

Interactive comment on “Oceanic dominance of interannual subtropical North Atlantic heat content variability” by M. Sonnewald et al.

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We greatly appreciate the reviewer’s efforts in commenting on the manuscript. Here we reply to the points raised in the order they were mentioned by the reviewer. The comment from the anonymous reviewer is presented in italics, and our reply in normal font.

1. Although I do not mean to comment on the style of the paper, I did find this very difficult to understand what the main results of the paper were; for example the abstract states “the main goal of the study is to identify to what extent the ocean heat content variability is of atmospheric or oceanic origin”, and makes no mention of

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the results regarding the actual observed changes in heat content. The latter point seems to be, at least to me, the most significant result of the paper. I would recommend a substantial rewrite to make the main points of the paper much more clear to a reader.

We recognize that the motivation of the study is unclear in the abstract and overall text. In a revised version of the paper we will reorganize the introduction substantially to clarify the motivations for the study, and adapt the abstract, discussion and conclusion where appropriate (see also reply to reviewer 1). We agree with the reviewer that we should highlight the use of the observational data, and see this as our main motivation. In a revised version, we wish to highlight that the overall goal of the study is to demonstrate that the meridional heat transport observations from RAPID can be used to estimate the oceanic contribution to the ocean heat content variability in the subtropical North Atlantic. To accommodate this, we will change the title to: “Atlantic ocean meridional heat transport at 26°N: Impact on subtropical ocean heat content variability” which highlights the use of the RAPID array. With such a restructuring, we hope to make the main achievements of the paper clear to the reader.

2. Carrying on from point 1. Generally the abstract seemed too long, and too focused on the details rather the main conclusions; The introduction poorly motivated the study (What work has been carried out before, and what were the main open questions remaining that were being tackled in this study?). The discussion was also very difficult to follow, I recommend shortening this to the key points. Finally, the way the results are presented was also confusing, as the authors did not make it clear what was driving their FV model and when. For example figure 5 clearly uses the results from OCCAM, figure 6, the FV model uses the RAPID array, and then figure 7 uses....I don't know what! I suggest the authors first validate the model using the model derived data, and then apply the FV model to the observations to make it clear

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to the reader.

As we discuss in more detail in our response to reviewer 1, the reorganization of the introduction will contain a summary of previous work where we highlight how previous studies compare to ours, and clearly outline what open questions we will address. We are also happy to change the abstract to be more brief and focused on the main conclusions.

There has been some previous debate in the literature regarding the drivers behind OHC variability with publications such as Grötzner *et al.* (1998); Deser and Blackmon (1993); Kushnir (1994) suggest that decadal ocean heat content variability in the North Atlantic is due to a coupling between the ocean and atmosphere expressed through unstable air-sea interactions. While work such as Seager *et al.* (2000); Cayan (1992); Bjerknes (1964) argue the opposite, suggesting that the large scale atmospheric circulation is the dominant driver. However, our study illustrates quantitatively how the interannual ocean heat transport increases in importance from monthly to interannual in a basin-wide section of the subtropical North Atlantic using both model and observational data.

Relevant recent work includes work by Dong and Kelly (2004); Dong *et al.* (2007). The main conclusions of our study concerning the OHC variability concur with these studies. However, unlike Dong and Kelly (2004); Dong *et al.* (2007) our study does not focus on the Gulf Stream region, but considers a basin-wide section of the North Atlantic. We chose the region 26-36°N on the basis of studies such as Bingham *et al.* (2007); Grist *et al.* (2009). These suggest that there is a significant character change in the circulation around 40°N, which our focus region avoids unlike Dong and Kelly (2004). However, we still include the region of maximal heat transport as well as including the region where the dry atmosphere, ocean and latent heat transports are

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approximately equal (Bryden and Imawaki, 2001).

