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Interactive comment on "Effects of fluctuating hypoxia on benthic oxygen consumption in the Black Sea (Crimean Shelf)" *by* A. Lichtschlag et al.

Anonymous Referee #1

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This manuscript presents the findings of a study on the geochemistry and benthic infauna in sediments across a gradient of oxic to anoxic conditions in the Black Sea, which is topical given current interest in the effects of hypoxia on biogeochemical processes. The data set is well presented and the paper is generally well written. The key finding, which surprises me somewhat is that most of the oxygen consumption within these sediments is driven by the (inferred) direct oxidation of organic matter (including faunal respiration) as opposed to the oxidation of reduced solutes. One of the key conclusions is that organic matter is more efficiently mineralised in the oxic sediments which is generally consistent with current understanding, however, I am not convinced that this is to the extent inferred here. A change of 100% to 10% of organic matter mineralization seems extreme and should be backed up with some other measurements - %OC and sedimentation rates for example. The way things stand; these values are

based on the assumption of constant organic matter deposition at all sites – how valid is this? How do you rule out gradients of water column productivity as you move offshore?

The study would have benefitted greatly from DIC flux measurements (as well as profiles). If these were undertaken this would have enabled respiration quotients to be determined which would have greatly assisted in the interpretation. If, as the manuscript concludes, that the mineralization of organic matter was the dominant carbon degradation pathway, then this should be close to 1. I think that the RQ could be >1, particularly under hypoxic conditions, which implies the burial of reduced material, most likely sulfides. Many studies which have measured the RQ in coastal sediments (see for example Berelson, Hammond and Devol to name a few) and it would be nice to have a bit more literature context on what others have measured and their interpretations. It would be particularly nice if the authors could find such data for sites with high rates of Fe reduction as I suspect is occurring here (see below).

Following on from above, is burial of reduced solutes a significant fraction of ODU?, Can you do a mass balance of the oxygen equivalents buried in the reduced sulfur species measured here in combination with the sedimentation rates and add this to table 3?

I was also surprised that there is no data on the sediment carbon content, this information would help confirm the postulated differences in carbon mineralization, hence preservation across the study sites.

The high concentrations of Fe2+ combined with the relatively high concentrations of solid phase iron suggest that there is very active iron reduction taking place at St462 and to a lesser extent St487. I was surprised that iron reduction was not mentioned or discussed. Could it be that a lot of oxidation of reduced iron takes place on a time and spatial scale missed by the microsensors? For example there are some nice examples of profiles here showing O2 penetration to $\sim$1 cm (clearly mediated by

irrigation), yet the profile interpretations are all under taken on the mm/diffusive scale. Can you constrain this a little better? For example can you use the relationship between poorly crystalline Fe and %Fe reduction shown in (Jensen et al. 2003) to estimate the likely contribution of Fe reduction?

There is no mention of denitrification. This is probably not significant, but should be justified based on measured NO3 concentrations.

Ref cited. Jensen, M. M., B. Thamdrup, S. Rysgaard, M. Holmer, and H. Fossing. 2003. Rates and regulation of microbial iron reduction in sediments of the Baltic-North Sea transition. Biogeochemistry 65: 295-317.

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