

## ***Interactive comment on “NirS-containing denitrifier communities in the water column and sediment of the Baltic Sea” by S. Falk et al.***

### **Anonymous Referee #1**

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This is a descriptive study of denitrifier diversity in the water column and coastal sediments of the Baltic Sea using nirS as molecular marker. Similar works have been already been performed by the authors and the present study doesn't bring significant new insight into the ecology of denitrifiers in marine ecosystem. Some interesting data are presented here such as the shift in denitrifer community composition in the water column according to the biogeochemical gradients. On the other hand, the comparison of the denitrifier community composition between sediment and water column is rather trivial.

1. I do not really see the interest of comparing the denitrifier community composition between contrasted environments such as sediment and water column separated from 500 km (from a microbial scale, it's rather large geographic distance).

2. Since no replicates were performed in this study, investigating the diversity of nirS in 8 and 5 samples from the water column and sediment, respectively, limits the statistical analysis. Thus, almost no statistical analysis was performed in this study therefore it is not possible to know if modification of the diversity indices or abundance in T-Rf's are significant or not. Without replicates, analysis of a larger number of samples along the gradient is required.

3. The authors assumed that “ distinct marine nirS type denitrifier communities occupy different ecological niches which are defined by the habitat, water column or sediment, shaped by the prevalent environmental conditions, and can be isolated by large geographic distances”. However, the present study is based on the comparison between only two different niches. Since denitrifier communities are also likely to vary during the year (and without taking into account the methodological bias between labs), I am not convinced that comparison of the author's result with previous data could really support this assumption.

4. In the conclusion, the authors stated that “However, all the DNA-based studies focus on elucidating the genetic potential for denitrification but they do not reveal whether these communities are actively denitrifying or not” and I fully agree with that. However, if the authors were aware of this problem, why no (potential) activity measurements were performed in this study ?

5. “We focused on nirS as a functional marker gene to detect denitrifiers since amplification of nirK was shown to fail occasionally for marine samples (Braker et al., 2000). Whether this reflects primer bias or is of ecological relevance remains unknown.” Did the authors verify that it was not possible to amplify nirK with these samples or was nirK excluded from the beginning?

6. What is the % of similarity of the other clusters with the nirS of denitrifying isolates (what is the lowest %)?

7. What is the difference between this study and the article of Hanning et al (the article

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of Hanning et al. is not accessible online yet) ? How close/different is the denitrifier community ?

8. p. 9 : “However, we conclude that in sediments with comparable environmental conditions similar nirS-type denitrifier communities can develop despite large geographic distances.” I am not convinced that the finding of less than 10 T-Rf that are similar is enough to support this conclusion or at least it should be restricted to dominant nirS denitrifiers since it is likely that the denitrifier community consists in more than different 10 populations.

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