities? Rates of Primary production? Nutrient availability? Maybe there was not enough time for the storms to induce a particle producing export event? Does dust deposition have anything to do with it?

Specific Points:

1) Page 148 top, why would U fall off the Chen line in the Med ? Please be more precise. How do the results compare with Chen et al and Pates et al. 2007?

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2)Page 148 models. Fluxes are VERY low. Therefore, advection and diffusion becomes increasingly more important. Physics are definitely important at the DYFAMED site - see papers from Cochran et al of the MEDFLUX group. The authors also state that significant mixing occurred during sampling. The assumptions need to be more justified.

3)Page 149. Please show profiles of Fluorescence, Temp/Sal? The storms suggest significant mixing occurred (so please show). If this is the case, then the advection term in the Th model should be significant and Th fluxes underestimated?

4)Page 150. The May 1995 data needs to be shown again in the paper as it is discussed in so much detail here. I am not sure this information is needed? A discussion of the difference and similarities between the two sampling periods is surprisingly minimal. This could be substantially expanded.

5)Page 151. It needs to be very clear which model was used with what C/Th ratio and why. Also, why were the Sept 17 and Oct 3 dates chosen? Why only 80 m? You have two U-Th profiles which could be used to directly compare to the sediment traps and determine NSS over 200 m. How was primary production determined and over what depths integrated?

6) Page 152. Fluxes at 60 m do not include the DCM at 50-60 and 90 m, so could fluxes have occurred below these depths? Please show sediment trap data so that comparisons of water column and sediment trap derived C and Th fluxes can be evaluated.

7)Table 1. I was very confused by this table and could not determine any of the SS and NSS Th fluxes given. I could also not figure out how the POC fluxes were derived from Th. Also need to include error bars.

8)Figure 1- should center the DYFAMED site on the map. It is almost lost in the corner

9)Please show all the May data for comparison.

Summary: 1) Does the paper address relevant scientific questions within the scope of BG? Yes 2) Does the paper present novel concepts, ideas, tools, or data? No 3) Are substantial conclusions reached? No 4) Are the scientific methods and assumptions valid and clearly outlined? No 5) Are the results sufficient to support the interpretations and conclusions? No 6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No 7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes 8) Does the title clearly reflect the contents of the paper? Yes 9) Does the abstract provide a concise and complete summary? Yes 10) Is the overall presentation well structured and clear? No 11) Is the language fluent and precise? Yes 12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? No 13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes 14) Are the number and quality of references appropriate? Yes 15) Is the amount and quality of supplementary material appropriate? N/A

Interactive comment on Biogeosciences Discuss., 6, 143, 2009.