Moreover, the goal of our study was to assess the ocean heat content variability in a region that covers the entire basin width. Dong and Kelly (2004); Dong *et al.* (2007) rely on satellite altimetry to estimate the geostrophic velocity in regions located in the western parts of the Atlantic, which is a valid approach to infer geostrophic transports in regions where large SSH gradients occur (e.g. across a western boundary current and its eastward extension). However, it is not necessarily a good indicator for basin-wide transports. Kanzow *et al.* (2009); Hirschi *et al.* (2009) show that good transport estimates can be obtained for partial sections across an ocean basin but not for basin-wide sections. This is due to a decreased correlation between SSH and meridional transports close to continental margins. Investigating the basin-wide OHC variability requires knowledge of the basin-wide transport. To us this seems an excellent opportunity to study the extent to which the heat transport across 26°N can explain the ocean heat content variability in the subtropical North Atlantic.

Furthermore, our work extends that of Dong and Kelly (2004); Dong *et al.* (2007), by using different timeseries and illustrate that their findings are consistent for the basin-wide section. We demonstrate the validity of using the RAPID data to assess the OHC variability, validating our modeling approach using 20 years of high resolution OGCM data from OCCAM before applying it to the RAPID data timeseries from 2004 to 2011.

Regarding the discussion and results, as mentioned in reply to point **1** above, we will change the discussion to accommodate the restructuring of the introduction. We appreciate the suggestion of focusing on the key points, and will be happy to do so in a revised manuscript. In the results section we will restructure the presentation of the results more clearly into the advective version (AV) and flux version (FV) of the box

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model, clearly stating how they were forced. For the model validation in figure 5, we will provide more detail in the text and figure caption to highlight how and why we used OCCAM to validate the FV of the box model.

3. I did not fully understand all the choices that were made with the box models, and I think more justification is needed in the text. For example, why use the latitudinal mean temperature to derive the ocean heat transports. Using latitudinal mean temperature ignores much information about other processes, such as changes in the gyres, or eddies etc. Is the point to suggest that those processes do not matter to the heat content change in the subtropical gyre region as much as the overturning? If so the authors should make this clear, and they should quantify this further using the model data to explain why the errors do or do not arise (i.e. what more do we have to know, apart from overturning component of the heat transport). Quantification of the errors seems especially important for the FV model especially, given the extra assumptions used (e.g. the relationship of the heat transport at 36N, the climatological mean atmospheric heat flux). Also, why was the climatology of the NCEP reanalysis used as the atmospheric heat flux in the FV model, surely the NCEP data goes beyond 2006 or other products that could be used to test the sensitivity (this merges with point 4 below)?

The reviewer refers to equations 4-6 given in the text, which were unfortunately wrong (see also the response to reviewer 1). The meridional heat transport (MHT) used was calculated as the depth (0 to 800 m) and basin-wide longitude integrals of the temperature (T) multiplied by the velocity (v) component normal to the section:

$$MHT = \int_W^E dx \int_0^{800m} dz T(x, z)v(x, z)\rho c_p.$$

Thus we include eddies and the gyre circulation. We leave out the horizontal mixing terms, but figure 4 and 5 are used to illustrate that the diffusive term is not large by

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comparison with the full OCCAM model where the horizontal mixing is included.

The errors resulting from the main assumptions made in the FV box model are illustrated in figures 4 and 5, and we will describe more clearly how this validation was performed. Errors relating to the use of the climatology have not been illustrated, but we are happy to do so in a similar fashion. We are aware that NCEP reanalysis products are available beyond 2006, but unfortunately the OCCAM project only ran until 2006. The air-sea fluxes used to force the box models were those obtained when feeding NCEP into the bulk formula used in OCCAM. Thus, although the NCEP data is available we cannot extend the air-sea fluxes beyond 2006. For consistency, and to make the comparison of the AV and FV box model as fair as possible we decided to use the same air-sea fluxes where available, and thus using a climatology for the remaining time in the AV box model seemed reasonable.

4. The comparison with the observed heat content change (figure 8) was not entirely convincing. Clearly there is agreement, which is very interesting, but there are times the agreement is rather poor, take the drop in 2004/2005 for example, which is predicted to be very abrupt in the AV box model, and the missing of many peaks and troughs. The choice of boxes to average ARGO data across for the comparison was also rather puzzling, and comes back to the choice of 26-36N. Surely there are other data sources that are available here? Also, are there errors for the time series of ARGO data?

