The previous version of the paper submitted to the `Ocean Science' has been substantially changed, according to the comments by two reviewers and Dr Sarafanov. The questions raised by reviewers have been addressed.

In light of recent comments from the Argo Centres we applied a more stringent quality control on the Argo data to address this problem, which required the data to be reanalysed. In addition a bug was found in calculating volumes that has been corrected. As a result the temperature and salinity are similar to the previous version of the manuscript, but heat and salinity content has changed. Some of the figures and tables have changed in a response to the reviewers comments, which includes a new additional trend analysis. We have addressed the reviewers comments below.

Reviewer 1

- 1.1 `*I strongly advise the native English speaking co-authors to review...*'
  -Done

- 1.2 `*The introduction part should be rewritten. ...In the literature review on global warming a new paper by Levitus et al. (2008) must be used...'*

  -Done. We believe, that the Reviewer probably meant Levitus et al. (2009), which we have used. Also, we have included a number of recent papers in the text of manuscript. Here, the total number of cited papers is now 43 compare with 29 as before.

- 1.3 `*Throughout the paper the word "anomaly" is used to characterize deviation...*'

  -Done. We have changed the word "anomaly" to "deviation" as suggested by the reviewer.

- 1.4 `*As acknowledge by the authors, the reference dataset may be biased due to instrumental problems... therefore figs.1 to 4 have no physical sense...*'

  -We do not agree with this point. One of the most challenging aims of this study is to show that the used algorithms are stable. That is, by removing 50% and even 75% of the data does not strongly change the integral values, represented in these figures. Thus, to partially satisfy the Reviewer's comment we have removed half (i.e. two) of these figures. Also, it is important to note that the values of heat/salt content deviation depend on the climatology, but the TRENDS do not.

- 1.5 `*Throughout the paper there is a lot of discussion on negative or positive anomalies, ...might be strongly biased...*'
  -Done

- 1.6 `*I would agree on using some reference climatology... but only if 1) the climatology were for a known (fixed) time period ...'*

  -We are using well-known Levitus climatology. It is not our aim to develop a new climatology, and estimate biases in it. And (as we noted above) the

absolute values of deviation of heat/salinity content could be changed, if use another climatology. As previously mentioned the trends are not affected by a bias in the climatology (see 1.4). Saying HCD is positive/negative at time t, we meant that the heat content is positive/negative relative to the Levitus climatology, but not relative to the reference frame of true time of the Levitus climatology.

- 1.7 *`The results should be better represented by comparing respective time series (i.e. 50% and 75% removed data -voi) ...'*

  -Done. We have improved the analysis for trends for the experiments W, H, and Q (see Tables 1 and 2). In addition, we have included an additional feature of representing the seasonal cycle.

- 1.8 *`...the preference of a linear trend model is at least questionable...'*

  -Done. We have improved the analysis to include the periodical signal (model LTPO) (see Sect.2, Eqs.1-4 and all the Tables).

- 1.9 *`I have not understood ..."problem of synchronization"...*

  -Done. We have clarified this in the text.

- 1.10 *`The summary and discussion section is unsatisfactory.'*

  -Done

- 1.11 *`Computation of the SCA is subject to errors in salinity... at least an attempt of the error estimates is desirable'*

  - This is out of the scope of this study, however Resnyansky et al. (2010) has calculated the signal to noise ratio for temperature amd salinity of Argo data (see Sect.1).

- 1.12 *(Minor comments): `I would suggest HCA and SCA as abbreviations ...'*

  -Done: we have addressed this point in the text.

- 1.13 `Lines 179-188... It is not clear what the word "this" points to...

  -Done

- 1.14 *`Yes, the movement of buoys is stochastic, but it is the number of profiles per area...'*

  -Done. The number of buoys at time (t+1) strongly depends on the strength of the current and on energetic stochastic (eddy) dynamics, and not only on number of buoys at time t. Big variation in numbers of buoys at time t and (t+1) could result in instability of analysis.

