Biogeosciences Discuss.,
doi:10.5194/bg-2015-636-RC2, 2016


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

**Anonymous Referee #2**

Received and published: 15 February 2016

This manuscript presents an analysis of 15-year time series of chlorophyll fluorescence at six spatially averaged regions of the Baltic Sea. Fluorescence observations come from ships of opportunity and are used to evaluate trends on phytoplankton phenology. The detailed description of the systematic quality control methods for this type of observational datasets is an important contribution that ensures replicability of the analysis. The authors discuss trends between 2000 and 2014 using different phenological metrics. The manuscript is well written and interesting patterns in the region are brought to attention. I would recommend the following revisions to improve the clarity of methods and discussion:

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 intro-



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duced. 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.

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.).

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

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.

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.

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.

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.

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Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2015-636, 2016.