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Interactive comment on "Regime changes in global sea surface salinity trend" *by* A. L. Aretxabaleta et al.

Anonymous Referee #1

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The manuscript presents an analysis of the observed surface salinity trends over the last decades from two observational data sets, one comprising in-situ measurements over the last 50 years, and one comprising satellite-derived surface salinity over the past 20 years. The analysis is focused on identifying break points and linear trends in-between the break points. The statistical method to estimate the breakpoints and values of the trend sis based on Expectation-Maximization algorithm, a well established method originally developed to fill data gaps in a panel data. The study identifies three 'regimes' through the past 50 years, each one with a different value of the linear surface salinity trend, with increasing magnitude of these trends, which the authors tentatively attribute to accelerated hydrological cycle.

My view is that the study is valuable and offers an interesting statistical view of the

long-term surface salinity trends. I have some general comments that the authors may want to consider in a revised version and a few more particular points.

1/ The overall goal of the statistical analysis is the identification of 'regimes' or break points in the SSS series. This statistical model of the time evolution of SSS is obviously an approximation to reality, as al models are. But a break-point model applied to observations arguably has a more artificial character than other statistical models. It is hard to accept that the mean value and the possible trend of a time series derived from unperturbed observations undergoes a step-change at a specific point in time . More likely, these variables may undergo a more gradual change, which may indeed be more difficult to model statistically. My impression of the analysis is that the authors somewhat uncritically accept the results of the break-point analysis and overlook that this can only be an approximate description. For instance, the estimations of the break-points in time are given without uncertainty estimates. However, when the estimation of the break-points does depend on the data set used, as the authors show when comparing the gridded data from the Hadley Centre and the satellite data, there are differences in the estimated break-points of a few years. There is, therefore, a sample uncertainty which is not considered in the study, or only implicitly.

2/ I found the introduction section a bit disorganized. This is to some extent a matter of writing style, but sometimes I was left wondering what was the connection between senescence in the same paragraph. This section largely reads as a collection of previous studies, followed by a statement of the goal of the manuscript. I found it difficult to find a connection between both. Previous studies show that there may be a trend in SSS, and that this trend could be due to accelerated warming, but why should 'regimes' be present in the series ? why is this particular statistical model more suitable than others and what can we learn more from this type of analysis that from a simple correlation to sea-surface temperature, for instance ?

3/ Related to point 2, the study is essentially an analysis of the SSS time series, but there is little comparison with other variables that may justify some assertions made

in the manuscript. For instance, in several paragraphs the authors mention accelerated warming or accelerated hydrological cycle, supported by references to previous studies. However, it is statistician quite difficult to identify these accelerations. The satellite record of sea-level does not show an acceleration over the period 1993-2014, and the comparison between sea-level reconstructions in the 20th century and the satellite altimetry as indication of acceleration is quite contended. Similarly, the rate of global mean temperature has not clearly accelerated. The current discussion is rather whether or not the warming has slowed down in the last 20 years. The manuscript would then benefit from a comparison with, say, surface-surface temperature. Does this variable also show regime changes coincidental with the SSS break-points so that a more clear attribution can be established ?

More particular points

4/ For some readers it would be helpful to indicate somewhere in the manuscript (perhaps in the data section) that salinity units are PSS-78

5/ The intensification of the global water cycle is expected to be occurring at a rate of 8 % degree of surface warming (Durack et al., 2012) or around 20 % considering the projected 2–3 of temperature increase over the next century.

'Expected to be occurring' sound weird and is a bit misleading. Perhaps the authors mean that the present rate has been estimated as .. . But this sentences mixes the estimation of the present rate with protectives changes, which are lower, of the order of 2% /K. There is therefore a mismatch between the current estimate rate and the projected rate.

6 Around equation 1, the symbol G is used to denote different concepts. On page 988, line 21, it denotes a gaussian distribution of zero mean. In line 27, G is a different distribution.

7/ On page 989, the symbol Psi is used to denote different meanings. In line 2, it

denotes the unobserved state, in line 3, it denotes the observed data set

8/ Using the eigenvectors of Σ , functions that are meaningful for times when

what does 'meaningful' mean here ?

9/ The described salinity change acceleration is likely the result of global hydrological cycle intensification as was suggested by Hosoda et al. (2009) and Durack and Wijffels

It is not clear to what regime this sentence is referring to. If it is referring to the break-point in 1990, I think it is arguable that there has been an acceleration of the warming rate in 1990-2009 compared to the 'previous' period, depending of what previous is meant here. My point is that a break point in 1990 does not seem to fit with a break point evolution of the global temperature data, nor with accelerated temperature trends over 1990-2009. If the regime would encompass 1990-1998 for instance, it would be much more plausible. In other words, if the SSS are driven by global temperatures, why is no break point detected in 1998 ?. Also, the trend in global temperatures over 2009-2014 (although this is indeed a short period) does not really stand out as a period with clearly strongest warming trends compared to say 1900-1998. Independently of the question of attribution of the temperature trends, I think that a more nuanced link between SSS trends and global T trends is needed.

Interactive comment on Ocean Sci. Discuss., 12, 983, 2015.