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Interactive comment on "Satellite detection of multi-decadal time series of cyanobacteria accumulations in the Baltic Sea" *by* M. Kahru and R. Elmgren

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Journal: BG Title: Satellite detection of multi-decadal time series of cyanobacteria accumulations in the Baltic Sea Author(s): M. Kahru and R. Elmgren MS No.: bg-2014-26 MS Type: Research Article

Overall quality of the discussion paper The manuscript 'Satellite detection of multi-decadal time series of cyanobacteria accumulations in the Baltic Sea' by M. Kahru and R. Elmgren addresses an important issue in ocean colour remote sensing: to merge the results of different satellite missions into a time series that is long enough to evaluate long-term environmental changes, such as the effects of climate change. However,

I do agree with the anonymous reviewer #1 that some technical details need to be clarified before one can really be sure that the observed fluctuations are purely due to changes in cyanobacteria abundance rather than methodological problems. I confirm that the methods are not sufficiently clearly described, and need to be clarified. I have started working on this a week ago, and also read the answer of the authors to review no.1; but I still have some points to address. For the new plots the authors included in their response for showing the difference between MODIS Aqua and MODIS terra, I suggest that you quote the correlation coefficient instead of r2, the coefficient of determination, because we car not talking about a regression here, but about a correlation. How well does the data correlate? Also, in order to describe systematic errors, you should include the MNB, and for showing noise or random differences you should include the RMS difference. This is standard in evaluating the performance of different methods. In general, MODIS Terra is known to be unstable and prone to calibration errors, so it is important to assess its response. In general, I feel there is a quite loose use of the terminology of reflectance in the paper. It is not clear to me what physical units you use. Sometimes you call it albedo, 'brightness', sometimes water-leaving radiance, and sometimes you use the term reflectance. Please define clearly what you mean in whenever you write about any of these physical quantities. Define! I would like to go back to using a broad AVHRR channel to derive information on the reflectance of cyanobacteria. You stated yourself that you did not do any atmospheric correction to the AVHRR data, and that you used the information from channel 1 as an indicator of 'brightness' or albedo. Clouds etc. were masked out using the information from other channels. I would like to point out that some of the information that is included in your so-called 'albedo' also includes information about atmospheric aerosols, as you are using TOA radiance (is it radiance you are using?). So, some of the variability you get in the AVHRR image may be due to the variability in atmospheric aerosols or SPM in the water. This may also affect the final conclusions you draw, i.e. an earlier on-set of the bloom. Can you be sure that this is not caused by fluctuations of atmospheric aerosols? I also feel that you have not sufficiently addressed the different sensitivity of

the different sensors. Sensitivity not only refers to signal-to-noise ratio, but also to the actual radiometric sensitivity and the dynamic range of the instrument, i.e. how well it is adapted to sense dark or highly reflective targets. MERIS, e.g. has a very high dynamic range and is adapted both to highly reflective land and dark ocean targets; SeaWiFS e.g. needs a few pixels to adapt from highly reflective (land or clouds or cyanobacteria blooms) to dark water pixels; this is called bight-pixel recovery effect. The AVHRR has very broad channels which may increase its sensitivity, but at the same time it makes it impossible to detect the difference of different optical constituents, e.g. SPM versus cyanobacteria (which are both highly reflective). Turbidity is not the same as cyanobacteria blooms! Turbidity is a physical measure (scatter at a certain angle) which may also be caused by particles. In the southern and eastern Baltic there is a high load of suspended particulate matter and the sediment plumes may reach 100-120 km off-coast, whereas research in the Northern Baltic Proper showed that the extent of sediment influence is in the range of 10-15 km (Kratzer and Tett, 2008). That paper shows that the effect of inorganics tends towards zero at 10-15 km off coast. The cyanobacteria are often driven into bays and coastal areas by wind, so one should really aim to establish methods that are also applicable in the coastal regions where most of the production happens, and where it mostly affects tourism and maybe even the well-being of animals. Maybe one should look into different pattern recognition methods that differ between the typical patterns of cyanobacteria blooms and effects from coastal run-off (inorganics), and if not included in the paper, this should at least be discussed. It may well be that some of the features that you see on the highly sensitive ocean-colour images really derive from suspended matter (which in this case also indicate the typical eddies and other meso-scale features), and I wonder if the shift of the starting point of the blooms that you detect are maybe partially related to the increased sensitivity of ocean colour sensors and also the improved dynamic range of MERIS. How sure can you be that these changes in instrument specification do not affect your results? Other comments to improve the quality of writing I also find the paper rather difficult to read. The methods part seems rather technical, but I feel it does not sufficiently describe

