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Interactive comment on "Silica cycling in the ultra-oligotrophic Eastern Mediterranean Sea" *by* M. D. Krom et al.

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Most of the studies in the last years on Mediterranean Biogeochemistry have been focused on Nitrogen and Phosphorus and the intruiging ratios in their stocks. After the classical work by Shink (GCA, 31, 987, 1967) Silicates have seldom been included in the analysis of nutrient budgets of the Mediterranean sea. A few years ago Ribera et al. (JGR, 108, 8106, 2003) highlighted that Silicon budgets were hard to balance both in the Eastern and Western basins.

Krom et al. address the issue for the Eastern Mediterranean (EMed), which they have repeatedly explored in the last two decades, and wisely add new terms to the previous scenario which may help in reconciling the budget or, at least to mitigate the big difference between the export and the previously estimated imports. Their estimate does

not balance the budget by ∼32000 Mmol a-1 which, they argue might also be due to error accumulations in the different fluxes. By the way, it is not necessary to assume that Emed is in steady state for Silicon and therefore their estimate may be much closer to the reality than the previous ones.

Considering that the phylosophy of BG which is of fostering the scientific discussion and considering that the authors are proposing a novel view of Silicon dynamics in the basin, thus making a real step forward on the issue, I support the publication of the paper but I want to raise a few points that the authors may consider in their contribution or in future contributions. Should they opt for the latter, I would ask them to present the paper more as an attempt to improve the budget estimate to stimulate further analysis than as a conclusive reassessment.

They add three terms in the input that are worth discussing separately.

Biogenic Silicon (BSi).

The contribution of BSi in the flux at the Straits of Sicily, that they assume in the budget, might be overestimated. Indeed Crombet et al. (BG, 8, 459, 2001) report a lower concentration at the Straits of Sicily (see their Figure 11 and consider a layer of 125 m). Their values, averaged over the whole water column, are of the same order of magnitude of the values reported by Price et al. (PO, 44, 191, 1999) for the Otranto Strait and the Aegean sea. The reason for such low value is likely the same invoked by the authors for the waters exiting the Adriatic sea. BSi dissolves quickly especially at the relatively high temperature of the Mediterranean surface waters. In addition, due to the complex dynamics of the Straits of Sicily it is important to compute inputs and outputs along a section and not as single average values. The Northern part of the Straits is a site of intense upwelling events and the whole strait is a site of intense mesoscale activity. Some of the BSi may be produced by Si contained in upwelled LIW and not originating from the WMed. This stresses the importance of having inputs and outputs measured along one section, possibly with repeated measurements.

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Ground waters

The contribution of 25% of ground water may be overestimated. Such an input would significantly alter the fresh water budget of the Mediterranean sea. Mariotti et al. (JC, 15, 1674, 2002) consider that ground water may contribute to the fresh water budget of the basin, though not to such an extent. However, it is certain that the contribution of ground waters has been ignored so far in the Mediterranean sea and requires a proper assessment.

Release from sediments

This is the most interesting addition to the analysis but requires a more in depth discussion. The authors go back to the classical works by Shink, Fanning, Gieskes et al. and cleverly rescue measurements of Silicate concentrations in pore waters carried out in 1975 at three different sites of the Eastern Mediterranean (EMed). From them they derive an estimate of Silicate release from the sea bottom. I think that this is an important suggestion for future work. Though, I have two perplexities on the value they derive. The thickness of the upper layer of the sediment, down to the first sapropel layer, which is present in large parts of the EMed, is of a few tenths of centimeters. Distribution of ions in EMed cores close to S1, as well to other sap-layers do not show a typical diffusive pattern through the sap-layer (e.g., Nijenhuis et al, EPSL, 169, 277, 1999; Slomp et al., GCA, 66, 1171, 2002) which seems to act as a cap. If we assume a Silicate concentration in upper sediment layer of 150 mmol m-3 and a total volume of $\sim$1.4 1013 m-3 the Silicon stock would be in the order of 210000 Mmol, which is approximately four time the annual release the authors assume. While liking very much the idea I am curious to know how thick the authors imagine the layer and, therefore, which is the time scale they are considering. Obviously I am assuming that the released silicates are not the one entering the basin form the other inputs and eventually deposited, because those have already been accounted for by other terms of the budget. I also think that to test that hypothesis they may use a simple box model and a careful analysis of the Silicate profiles in the whole area of the EMed that the

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authors have in their hand and that are also stored in Pangaea data base (Lavezzi et al., doi:10.1594/PANGAEA.771907, 2011). My other point is the following. The silicon profile with a subsurface maximum at around 1000-1200 m is hard to reconcile with the only significant flux coming from the bottom. The authors may argue that this was not the case before the EMT, which might be partially true for the Levantine but it is not true for the Ionian sea. The change produced by the EMT is in fact a good opportunity, because the drastic decrease of Silicate at the bottom should be slowly compensated by the release from the botton and therefore the deep concentration should slowly rise again. An impact should be already visible because 12 years have already passed since the EMT. A multilayer box model should help to verify if the expected profiles are consistent with the assumed fluxes from the bottom. This will also help in better tracking the Adriatic contribution that is still a crucial term of the budget because of the significant fresh water inputs.

I am aware that I am not offering alternative solutions and that what the authors are proposing is very reasonable but I would like to that they, either in this contribution or in another one, describe in more detail the processes they envision.

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