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Interactive comment on "Long-term trends at the Time Series Station Boknis Eck (Baltic Sea), 1957–2013: does climate change counteract the decline in eutrophication?" *by* S. T. Lennartz et al.

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This paper presents and discusses the results of a long-term time series of the hydrographical, nutrient and suspended matter properties of a station located at the tip of a channel system in the Kiel Bight, a water mass off the major exchange route between the Baltic proper and the North Sea. Long-term data sets are always worthy of publication regardless of whatever trends the data show or even if they can be interpreted or not. Being rare, they need to be brought to the attention of the scientific community and the one presented here is probably one of the oldest, going back to 1957 with comparatively few interruptions. As such, this manuscript is worthy of publication but it

can be improved as indicated below.

Marked changes in the environmental chemistry of the Belt Sea have occurred during the study period: eutrophication due to nitrogen and phosphate fertilizers in the catchment area became noticeable in the 1970s reaching a peak in the 1980s. The rising trend stabilised in the 1990s and, at least in some regions, started declining into the 2000s. On top of this effect is the gradual warming trend recorded in European coastal seas. The warming trend was also recorded at this site but the effects of increasing and then decreasing eutrophication are less evident. Indeed, the only significant change in chemical and biological parameters is an ongoing trend in decreasing oxygen concentrations in the bottom water of the study site. There are several reasons that could explain the lack of trends in nutrient and plankton biomass concentrations and the decrease in deep oxygen.

During the 1970s I was involved in interdisciplinary investigations of the ecosystem close to the site of the Boknis Eck station dealt with here. My group carried out a detailed study of the annual cycles of physical, chemical and biological properties of the water column at time scales of twice a week during blooms and a maximum of a fortnight in the winter over 2 $\frac{1}{2}$ years (1972-74). I was deeply impressed by the rapidity with which the spring blooms peaked and collapsed any time between mid-March and mid-April depending on the weather. The magnitude of the bloom is an index for the degree of eutrophication but it is likely to be missed by a programme carrying out monthly measurements. Another measure of eutrophication could be the concentrations of macronutrients phosphate and nitrate left over after sinking out of the spring bloom, i.e. in the month of April. In our time period this was the period of minimum nutrient concentrations but they were noticeably higher during the 1980s. So it would be interesting to know the situation today. Yet another measure is represented by the winter nutrient concentrations in particular nitrate, as phosphate concentrations are maintained at around 1 μm L-1 by interaction with solid phases in the sediment. The nitrate concentrations presented here are remarkably low, to my mind, so the system

can hardly be regarded as eutrophied. In the German Bight concentrations used to be an order of magnitude higher.

Because of the tilt of the land surface, drainage of the agricultural area surrounding Kiel Bight is into the North Sea hence nutrient loads in surface and deeper layers are exported from the Baltic proper and Belt Sea respectively. Hence, it would be interesting to see what the eutrophication trends look like in these regions, surely there are nutrient data from the Bornholm Basin and the Kattegatt available? Given the low winter nitrate values at Boknis Eck a comparison with the above regions would be quite useful.

On another note, I remember coming across papers on submarine freshwater discharge in the Eckernförde Bucht that are likely to have been affected by regional eutrophication. Would they be of significance to the nutrient budget?

I would be cautious with the chlorophyll measurements: are they still being measured spectroscopically and not by fluorometer?

Stratification in Kiel Bight is driven to a much greater extent by salinity than by temperature. So I missed a time series of bottom water salinities, what are the trends like and how do they compare with oxygen values? These should be presented before the wide-reaching conclusion suggested by the title is reached. With regard to the trend in decreasing oxygen, it should be remembered that the volume of water below 20 m depth in Kiel Bight is a small percentage of the total. Because of this, comparing primary production over the area of Kiel Bight with oxygen conditions in the narrow channel is misleading. It would be more useful to compare O2 values at 20 m depth as these are more representative of the bottom water in Kiel Bight as a whole.

I don't like the end phrase of the title. What is being counteracted? As the authors correctly point out, oxygen concentrations in the deep water channel are a function of its residence time below the euphotic depth but also the oxygen demand of the seabed at that depth. So what would be the consequences of the decreasing O2 not caused by

increasing production but by temperature-related increase in microbial activity? This implies that the Corg fuelling the bacteria would not have been remineralised at lower temperatures and would thus have been sequestered in the sediments. The implication is that the higher oxygen demand due to temperature increase is breaking down "fossil" organic carbon that would otherwise be buried in the channel system. The situation would be analogous to that of high-latitude soils exposed to global warming. Since this would be an important conclusion, it is necessary to make a stronger case for it.

I suggest that seasonal stages be compared with one another rather than the entire year. The major seasonal stages, by month, have been presented and explained in this paper: Smetacek, V., B.v. Bodungen, B. Knoppers, R. Peinert, F. Pollehne, P. Stegmann and B. Zeitzschel (1984) Seasonal stages characterizing the annual cycle of an inshore pelagic system. Rapp. P.v. Reun. Cons. int. Explor. Mer 183, 126-135.

For example comparing the averaged values for the summer months, ie April-July for the available data sets would represent nutrients left over after silicate exhaustion of the spring diatom bloom. There are papers from the 1980s that mention this effect.

The spring chlorophyll values are strongly influenced by the temporal spacing of the sampling day relative to the bloom peak hence should not be examined for trends. The May peak in bottom ammonium values is interesting and possibly more representative a proxy of the spring bloom: how constant is it? It would indicate the degree of regularity of sinking out of the spring diatom bloom.

I do not think that primary production measurements in the surface layer are of any use in judging the oxygen condition of the entire system, particularly the deep water, as the classic methods also record recycled production in the surface layer which leaves no imprint on the deep water. Mass balances: the control of biomass build up by the limiting nutrient and sinking out of organic matter are much more pertinent to judging the oxygen demands of the sediment and deep water.

It is hoped that future comparisons also compare the results with those from time-

series stations in other areas. Of interest for example, is the persistence of the spring bloom in Kiel Bight and its disappearance in some other comparable sites such as Narragansett Bay and San Francisco Bay (for references see Smetacek, V. and Cloern. J.E. (2008) On phytoplankton trends. Science 319, 1346-1348). I would greatly encourage repeating the detailed studies carried out by teams of students for their respective theses during the 1970s and early 1980s. Meaningful comparisons to judge changes in ecosystem properties could then be made. My experience from that time also was that the members of coordinated teams not only developed a broader vision of functioning marine ecosystems but they also acquired skills in sectors they would have missed if working alone.

I do not expect the authors to follow all my suggestions; this could be done in follow-up papers.

I was appalled by the many spelling mistakes that a spell-check would have picked up. The paper needs to be edited by a native speaker.

Interactive comment on Biogeosciences Discuss., 11, 7615, 2014.