The choice of 26-36°N was mainly motivated by the location of the RAPID array. We chose to extend the region northwards to include the region where the dry atmosphere, ocean and latent heat transports are approximately equal and the maximal heat transport occurs (Bryden and Imawaki, 2001). The decision to look at a 10° section was motivated by Bingham *et al.* (2007); Grist *et al.* (2009) who suggest that

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the meridional coherence changes around 40°N, thus the region 26-36°N seemed a natural choice.

As we stated in our response to reviewer 1, Dr. McDonagh and Dr. King have kindly agreed to supply a more suitable timeseries of the equivalent area from ARGO data, as well as the EN3 data shown in figure 1. The EN3 and ARGO product timeseries domains overlap exactly with the domain used in the box model, and in a revised version of the paper we will use these instead of the data from Ivchenko *et al.* (2010). Dr. McDonagh and Dr. King suggested we discard the data prior to 2004, as it could be unreliable due to insufficient data coverage. Thus we will focus the validation of the FV box model results based on their expert opinion.

Of particular interest is the OHC evolution after 2007, as illustrated in figure 1. In ARGO, EN3 and FV there is a period of reduced OHC variability in 2007 and 2008 which is then followed by a pronounced OHC decrease in 2009 and into 2010. Even though there are differences in the precise timing of the OHC reduction during this phase we see a broadly similar evolution in ARGO, EN3 and FV: from 2009 to mid 2010 the temperature in the subtropical ocean box reduces by about 0.2°C. The box model results (FV) strongly suggest that this temperature change can largely be explained by a reduction of the meridional ocean heat transport at 26° which coincided with the pronounced MOC minimum described in McCarthy *et al.* (2012).

This is in contrast to the events prior to 2007 where figure 1 illustrates that there is a larger uncertainty about the nature of these events. Differences between ARGO and EN3 may reflect differences in the observational data used in EN3 compared to ARGO (uncalibrated ARGO and other hydrographic observations in EN3, calibrated ARGO float data only for the ARGO OHC product). The main factors contributing to the differences for FV are the surface forcing, as well as the simple closure used at

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36°N. However, we also note that the differences with ARGO/EN3 are not in general larger than the differences between EN3 and ARGO meaning that we cannot say how robust the OHC variability observed in ARGO and EN3 actually is prior to 2007.

Furthermore, the discrepancy between ARGO and EN3 could be due to changes in the data coverage. Figure 2 illustrates the change in available profile data over this time period for the region 10°N-70°N; 90°W-0°, which is qualitatively representative for our region. The discrepancy between the FV box model and ARGO/EN3 prior to 2007 could be due to errors from poor data coverage. In a revised version of the paper, we will illustrate this using data similar to that displayed in figure 2 for our region. Errors could further be introduced through the location of the floats, and we will include this in our metric by looking at the number of occupied 3° boxes in our domain.

Moreover, we have not been able to obtain error estimates for the ARGO product. Error estimates are currently lacking in the ARGO product due to uncertainties in the temporal and spatial correlation length scales. We are waiting for a response regarding the EN3 product.

In a revised version of the paper we would place the main emphasis on the OHC reduction observed in 2009/2010 and we would add a discussion as to why the interpretation of earlier events is more uncertain.

Minor comments

I did not understand the need to show the seasonal anomalies at the same time as the interannual (actually, they are just deseasonalized?) anomalies in say figure 6, as the key result is what drives the interannual variability. I suggest showing that the atmosphere accounts for the seasonal cycle, and then just show the interannual

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variability. some minor suggestions for clarity. Use seasonal cycle or annual cycle when describing changes in the heat content that is associated with the progression of the seasons. Use deseasonalized or anomalous for everything else to make it clear. Try to use lucid repetition, especially when discussing the different models; try to stick to one form, e.g. the FV box model, or the FV model, it will help the reader if they keep seeing the same set of words.

We will divide figure 6 into four panels to illustrate the seasonal and interannual components of AV and FV box model separately.

We appreciate the suggestion to use consistent language and lucid repetition, and will do so in a revised manuscript.

Lastly, we look forward to producing a revised version of the manuscript and would greatly appreciate further comments from the reviewer. We wish to thank the anonymous reviewer again for the helpful comments.

