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Ocean Science
Discussions

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

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We thank the anonymous reviewer for her/his efforts in commenting on the manuscript. In response to the comments, we have carefully considered the concerns raised, and are confident we can address them. We hope to produce a revised manuscript to which we would greatly appreciate any comments or feedback. The structure of our reply is as follows; each comment from the anonymous reviewer is presented in italics, and our reply in normal font.

First of all, in my opinion the study is poorly motivated. Many studies (say, Vivier et al. 2002) have looked at ocean heat content variability, and few, if any, leave reason to

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> *believe that atmospheric heat fluxes play an important role in ocean heat content variability on interannual time scales. So what is the main problem here? Why 26N-36N? Are the authors interested in the AMOC-related climate signals? Predictability? Is the goal to see if the RAPID data can be used to estimate heat content? There is some discussion regarding Hurricanes in the Conclusion segment, but if that is a main driver for this study, it is awkwardly out of place. . .*

...

> *The paper is rather poorly written. The Abstract seems to contain a bit too much detail; the Introduction, in addition to a clear motivation, could use more background and discussion of previous work (e.g., can you be more specific about the Grist et al. study? What other studies have looked at OHC from observations or box models? Have other approaches been used to address the same research question? What questions remained unanswered that will be addressed here?);*

We recognize that the introduction was quite brief, and greatly appreciate the suggestions for improving the writing. To this end, we will reorganize the introduction substantially to clarify the motivations for the study, putting more emphasis on discussing our work in the context of previous studies, provide a more suitable background and modify the discussion and conclusion where appropriate. We will 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.

The study by Vivier *et al.* (2002), pointed out by the reviewer, investigates the Kuroshio current region, while its complementary study Dong and Kelly (2004) considers the Gulf Stream region (25-$45°$N and 40-$85°$W). The main conclusions of our study concur with those of Dong and Kelly (2004). However, our study differs from these in that we do not focus on the Gulf Stream region, but consider a basin-wide section of the North Atlantic. Our study region was 26-36°N which was chosen 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 south and north of 40°N, which our focus region avoids, while still encompassing the region of maximal heat transport as well as including the region where the dry atmosphere, ocean and latent heat transports are approximately equal (Bryden and Imawaki, 2001). Furthermore, Dong and Kelly (2004) use a thermodynamic model, investigating a period from 1992 to 1999, and calculate the temperature of the mixed layer using geostrophic velocity, Ekman transport and surface heat fluxes from observations. Our study complements that of Dong and Kelly (2004), using a box model heat budget analysis, where we prescribe the fluxes through the surface, southern, northern and bottom layer of the study region. We validate our modeling approach using 20 years of high resolution OGCM data from OCCAM before applying it to to the RAPID data timeseries from 2004 to 2011. This provides a temporal overlap with the Dong and Kelly (2004) study through the OCCAM data, where broadly the same variability can be seen.

Dong *et al.* (2007) also conclude that the oceanic heat transport controls interannual changes in upper ocean heat content looking at a similar region to Dong and Kelly (2004). The study is very similar to Dong and Kelly (2004), looking at the period between 1992 to 1999, but uses an inverse modeling approach relying on subsurface data from the Global Temperature-Salinity Profile Program, satellite SSH and NCEP-NCAR reanalysis products. However, considering Argo data, Hadfield (2007) concluded that the region was sensitive to sampling issues which could lead to underestimating heating from divergence. Our use of the OGCM and the RAPID transect would not be affected by such aliasing.

Both 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 Pacific and of

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the Atlantic. However, the goal of our study is to better understand the ocean heat content variability in a region that covers the entire basin width. Whereas SSH from satellite altimetry is well suited to infer geostrophic transports into/out of regions where large SSH gradients occur (e.g. across a western boundary current and its eastward extension), it is not necessarily a good indicator for basin-wide transports. Kanzow *et al.* (2009); Hirschi *et al.* (2009) show that the correlation between SSH and meridional transports decreases significantly when moving close to continental margins. Good transport estimates can be obtained for partial sections across an ocean basin but not for basin-wide sections which have a low signal to noise ratio. However, our study requires knowledge of basin-wide transports which the RAPID observing system provides for $26°$N. 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, Grist *et al.* (2010) recently published a paper illustrating that interannual ocean heat content variability is increasingly dominated by surface fluxes as one moves equatorwards, but in the zone 26-$36°$N anomalies of MHT divergence are significant. Grist *et al.* (2010) use the NEMO OGCM, but rely on eddy permitting $1/4°$ rather than the eddy resolving $1/12°$ used in our study. Furthermore, we extend our study by including observations from the RAPID observing system.

Other 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. However, our study illustrates quantitatively how the interannual ocean heat transport increases in importance from monthly to interannual. Moreover, 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.

We concede that the title of the paper may have been a little too blunt and obvious. In a revised version of the paper we will change the title to reflect a more nuanced interpretation such as: “Atlantic ocean meridional heat transport at $26°$N: Impact on subtropical ocean heat content variability” which highlights the successful use of the RAPID array.

Overall we feel that our study represents a significant update to the state of our knowledge on ocean heat content variability in the subtropical North Atlantic, confirming and refining previous insights using both models and very recent observational data.

