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Biogeosciences
Discussions

Interactive comment on "Impact of change in climate and policy from 1988 to 2007 on environmental and microbial variables at the time series station Boknis Eck, Baltic Sea" *by* H.-G. Hoppe et al.

N. Wasmund (Referee)

norbert.wasmund@io-warnemuende.de

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Review of the paper submitted by Hoppe et al.:

Overall quality of the discussion paper: The paper submitted by Hoppe et al. is a valuable contribution to understanding long-term changes in ecosystems and is in the scope of BIOGEOSCIENCES. The connection to climate changes receives high attention nowadays. The consideration of political impacts is a new and interesting concept that activates some quarrel (see below). The political causality may cause some de-

bate, but the measured trends in physical, chemical and biological parameters are real. These are valuable data, collected with appropriate methods and clearly presented in numerous figures, which are all of relevance.

Comments to the separate sections: Abstract: Line 2: Was the Boknis Eck time series really stopped in 2007? What a pity. Nutrient concentrations and bacterial variables decreased and temperature increased. But in line 20 it was written that "in the long run all variables correlated positively with temperature, except chlorophyll and salinity". How can nutrient concentrations and temperature correlate positively if their trends are opposite ? Obviously, seasonal and long-term effects are mixed here.

Introduction: p. 18657, line 26: The HELCOM monitoring programme (Baltic Monitoring Programme, BMP, now COMBINE) started in 1979. Of course, for some parameters and regions, data were recorded already much earlier and in some regions data series started later. But we consider 1979 as the beginning. I think in the references given (Nausch et al. 2011 a, b), the year 1980 was not mentioned as the start of that programme.

Study area, sampling and methods: p.18659, line 9: Is the water stratified only in summer ? Because of the large depth (28 m), I suppose a permanent haline stratification, also in winter. Therefore the question: Does the vertical mixing in fall and winter (why not in spring ?) reach the bottom or only the halocline, if there is any ?

I would expect in section "2.1 Study area, sampling" already some information on sampling frequency. Were all years and seasons equally covered? But I see that this information can be neglected here because in many of the figures the singles data points are shown and the number of data is given in Tables 1 and 2. p. 18660, line 21: How was daily primary production rate calculated from the three-hours incubation ?

Results The kind of presentation of the data e.g. in Figs. 2, 5, 13 is informative as both seasonal and long-term patterns are included. Minima and maxima are accentuated and especially discussed. For the regression analysis and correlation matrix the com-

bined seasonal and long-term effects may, however, be problematic. For example it is written on page 18668, line 4, that “all nutrients correlated positively with salinity and negatively with temperature (Table 3b)”. This may primarily result from the seasonal pattern: During winter (low temperature, high salinity) nutrients are of course highest. This cannot be applied to long-term considerations. Here I would expect higher nutrient concentrations if low-saline but eutrophied Baltic Sea water is outflowing and passing the station (?).

Discussion P. 18676, line 25 ff.: Wasmund et al. (2011a) were quoted not quite correctly. They did not show data from Kiel Bight but from Lübeck Bight. In the Mecklenburg Bight they found a strong negative trend in spring chlorophyll a data. However, if the time-series is adapted to the period from 1988 to 2007, the spring data of Mecklenburg Bight may show no trend, as stated by Hoppe et al. I would not try to interpret too much into the differences of the two series; probably it is the difference in sea areas. For example, the authors say in line 29, that “the improvement of water quality after 1989 would not yet have penetrated the Western Baltic Sea area.” On the other hand, the data of Wasmund et al. (2011a) could be interpreted as a strong improvement already before the political change. P. 18676: The differentiation into “eastern” (line 17) and “western” (line25) Mecklenburg Bight was not made; say simply “Mecklenburg Bight”. P. 18678, Line 10: Wrong author name: this is not Winkler, but Wikner.

My concluding remarks: It was a tremendous task to measure these many parameters, most of them (primary production, bacterial parameters) very laborious, with a high resolution both in time and in the depth profile.

The authors found decreasing nutrient trends and claim that they are caused by the break-down of the socialist systems. The nutrient decline is basic data, but the causal relationship to the political changes is simply postulated, is hard to prove and remains speculative. In principal such relationship seems evident and I don’t mind, despite the involvement of policy tends to be a bit lurid.

In the Discussion (p. 18668), the authors shortly explain which “political change” they mean and which factors were relevant for environmental trends (e.g. decreased use of fertilizers, improved sewage treatment) and what years they consider as the decisive years. As the time series shown starts with the years of political change (around 1989), it cannot be excluded that the negative trend existed already before 1988 and no trend break occurred around the year 1989. For example, Wasmund et al. (2011b) found no trend breaks in Kiel Bight. If the neighbouring Mecklenburg Bight is considered, also there is no trend break noticed in the chlorophyll (Wasmund et al., 2011b). The arguments become weaker if the biological factors react with delay of different length. If for instance bacterial parameters react only 10 years after the political change (p. 18679, line 17), it is hard to prove that a causal relationship exists. Instead, no stepwise trends but oscillations seem to occur (Figs. 10 and 16), that can hardly be explained with political changes as politics did not change in an oscillating manner.

In general, the paper is well-written and easy to understand despite the complex data. The data are clearly laid out and sufficiently discussed. The local literature was considered. The paper of Wasmund et al. (2011) could also be taken into account, particularly because the trends agree with the data of Hoppe et al. (this paper). Admittedly, Wasmund et al. split their data into seasons; therefore the numbers of data for statistical analyses decreased and significant trends were found rarely: In the central Kiel Bight (Station N3) decrease in phosphorus and in the eastern Kiel Bight (Station N1) decrease in salinity and increase in temperature.

Literature mentioned: Wasmund, N., Schöppe, C., Göbel, J., vonWeber, M. (2011a): Chlorophyll-a in den deutschen Ostseegewässern. Meeresumwelt Aktuell Nord- und Ostsee 2011/2. 1-8. http://www.blmp-online.de/PDF/Indikatorberichte/2011_02_d.pdf

Wasmund, N., Tuimala, J., Suikkanen, S., Vandepitte, L., Kraberg, A. (2011b): Long-term trends in phytoplankton composition in the western and central Baltic Sea. Journal of Marine Systems 87: 145–159. http://dx.doi.org/10.1016/j.jmarsys.2011.03.010

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