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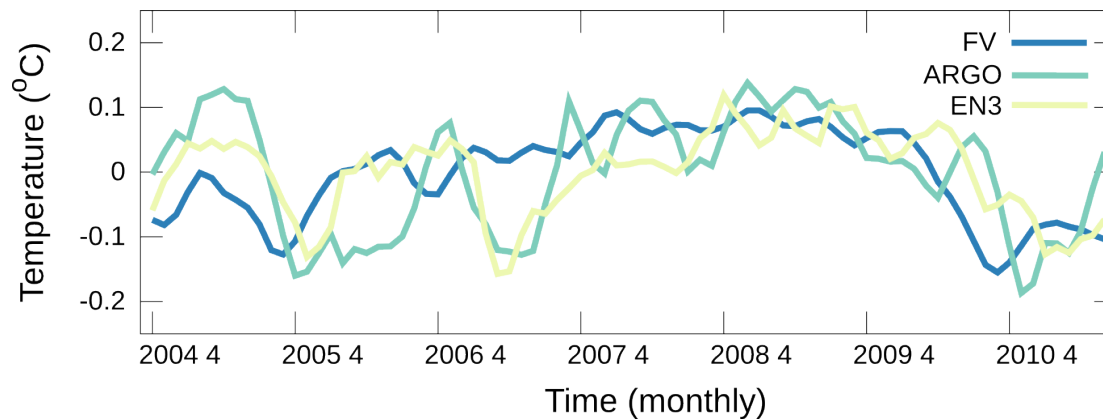


Fig. 1. Temperature, detrended and mean removed, for the domain used: 26–36°N, upper 800m. We show ARGO (green), EN3 (yellow) and our FV case (blue).

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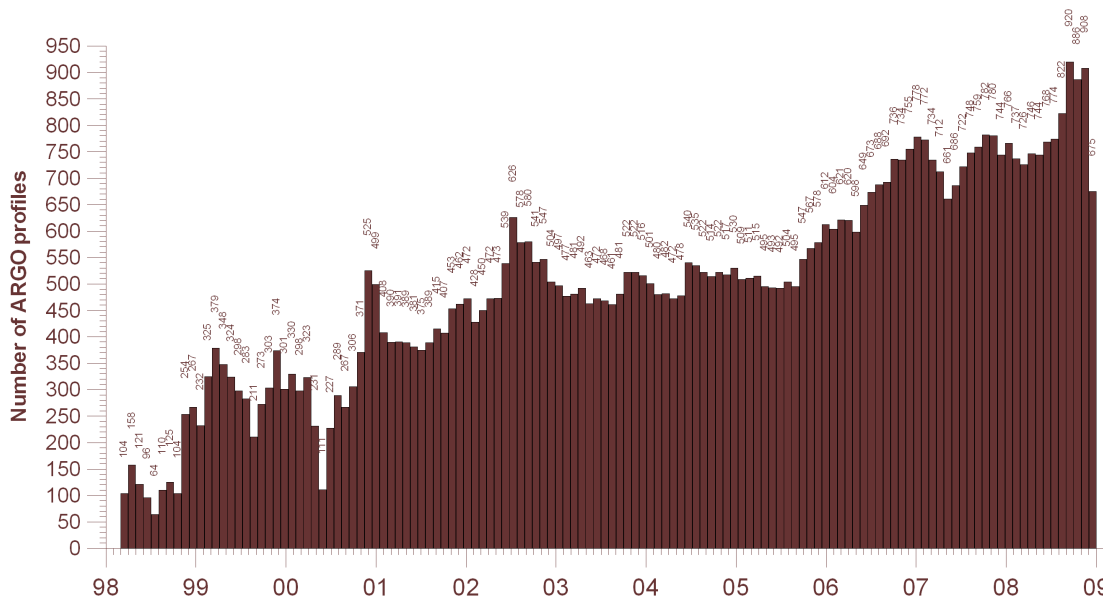


Fig. 2. Number of profiles for a given time in the region 10oN–70oN; 90oW–0o. Note the increase between 2004 and 2007. From: noc.soton.ac.uk/ooc/PROJECTS/MONACO/

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