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Discussions

Interactive comment on "Hypoxia and cyanobacterial blooms are not natural features of the Baltic Sea" *by* L. Zillén and D. J. Conley

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Review for L. Zillén and D. Conley Hypoxia and Cyanobacteria – not natural features of the Baltic Sea

The title of the paper is also the main message the authors like to spread. It is an interesting statement and contradicts the findings of Bianci et al from 2000 who stated the opposite that cyanobacteria in the Baltic Sea are a natural feature since app. 7000 B. P. when the Baltic turned into a brackish sea. The authors compiled information from various fields of sciences like history and demography to show the existence of a major human impact for the last two millennia. I like this approach very much. Periods of anoxia and lamination of sediments are suggested to be a result of human activity and cultural development. However the reasoning is not always logic, the distinction

between nutrient requirements generally and the phosphorous dynamics for nitrogen fixing cyanobacteria is not always correctly separated and organic and inorganic nutrients not always clearly separated form organic. Therefore, the conclusions are too far reaching from my perspective which is described in more detail below.

So far all major reviews of the human impact on the global nitrogen cycle agree that eutrophication of the earth system started with the production of industrial fertilizers i.e. with the Haber-Bosch process around 1900 but not before (e.g. (Gruber and Galloway, 2008; Smil, 1999). Only with the industrial nitrogen fixation humans were able to produce nitrogen in much larger quantities than the environment was able to remove. There is no good reason to assume that this is not true for the Baltic Sea and its catchments.

The fact that laminations occurred as early as 8000 years B.P., indicates that natural processes alone can be responsible for hypoxia, in this case it may have been a restricted water exchange and high salinities – as the authors describe. The point is made that after this period hypoxia is not related to salinity changes and therefore other (human?) mechanisms may have triggered periods of oxygen depletion in the bottom waters and laminations in sediments (Page 1788). This view neglects other natural causes like temperature. Temperature is mentioned in the discussion paper (page 1797) but is not credited enough. Although the absolute temperature of a water body may not be responsible for the growth of cyanobacteria (Wasmund (1997) describes this in detail specifically for the Baltic Sea), it causes thermal stratification which is a prerequisite for enough light in the euphotic zone and less turbidity and mixing of the cells. The latter is especially important for the species Aphanizomenon.

But even if the human impact may have influenced the hypoxic period between 2300 and 700 BP (Fig.2) then why did it start already in 230BP, when the population increase occurred later? And the arguments for the changes in population density as presented under 3.1 are rather weak. To translate population growth data from Sweden to the whole catchment may not be correct. From the figure 3 it is difficult to tell what increase is meant and related to a specific in crease in the curve in Figure 2B. May be change to a linear scale.

It is difficult to follow the periods of slightly varying length throughout the text. First introduced on page 1787, but then differing in the later pages. They may be harmonized in length and names give (Medevial expansion etc).

My most concern is related to the arguments and guesses concerning the past nutrient loads to the Baltic Sea. 1. Nutrients from farms (if they were numerous) would have to deliver their nutrients to the Baltic Sea and to the Baltic Proper (coastal hypoxia is not meant in the text). Many villages and farms may have existed along rivers or at lakes which have a connection to the sea. But what about the nitrogen removal along the rivers, in soils, and groundwater in which Seitzinger (2006) estimated considerable removed (today!). But the ancient rivers must have been even more efficient in nitrogen removal because they had a natural river bed with natural vegetation. Most nutrients will then be sequestered / removed before they can enter the large rivers and the Baltic Sea. 2. The first paragraph under 3.2 makes important statements on nutrient release from soils after tilling which is the basis for the major conclusions of the paper. However, they are not backed with any citation and Ulén et al. seemed to study P only. I certainly agree that the agricultural practices impact the nutrient release (as does the deforestation). However, this is a speculative argument with no quantitative information. 3. To strengthen the point of high nutrient loading from cutting trees in the catchment and increased N and P loads to the Baltic Sea, a budget should considers the input, and the removal, and the dilution into the Baltic Proper. My own back-of – the-envelope calculation does not agree with a substantial increase. On page 1793, 9.8 million tonnes from AD 1000 to AD 1300 are set in relation to todays annual input of 737 kilo tonnes. However, the first number is an input generated over a period of 300 years, if I understand the paragraph correctly. Assuming a linear input, this would result in an annual input of 32 kilo tonnes which is 4.4% of todays input. This is not as much as the authors suggest. Moreover, this input would enter the Baltic Sea at

a time when the lamination already stopped. 4. The above mentioned calculations are based on organic nitrogen which is not equivalent to nitrate. Most of it – after our current understanding (Stepanauskas et al., 2002; Stepanauskas et al., 1999) – is not bioavailable. Moreover, nitrogen inputs are irrelevant for nitrogen fixing cyanobacteria. 5. P 1793 lines 26-30: First of all the N-release it two orders of magnitude less not one. The sentence is not related to the text above, but tries to make a link to P-inputs without any logic relationship. Here and later (page 1797, line 1) C is summarized with N and P as nutrients, but C is not at all a nutrient. 6. There are good arguments that nitrogen removal is especially strong along the coastlines of the Baltic Sea(Voss et al., 2005) as it is for continental shelves in general (Middelburg et al., 1996; Seitzinger et al., 2006). This process would remove a major share of the incoming nitrate from land as it does today. 7. The inorganic nutrients nitrate and phosphate are not clearly separated in the text and not clearly separately discussed from the organic nutrients. But as I said above organic and inorganic compounds behave quite differently in terms of nutrition and bioavailability in marine waters. More important is that cyanobacteria of the Baltic Sea are assumed to profit from the P-surplus today (phosphate left over after the spring bloom). 8. The text does not clearly separate between P, which is important in the context of cyanobacteria blooms, and N, which is only important of general primary productivity. High primary production rates generate biomass which presumably consumes oxygen from the water. So there is a clear interaction between productivity in general and the generation of anoxia which in turn is responsible for enhanced release of P from sediments.

Overall I do not agree with the statement made on page 1798 L 26-27 because the lines of evidence are not solid enough to make such statements. However, I do agree with the fact that hypoxia increased over the past app. 100 years and also the biomass and occurrence of cyanobacteria blooms. There is enough evidence in the literature to back this statement. My concern is related to the causes of the early laminations for which I like to see much clearer evidence.

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