the methods applied to each sensor -as already commented by reviewer 1#, and as I already mentioned you need to define the physical units you use. Also, to compare the results from satellite-derived information to ship transects with a water intake at 5 m depth is rather misleading, as the cyanobacteria tend to accumulate in the top meter during high isolation and strong stratification. This needs to be discussed more in the article. You clearly look at surface accumulations from satellite, which may be correlated to the measurements at 5 m depth, but they also may be decoupled, depending on the degree of temperature stratification in the near surface versus a surface mixed layer (which usually means that the surface accumulations break up). Incidentally- there is another weakness in the methodology. One should really use the same spatial resolution for the comparison- i.e. use the 1 km resolution for all sensors. This is because with the high horizontal heterogeneity of the cyanobacteria blooms you get different accuracies for the different spatial resolution. The accuracy for MERIS e.g. may improve by 100% when going from 1 km resolution down to 300 m resolution. If you go from 1km to 4 km resolution you therefore may have a large decrease in accuracy. In order to make the methodology more coherent 1 km resolution should be used for the whole data set! I also see a great disadvantage of using 5x5 pixel averages to compare to the ship measurements. Usually, in such heterogeneous waters, one extracts 3x3 pixels (5x5) is really only appropriate in open ocean where you can assume that the water does does not significantly change over a 5 km distance. But in this scenario it really seems not appropriate! Are you really comparing the same water body? On top of this you have the difference in vertical structure, so what are you actually comparing? I guess you must address these points both in the discussion and the conclusions you draw. General comments: In general I would also try to avoid writing in the first person in a scientific article. It is ok on the odd occasion, but I find the frequent use of ‘we’ very disturbing, and I would like you to consider to change to a more neutral and factual way of describing your methodology and results. For example: ‘While we have a better data coverage’ can be easily rephrased to ‘whilst there is better data coverage’ – it is not necessary to include the word ‘we’ here at all! Please

check and modify the whole document for this! Some odd errors I found: Page 3322 line 11: use biologically available rather than plant-available or bio-available Page 3322 line 25: Aphanizomenon sp. often dominates deeper layers in the water column (it is adapted to lower light conditions) Page 3322 line 2: what do you mean with 'brightness'? Page 3322 line 26: MERIS operated from 2002-2012; a time series only based on MERIS would be too short Page 3323 end of introduction: include here the aims of your study, try to avoid 'we' Page 3323 line 24 use 'highly reflective' instead of bright blooms; brightness is not what we detect, it is the change in reflectance; brightness is not really a physical measure or quantity. What are you quantities what are your units? Page 3323 in the last paragraph you talk about the poor sensitivity and the low spectral resolution of AVHRR; I feel this belongs into the discussion, as this may affect your results, but you do not really have a clear way to correct for these effects. Page 3324: several times 'we' – is not necessary-please use passive form instead Page 3325 line 18: I thought that SeaWiFS does not have a 4 km 'mode' – it always measures in 1 km resolution- the data is binned to 4 km after reception (level 3 product) Page 3327: this full page should be rewritten so it is easier to follow. Some of the sentences from below need to be moved up for logical consistency. How do you define your thresholds? You do not correct for the band shifts – what effect may this have?What are your underlying assumptions – that the effects are insignificant? Can you be sure? A flag is not 'set' but 'raised'; if you use the term 'set' it sounds like it is actively done by the operator- if it is raised- it sounds like it is done by an automatic process, e.g. by reaching a certain threshold. What are your thresholds? Use term 'particle backscatter' Line 27: instead of 'were manually filled' use term 'manually denoted' or designated? Filled sounds strange. Page 3328 The Gulf of Riga and Gdansk are known to have high SPM loads. Quote some of the relevant papers! Line 5. Which algorithms??? Line 10: how did you choose your threshold value? Why just this value? Any statistics to quote? Page 3328: a map is usually projected, not registered Page 3330 from line 21: does this not belong in the discussion? Page 3333 what do you mean with the relationship is less tight?

Interactive comment on Biogeosciences Discuss., 11, 3319, 2014.