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Discussions

Interactive comment on "Magnetic quantification of Fe and S bound as magnetosomal greigite in laminated sapropels in deeper basins of the Baltic Sea" *by* M. Reinholdsson and I. Snowball

Anonymous Referee #1

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General comments: The manuscript of Reinholdsson & Snowball deals with the application of geophysical snd some elemental methods, first to quantify the greigite contribution to sediments provided by magnetotactic bacteria and second, to relate this to quantitative fluxes of greigite-related iron and sulfur fluxes towards selected Baltic Sea sediments. In principle this is an interesting story, since the authors have previously shown that microbial greigite may contribute to Baltic Sea sediments and may probably be quantified by geophysical analyses of cores. The authors attempt to compate the results of their approach with estimated fluxes from other Baltic Sea cores for reactive iron ('FeR') and total iron ('FeT') and limnic settings. In the discussion part, the results are compared to selected estimates from other sites. As a whole, the approach

is interesting, however, as outlined below, the combination of selected data sets from very different sites and environmental conditions weakens the presentation. In particular, an in-depth discussion on possible diagenetic impact on the preserved greigite is completely missing.

Specific comments: Greigite should transform in the presence of dissolved pore-water sulfide into pyrite as shown for the brackish Black Sea sediments (e.g., Neretin et al., 2004; GCA), that has a similar change in paleoenvironmental conditions since the last glacial than the Baltic Sea basins. Since, according to the authors information, several brackish sites are investigated, the impact of microbial sulfate reduction, sulfide production has to be considered in the discussion of a quantitative use of residual greigite data for an estimate of fluxes into the sediment. This likely superimposition by diagenesis is not considered a all. I don't wish to comment on the use of LOI as a semi-quantitative proxy for TOC contents. It is difficult to understand, why a parameter which is not a quantitative measure for organic carbon (TOC) content is used instead of a real TOC analysis. Anyhow, LOI may be used for stratigraphy correlation purposes, as mostly done in the present communication, but should not for flux estimates for organic matter. For one near-coastal site, the authors report the estimated flux of reactive iron to the sediments. This is done by a chemical extraction that gives actually a measure for still reactive iron in the sediment. However, this approach neglects the fraction of reactive iron that is already converted into sedimentary pyrite, but was originally a FeR fraction. Since the authors do not provide us with any informations about the (claimed as brackish) depositional conditions for this site, we have to assume, that some pyrite may have been formed and buried. Therefore, the flux reported for this site can not be taken as a realistic reference, at least based on the informations provided by the authors. Finally, as a comment on the discussion part of the manuscript, is looks as if the used number of references was selected not to cover all known aspects about the controlling factors for MTB abundances and activities in freshwater-brackish sediments. In this context also the question remains, if magnetosome greigite is a product of pelagic or benthic microbial activity.

Interactive comment on Biogeosciences Discuss., 11, 729, 2014.