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Discussions

Interactive comment on "Changing seasonality of the Baltic Sea" *by* M. Kahru et al.

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This paper, "Changing seasonality of the Baltic Sea", is an excellent complement to the authors' previous work on cyanobacteria (Biogeosciences, 2014). The evaluation of the various data sets shows changes in the system, some indicative of trends, some may indicate "state" changes, and some may suggest short term aberrations. I trust that the data presented in the figures will be available as supplements to this paper, as much analysis could be done with them. The paper is an important contribution to the science and an excellent demonstration of how to use remotely sensed environmental data to address an ecological problem.

One interesting example is that the irradiance (figure 3) shows the longest delays in the 1980s (Figure 3), when warming (SST) appeared most delayed (Figure 4). This is expected when we see it, but it indicates some interesting questions for climate change.

If climate change alters cloud cover, will the SST change more or vary more?

While satellite chlorophyll is problematic, which the authors clearly and carefully discuss, the data in Figure 9 shows a striking pattern of timing, particularly relative to the cyano blooms (Figure 10). Figure 9 uses field data and satellite data to show timing changes that are quite large and unlikely to be explained by simple uncertainty in the data. The authors present a combined data set to argue convincingly that the change in timing of the cyanos is likely due to changes in SST.

On the question of trend vs "state change". The SST phenology could argue either way. After 1996, the Baltic warmed to 12C always before day 170, where until that year 1/3 of the years did not see 12C until after day 170. Similarly 17C was reached before day 195 all but 2 years after 1996, but the warming was frequently later than that before 1996. This could be a trend, and I would not delete the trend line.

The same thing occurs at the end of season. In all but 2 years after 1996, the Baltic did not cool below 17 until after day 230, and more dramatically, warm water persisted until after day 245, which never occurred before 1996. Same thing with 12C, day 275 was not reached until 2000 (and not any of the previous years). Is the Baltic simply warming, or did the the system shift in 1996? The result is the same, but a provocative question.

Some Technical comments. Switching between satellites should be noted, while they don't appear to have an effect, except for the change from AVHRR to SeaWiFS for bloom area. (I looked first for discontinuities at years when satellites would change, and didn't see anything obvious.). The SST change could possibly change, but the Pathfinder set does cross-match AVHRR and MODIS rather meticulously. Still there is the possibility that satellite algorithms may cause some changes.

The one outstanding point is that uncertainties in timing are not discussed or estimated. This is important for the statistical confidence, particularly as this paper will become a reference on some of these methods. Clouds are the obvious factor in uncertainty.

Mostly overcast conditions can lead to delays in the day of interest (e.g., the date that the threshold was passed may have been overcast). Partly cloudy can also cause an error. For example, if clouds cover the southern part of the Baltic for a day or more, the averaged temperature will be biased to the northern Baltic, and may be colder, leading to a delay in the warming temperature threshold. The error may be slightly larger for AVHRR, but the authors should be able to provide some discussion on this.

One key reason to capture the uncertainty is to determine trend slopes that are real. The slope should be greater than that caused by the uncertainty. For example, on Figure 5, if the error is +/- 3 days, than the slope needs to be at least twice that to be real (so more than +/- 0.2 day/year over the 30 years). That is a perfectly reasonable way to present whether the trend is real. A rigorous assessment of uncertainty in date is not needed, just a brief description of what that might be, as many trends are much larger than those uncertainties.

Slope confidence, by the way, is captured by Figure 3b and Figure 5. These figures provide good metrics of confidence in the slope estimates. The consistency across the plots indicate that the slope errors are probably small. There are numerous ways to estimate slope confidence (jackknife, Bayseian, etc.), but that would be an exhausting point, and use of these plots is reasonable.

P-values, by the way, are not valid for these data sets and should be replaced with estimates of significant slope based on uncertainties or scientific significance (like the previous paragraphs). Null hypothesis P is the probability that a sample taken from a population that has no trend will return a trend at least as large as what was observed. This paper is working with populations, so P has no value. While P looks like it is telling us what we want to know, it is not. Nicholls 2001 (Bulletin Amer. Meteor. Society) describes the problem nicely. (In spite of what we've been led to believe, P-values are rarely appropriate for any data set, and a few journals have even decided to refuse to publish P-values, preferring other metrics).

The trend on Figure 8 (chlorophyll) is a bit dodgy, and probably better to leave off. There is a trend in the lowest observed winter "chlorophyll", but an overall trend will require a different statistic, like a seasonal Theil-Sen slope (to address the seasonality). That's not necessary.

The paper sets the stage for environmental modeling of the cyano blooms in the Baltic. I hope the authors tackle that.

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