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

Anonymous Referee #2

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The paper presents estimation of trends and possible regime shifts of the EN4 SSS product from 1950 to 2014 with the possible addition of SMOS-derived SSSS fields in 2009-2014.

The methodology is promissing, but requires careful estimates of errors of the product and I suspect a fairly homogeneous data set. Whether the EN4 fields can be used for this purpose is to my sense in question, due to large changes in data coverage, data origin and level of validation of the data.

There have been published analyses of the dataset, suggesting largely insufficient data coverage in large parts of the domain before the 1970s, for example (Skliris et al, 2014), and probably inhomogeneities in data processing and data validation, which are suggested by some of the features on the maps of averages or spatially-averaged time

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series (Fig. 3 to 5). For example in 1990-2009, large negative anomalies are found in the equatorial Atlantic Ocean. These anomalies do not appear in other analyses of SSS. There is indeed a low in late 1990s, but it should be rather compensated by higher values after (and also lower values before the 1980s for regime A). I would question whether doubtful PIRATA/TAO/RAMA near surface S data have been removed or incorporated in the analysis. I personally estimate after examining a selection of the records that more than 10% of those records present moderate to large biases (> 0.1 psu), and they were still included until recently in the PMEL 'validated' files. How these are edited out can strongly influence the analysis in the equatorial oceans since the 1990s (see also, the huge anomalies for a few months in the equatorial Pacific near 1998: which area is averaged and is it associated with ENSO 1997-1998?surprisingly one does not see the well-documented earlier ENSO signals in this time series (late 1982/early 1983); compare for example with Singh, A., and T. Delcroix, 2013. Eastern and central Pacific ENSO and their relationships to the recharge/discharge oscillator paradigm. Deep Sea Research, 82, 32-43. Or Singh, A., T. Delcroix, and S. Cravatte, 2011. Contrasting the flavors of El Nino Southern Oscillation using sea surface salinity observations. J. Geophys. Res., 116, C06016, doi:10.1029/2010JC006862, or Singh, A., and T. Delcroix, 2011. Estimating the effects of ENSO upon the observed freshening trends of the western Tropical Pacific Ocean. Geophys. Res. Let., L21607, doi:10.1029/2011GL049636.) I am also surprised by the moderate positive anomalies in parts of the northwestern subpolar Atlantic in 1990-2009 compared to 1950-1990. In my estimates, it depends to some extent whether the end years are included, and whether strong anomalies (often insufficiently sampled) on the nearby shelves are included in the estimates or not. Because of radii of influence, these shelf values can have large influence through a large part of the ocean. The question on the data set validation is reinforced when seeing the curve for the Mediterranean. There has clearly not been a basin-average anomaly of -0.7 psu in the late 1990s (I wonder whether this is due to erroneous data or influence from insufficient data in the Adriatic or in the Back Sea that pollute the mapped values in the Mediterranean Sea).

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If one does not have full confidence in the means over the different periods, one can also question the confidence one has in the method used to identify changes in the trends. One should question whether the different lengths of periods identified by the algorithm might result from the different degree of data coverage (or data validation) for the different periods. There might be less variability in the EN4 product in some parts of the world ocean before 1990s because there was simply not enough data. And it is only after the mid-2000s that there is sufficient data coverage in the southern ocean (surprisingly, I don't see it springing out on the plots in figure 4 and 5, but maybe it shows in the EOFs of figure 6) (there are also issue with the elephant seal data that provide some of the data coverage in recent years: is it only the new validated data sets that are used, or the original 'biased' data transmitted in real time that are used; they are also issues on the Argo data validation/correction in this region). These strong issues also show in the principal components of the EOF1 and 2, which are rather suspicious (and not just near the end; see also the abrupt changes in 1955).

When using SMOS for the recent period (figures 8 and 9), what jumps out of the figures is average biases and possible trends in the SMOS data. This is acknowledged at the end of section 3, but in this case it is difficult to assess what this brings to the scientific argument of the paper.

After these preliminary comments, I would suggest that the authors carefully address the issue of how data homogeneity, errors or mapping techniques might have affected the results. This could be done with synthetic fields and knowing what is the data distribution entering into EN4 (or with the EN4 analysis error estimates). It would be very helpful, because as presented here, there is no indication that the method might be able to identify such regime changes. If the method's performance is better characterized, it might indeed be interesting to identify large scale changes in trends. The possible change in 2009-2010 compared to the previous two decades is indeed potentially revealing, and indicating how well this can be identified needs to be reinforced. I am not sure at this stage whether the SMOS biased data set should be included, as it

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is not too clear to me what additional information this brings.

Another option would be to critically assess the properties of the EN4 SSS product, but in that case it needs to be complementary to what has been done in Skliris et al. (2014).

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

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