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Ocean Science Discussions

Interactive comment on "Variability of heat and salinity content in the North Atlantic in the last decade" *by* V. O. Ivchenko et al.

Anonymous Referee #1

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Review of the manuscript "Variability of heat and salinity content in the North Atlantic in the last decade" by V. Ivchenko et al.

The issue of climate relevant changes in the World Ocean is of great importance and is discussed in the scientific literature. The manuscript aims to quantify T- and S-changes in the North Atlantic – a region of the World Ocean known for its high variability.

My opinion is, that the manuscript does provide new useful estimates of the inter-annual changes in the ocean. However, I have strong reservations with respect to how the analysis is done in this study so that I can not recommend the current version of the manuscript for publication.

I would suggest the authors to re-work the manuscript profoundly and to re-submit it again.

As I was reading the manuscript I was frequently confused by the language and terms that the authors used. As evident from the authors' names English is not the mother-language for two of the authors, but it looks that it were exactly these authors who mostly contributed to the written text. WHY???? I strongly advise the native English speaking co-authors (or colleagues) to review the text before its possible re-submission.

Major Comments:

1) The introduction part should be rewritten. The authors posed three questions (lines 44-49, with only one of them (the last) being really addressed in the paper. In the literature review on global warming a new paper by Levitus et al. (2008) must be used instead of Levitus et al. 2000. Also, the reference to the paper by Domingues et al. (2008) must be added.

Line 110: "The main aim of this study is to calculate heat/salinity content". In fact, the heat content ANOMALY is estimated in such calculations. Throughout the paper the word "anomaly" is used to characterize the DEVIATION from some reference state (in this case it is the monthly climatology by Stephens et al. 2002) and the authors tell it explicitly. This reference state is obtained from a quite different ("independent") data set. The more common use of the word "anomaly" rather refers to a deviation from the mean based on the SAME dataset.

As acknowledged by the authors, the reference dataset may be biased due to instrumental problems (also the sampling is different!), therefore Figs. 1 to 4 have no physical sense in terms of heat or salinity anomaly as such. As the authors note in Lines 248-250, their heat content is lower compared to Levitus climatology – the fact which might relate totally to the biases in climatology, but not to the variation in the heat content.

Respectively, Plots 1-4 characterize rather problems due to different data and sampling. For this reason estimates of the “anomalies” (Line 229) are senseless unless a detailed study of the reference climatology is done. Any change of the (independent!) reference state automatically changes the “anomaly” value. One could repeat the same calculations using, say, Levitus climatologies each time obtaining different “anomalies”: what physical sense will they have???

Throughout the paper there is a lot of discussion on negative or positive anomalies, which in fact represents ONLY a deviation from the reference state, which, as I said, might be strongly biased (overall and regionally) due to instrumental biases and to a different averaging time period.

As noted in the text, the reference climatology is biased to the last half of the 20-th century. However, this “mean reference time period” varies strongly regionally due to a much more irregular sampling before Argo. Respectively, the deviations from the reference field (and it is what is mostly discussed in the paper) will inevitably mirror such irregularities. In this case the basic time period for the reference climatology is too uncertain/unknown for the climatology to be useful in estimating changes in time.

Of course, the independent reference climatology can be used but only in order to arrive on the anomaly time series (Fig. 10-12). As the authors write on lines 378-379 the trends might be almost free from any bias – a statement I partially agree with. In the literature time series similar to plots 10-12 are normally adjusted to some reference year, or an overall mean is subtracted. For this reason, Figs.1-4 should not appear in the paper, as the focus of the paper is not on biases in the data (climatology).

I would agree on using some reference climatology as it was done in the paper, but only if 1)the climatology were for a known (fixed) reference time period 2) biases were eliminated and 3) errors due to a different sampling (climatology vs Argo) were estimated.

2)__I appreciate authors' efforts to estimate the reliability of their estimates by conducting experiments with the reduced data size. However, the results should be better represented by comparing respective time series (similar to Fig.5). Such a figure should replace the present Figs. 3-4. The reader might be mostly interested to know if the produced time-series/trends are stable.

3)_The description of the obtained anomaly time series is unsatisfactory (Section 4). In case of temperature (Fig.5) the HCA in 1999 is exactly the same as in 2009, so that the preference of a linear trend model is at least questionable and deserves a proper discussion. My personal view of the time-series in Fig. 5 is that a simple sinusoid oscillation with a 10-year period (1999-2009) much better explains the variability compared to a simple linear trend. The time series is simply too short to allow definitive conclusions about the character of variability. The linear fir model is often useful, but it has its limitation when the series is short and variability high.

4) I have not understood a small discussion on the “problem of synchronization” of observations Lines 82-99). Do the authors mean that sampling is made randomly in time? If yes, argo floats do not sample in a synchronized way as well.

5) The summary and discussion section is unsatisfactory. Partly it happens because of a poor English. How the reader can understand for instance the sentence: “The 10 years of observations does not produce a clear view about variability of these (i.e. vertical distribution of time averaged) fields, because of decadal variability.” ?????

6) Computation of the SCA is subject to errors in salinity (both in the reference climatology and Argo). However, a discussion on possible instrumental errors is totally missing. Due to a generally much larger noise in salinity data (compared to the signal) for the lower parts of the water column at least an attempt of the error estimates is desirable

Minor comments: 1)I would suggest HCA and SCA as abbreviations for anomalies– these are normally used in the literature.

2)Lines 179-188. The authors describe the method of the optimal interpolation. However, immediately after that section (Lines 188-196) they write about "testing the influence of this constraint". It is not clear what the word "this" points to. Obviously, they describe the experiments with different outlier rejection criteria, but this should then rather follow the line 175.

3) I do not understand the link between the stability of the estimates and the specific behavior of Argo buoys being driven by currents (Lines 208-217). Yes, the movement of buoys is stochastic, but it is the number of profiles per area alone what is important for the optimal interpolation. The movement of buoys is not relevant at all!

4) Avoid phrases like "In Figs.9,9 one can see.." (Line 293)

Interactive comment on Ocean Sci. Discuss., 6, 1971, 2009.