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Discussions

Interactive comment on "Spring Blooms in the Baltic Sea have weakened but lengthened from 2000 to 2014" *by* P. M. M. Groetsch et al.

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1. Lines 90-95 / Figure 1. Text mentions that "any threshold-based metric" would introduce artificial trends in bloom duration. This is a clear problem for "fixed threshold" metrics, but not for "variable thresholds" as Siegel et al. (2002), which is later introduced.

Authors' response: Please see our detailed response with question 2).

Furthermore, results using the fixed threshold const5 show a negative trend in peak concentrations, but no significant trend in bloom duration. This seems a somewhat inconsistent. Further discussion would help clarify why the expected artificial trends do

not occur.

Authors' response: Peak concentrations are derived the same for all metrics, thus the negative trend is not metric dependent. Indeed, no artificial negative trend in bloom duration was observed for any threshold-based (fixed, or derived from climatology; see question 2)) metrics. We argue that this observation can be expected if blooms also became longer over the study period. An independent confirmation of this hypothesis is the strong positive trend in bloom duration in the Weibull-metric results. We agree that this could be stated more explicitly in section 4.4, e.g. adopting:

'Thresholds of const5 and median5 are fixed for the whole time-series. The observed negative trend in peak concentration introduces an artificial negative trend in bloom duration due to a shortening of the part of the curve seen above the bloom threshold, but this is solely by decreased amplitude of the curve (see Fig. 1). Contrary to this expected behavior, however, const5 and median5 revealed no significant trends in bloom duration. This indicates that the anticipated negative trend was countered by a positive trend. The Weibull-metric is based on concentration distribution-ratios that are calculated for each bloom individually. Therefore Weibull-metric results for bloom duration are not sensitive to long-term trends in peak concentration. Weibull-distribution metrics confirmed a highly significant, positive trend in bloom duration. These two sets of results mutually support the conclusion that spring blooms in the Baltic Sea have become longer, while chla peak and average concentration levels declined.'

2. It is not clear to me whether median5 (Siegel et al. 2002) is calculated for each individual annual median or for all years together. The latter would indeed produce a fixed threshold for each region (see previous comment). That detail is unclear in Siegel et al. 2002 as well, but see Henson et al. 2009 (Decadal variability in North Atlantic phytoplankton blooms – J. Geophys. Res.) and Brody et al. 2013 (A comparison of methods to determine phytoplankton bloom initiation – J. Geophys. Res.).

Authors' response: Indeed we assumed that Siegel et al. 2002 referred to the climatological median, rather than the annual median. Brody et al. 2013, however, state that both thresholds can be applied:

'The threshold bloom initiation method was introduced for marine phenology studies in Siegel et al. [2002]. This method finds the yearly or climatological median of a chlorophyll time series, then identifies the bloom start date as the first point at which chlorophyll levels rise a certain percentage above the median.'

To make the distinction clear we propose to change the respective paragraph in the introduction: 'Figure 1 illustrates how a gradual decline (negative trend) in bloom peak concentration causes any metric based on fixed thresholds (e.g. derived from climatology or expert-judgement) to introduce an artificial negative trend in bloom duration. In contrast, metrics based on growth-rate, distribution, or annually derived thresholds yield a single bloom duration for the given example, because bloom intensity does not influence these metrics.'

3. Lines 195-200: Day-of-year 31 is January 1?

Authors' response: This is in error and should read 'day-of-year between 31 (31 January) and 160 (9 June).

4. Why was the time frame between day 31 -160 selected? Is it possible that nutrient peak concentration occur prior to the minimum date considered? A shift to earlier peak nutrient concentrations is mentioned, but results of the nutrient metrics are not presented. I suggest extending Table 3 and/or including plots to support this.

Authors' response: The ship-of-opportunity (Alg@line) measurements typically commenced in late January, which is why we chose 31 January as the start of our analysis. The end date was chosen such that it covers all spring bloom events in all basins but not summer bloom. We propose to add this information after the first paragraph of section 2.4. The nutrient peak concentration is closely related to the day of bloom initiation, which is typically at least a month later (see table 3). Of more concern is that for

several years, Alg@line data collection commenced after bloom initiation. These data were consequently omitted from statistical analysis (replaced with multi-year median), including the nutrient statistics. Further detail on this issue is also given in response to question 7. Table 3 shows multi-year averages of calculated parameters, so we can not expand to trends in nutrient timing or intensity from these. Since these results are nevertheless available, we propose to add all nutrient metric results to the appendix to aid future research.