- 1.15 *`Avoid phrases like "In Figs. 9.9 one can see...'*

-Done

Reviewer 2

- 2.1 *`...a deeper examination of the profiling float data distribution and quality affect the estimated trend'*

  -Done. See Sect.2,3,4, and all new Tables.

- 2.2 `...whether a linear trend is really the best way...'

  -Done

- 2.3 *`Much more information about data distributions needs to be added'.*

  -Done, we have included three new figures describing the data distribution in time and space.

- 2.4 *`More information about data handling/data quality needs to be added'*

  -We used the most recent list of good Argo data available. Also, as an extra constraint to the Argo data set we applied our own quality control to reduce possible outliners. See Fig.4 and the Sect.2.

- 2.5 *`Does a linear trend give the best indication...?'*

  -Done, we have already addressed this comment.

- 2.6 *`The authors cite Resnyansky...'*

  -The thorough comparison of different studies requires to be done for the same region/depth and for the same period of time. This is not the intention of our study to do this particular comparison analysis. However, according to the Reviewer's comment we made a new estimation and comparison with Levitus (2009) which is in an agreement with the cited paper. (see Sect.5, the first paragraph).

- 2.7 `The authors should offer ... analysis and comparison...'

  -Done, (see 2.6)

- 2.8 *`Whether the authors have detailed... Lyman and Johnson (2008)...different default...in data sparse areas can have a significant effect...'*

  -We have addressed this in Sect.2 and Tables 1-2.

- 2.9 *`What does "near random" selection mean? ...What does the data coverage look*

like for the 1/2 and 1/4 dataset cases?'

- Done. We have removed the word `near'. From a theoretical (mathematical) point of view it is not a random, but in practice it is. We also have checked the spatial distribution in the experiments W,H, and Q and found that the coverage in H and Q experiments to be reasonably good in terms of whole basin. (see Figs. 5,6). For this purpose we also have produced a new Tables 1,2
-

- 2.10 *`Why is the log of the results...?'*

  - HCD and SCD are much smaller in the deeper ocean, than in the upper ocean. Therefore it is easier to represent the data in terms of using logs.

- 2.11 *`...problem of synchronisation'.*
  -Done

- 2.12 *`What is the importance of the lack of data along the shelf region, Gulf of Mexico, others...?'*

  - If the volume of such a shelf region (V_{sh}) is much smaller than the whole North Atlantic volume V (or part of North Atlantic), then the contribution to the heat/salinity content will be small (remember, that the heat content is an integral over the study area). We made a number of experiments by cutting off some 10°x10° areas and compared this with the whole data set. We found, that if $V_{sh} << V$ then heat/salinity contents will be similar.

- 2.13 *`Chang et al. (2009) ...found that they were warmer ...Please explore this further.'*

  - This is an important issue. As far as we know Dr Gouretski, as well as number other specialists are addressing this issue. However, this is outside the scope of our study.
  -

- 2.14 *`How is level of significance calculated?'*

  -Done, see Sect.2

- 2.15 *`The only misspelling I found was ..."uncorrecatble"*

  -Done

Response to Dr Sarafanov

- S.1 *`...a weak point of the paper is rather poor interpretation of the results.'*

  In order to understand mechanisms responsible for the variability of heat/salinity contents we need to establish a reliable data set. To address this issue the Argo data (as a Lagrangian variable) should be studied in terms of stability, possible errors and significance.

- S.2 `*...the local heat content is also linked to the dynamical processes*'

  -Done

- S.3 `*...these results should not be ignored...'*

  -Done. A number of recent papers were included in the text (see also 1.2)

- S.4 `*In general, the results of the listed studies point to the following main causes .... in the northern NA...'*

  -We have discussed some of these points about possible variability in the northern North Atlantic in the text (see Sect.5). The thorough analysis of the mechanisms of variability of heat/salinity contents in the North Atlantic (subregions), is the subject of further studies, that is outside the scope of this paper.

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