5. Lines 230-235: In 30 out 225 data combinations there were no ferrybox observations to properly identify bloom initiation. In these cases, bloom initiation date was replaced by the median value. It is not clear if this treatment was used only for the principal component analysis or the regressions as well. Cases identified by each timing method only account for 29 (const5:9, median5: 15, weibull: 5). I find it also unclear how these methods identified that the bloom had already started. A few words to clarify would be useful.

Authors' response: Median-filling of missing dates was applied prior to both PCA and regression analysis. We assumed bloom initiated prior to Alg@line data collection if the first data point already satisfied the bloom criterion for a given metric. This should be more clearly stated already in section 2.4 instead of in section 2.5. The number of missed bloom initiation events is incorrectly stated as 15 for the median5 metric and should be changed to 16.

6. The time series analyzed is relatively short to claim long-term trends, especially when considering the large interannual variability observed in all of the metrics. A study between 1979-2013 where decadal-oscillations were found is mentioned in the text. I would recommend extending the discussion a bit to include how that analysis compares with this one during the same time frame.

Authors' response: The authors of the mentioned study (Kahru2014) describe surface accumulations of cyanobacterial summer bloom. Links between spring bloom and cyanobacterial summer bloom are certainly worthwhile exploring. However, the complex interactions between light- and nutrient-limited spring bloom, and largely wind-modulated cyanobacterial surface bloom accumulations seem out of scope for the present paper (and may quite possibly be too complex). In section 4.4. we acknowledge the finding of Kahru2014 that summer bloom initiation moved to earlier dates, and thus that the period between dinoflagelate- and diatom-dominated spring bloom and cyanobacterial summer bloom decreased. The following sentence should be added to section 4.4 to clarify that we can neither prove nor disprove a decadal oscillation signal based on our time series: 'However, due to the shorter period covered here as compared to the time series presented by Kahru2014, it cannot be ruled out that the derived trends are affected by decadal oscillation.'

7. The final discussion and conclusions attribute the declining trend in bloom peak concentration to declining nutrient concentrations; however, no decline in winter nutrient concentrations (as estimated here) is reported. The conclusion is based on literature considerations and the "lack(ing) of other explanations". I think this pattern is quite interesting and an alternative explanation may be supported by the results here presented. The authors report a shift in peak nutrient concentration to earlier dates and a strong correlation between winter nutrient concentration and bloom peak magnitude. Earlier increases in nutrient concentrations mean that nutrient limitation is alleviated earlier during the year, when light limitation might still be strong. As the year progresses and light limitation is alleviated, a fraction of the nutrients has been already consumed. The nutrient concentration "available for blooming" would then not be equal to the winter maximum, but lower than it. That would produce a decrease in the bloom peak magnitude, an apparent extend in bloom duration, but no change in total chlorophyll during the bloom (also reported). This is just a quick idea and might be better captured by rate-of-change metrics of bloom phenology, which are mentioned in the introduction, but not used in the analysis. As I mentioned before, I think it is important to include the nutrient concentrations results in the manuscript to better support its conclusions. I would also suggest including the actual time series (environmental

factors and fluorescence) as part of supplementary material.

Authors' response: Unfortunately the temporal resolution of the nutrient concentration data is not sufficient to quantify the timing of nutrient uptake onset and light limitation alleviation – especially in winter when only few cruises are sampled for nutrients. Nevertheless, judging from the few transects sampled for nutrients in December and early January, nutrient limitation is alleviated well before light availability increases, and this has been our understanding of nutrient dynamics in the high latitude, semi-enclosed Baltic Sea. We looked into several metrics for nutrient uptake rates but could not link inter-annual nutrient variability to bloom phenology parameters. This may be due to the relatively sparse collection of bottle samples for laboratory analysis (on average every 6th transect). While at present this result does not prompt further discussion in the manuscript, future studies may benefit from additional data so as suggested, we propose to add nutrient metric results to the appendix. In addition we would add the following to the manuscript: 'Several times ship service had not commenced early enough in the year to record bloom onset, which implies that trends in bloom start and nutrient peak timing could not be derived with the same accuracy and precision as the other phenological parameters. Nutrient metrics are provided in the appendix to aid future work, if additional data or longer time-series become available.'

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