> *Second, the modeling approach used here completely baffles me. The authors have access to the output from a full, high-resolution ocean model. Yet, according to Eqs. 4-6, they calculate oceanic heat transports across 26N and 36N (and across 800 m) by multiplying section- integrated volume transports with section-averaged temperature in the upper 800 m. This approach ignores any contribution from the wind-driven circulation, eddies, any overturning in the upper 800 m, horizontal mixing, etc.. These contributions (or approximations of those) should be readily available from the simulation, and an analysis of these individual contributions should give a much more complete and interesting picture of the processes leading to heat content variability in the subtropical North Atlantic. The authors should motivate their choice for this simplification, and show explicitly that contributions from, for instance, the gyre circulation can be ignored. Without such a rigorous motivation, the current analysis seems pointless.*

With regard to the modeling approach, the equations 4-6 given in the text were unfortunately wrong. We did not multiply section-integrated volume transports with section-

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averaged temperature. The meridional heat transport (MHT) is 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:

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

We do not ignore the contributions from the wind-driven circulation, eddies or the gyre circulation. However, we do not include the horizontal mixing, but use figure 4 and 5 to illustrate that the diffusive term is not large by comparison with the full OCCAM model.

the Conclusion segment seems more than just a conclusion of the current study (e.g., as I said above, the discussion of Hurricanes seems out of place); the Data and Methods segment could use some more detail (e.g., it is nowhere mentioned that OHC anomalies are diagnosed from OCCAM; what is the time step used to integrate system 2-3? What is the frequency of the surface forcing in OCCAM? Daily?).

As mentioned above, the material covered in the introduction will be modified to include a clearly stated motivation as well as a more substantial background. The concluding remarks will be altered to match the introduction and the comments regarding the detail missing in the Data and Methods section will be included (OHC anomalies are from OCCAM; the timestep used to integrate system 2-3 was 5 days; The OCCAM surface forcing was 6 hourly).

p.34, l.10: The approach expressed by Eq. 10 uses a relation between F_{26N} *and* F_{36N} *that is valid only for long time scales, and hence will presumably underestimate the high-frequency contribution of the oceanic heat convergence. Can the actual time*

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series from OCCAM be used to study the error that is being made in the estimation of oceanic heat convergence on intraseasonal time scales?

The comment that the approach expressed in equation 10 used to infer $F{*}_{36N}$ using data from $F{*}_{26N}$ is a valid concern that we did consider. The regression model used to infer $F{*}_{36N}$ was made using the low frequency component of the equivalent sections in OCCAM, as the data did not suggest a simple relationship was plausible in the full forcing. However, a high frequency component is present at $36°$N. Using purely OCCAM derived data, figure 1 shows the effect of not using any forcing at $36°$N, using the inferred forcing with only the low frequency component of F_{26N} and the full F_{26N}. The case with the inferred forcing using the full F_{26N} therefore has a high frequency component. Figure 1 shows that its inclusion does have an impact, generally leading to an improved estimate with the resulting heat content closer to the full FV of the box model. The case with the inferred forcing using the full F_{26N} is the one we then use when the FV box model is forced with RAPID data.

p.38: The comparison with ARGO data is rather iffy, as there seem to be as many periods where the comparison breaks down as where there seem to be success. Would there be a more quantitative way than the ‘eye ball norm’ to make this comparison? What about error bars? If this is the main deliverable of the study, it is hard to tell whether or not the authors succeeded.

To address the reviewers comments regarding the comparison with the ARGO data, Dr. McDonagh and Dr. King have kindly agreed to supply a more suitable timeseries of the equivalent area from ARGO data. This will be used to determine to what extent the variance is captured using the AV and FV box modeling approaches. This will demonstrate quantitatively to what extent the study succeeded.

Some minor comments: p.29, l.11: ". . . the RATE OF CHANGE of heat content. . ." p.33: Has Eq. 9 been verified in OCCAM? p. 37, l.8: It seems to me that there are many reanalyses out there that could be used as forcing time series after 2006. Is there a reason why those have not been used here? p.39, l.1: ". . .the frequency spectra. . .": But these were not shown here? p.39, ll. 23-24: This seems a meaningless statement. Please explain. p.39, l.16: It seems to me that this would be a worthy cause, for which the model output would be perfectly suited. p.47: ". . .the OCCAM derived NCAR forcing. . .": Should it not be the other way around? p.52: Legends are (partly) illegible, because they interfere with the curves. p.53: "Note that the event. . .": there seem many instance in the time series where OHC anomaly at 30-40 was smaller than at 20-30, so I'm not sure what the authors are trying to say here.

With regard to the minor comments:

p.29, l.11: This has been changed.
p.33: Equation 9 has been verified in OCCAM as demonstrated in figure 2. However, the correlation is 0.96 between the MHT and the MOC at this latitude in observational RAPID data, so we were not surprised by a correlation of 0.994 in OCCAM, but will state this more clearly.
p. 37, l.8: We chose to use the surface flux field from the OCCAM project which ended in 2006 in both the AV and FV of the box model for consistency. The corresponding air-sea fluxes are not available beyond 2006. NCEP timeseries are available beyond 2006, but not the fluxes obtained when feeding NCEP into the bulk formula used in OCCAM. The timeseries used is a product of the response of OCCAM's surface field to the NCAR reanalysis, thus continuing the timeseries using the climatology seemed like a sensible choice.
p.39, l.1: This plot was not included as it simply reiterates what figures 4 to 5 demonstrate in a more accessible manner. However, we would be happy to include

this figure or exchange it with figure 5.
p**.39, ll. 23-24:** This has been removed.
p.39, l.16: A similar box model study was performed using OCCAM data by Huerta-Casas and Webb, 2012. Here they highlight the impact of computing heat budgets from model data where output was saved as averages over a five day period, and not the instantaneous model fields. A conclusive study of the missing flux in our case, and thus examining the apparent seasonality, would require non-averaged model data which is unavailable as the OCCAM project was concluded in 2006. We have contacted Dr. Huerta-Casas who commented that the missing flux would most likely be explained as an effect of the averaging, but most likely not fully addressable using the methods in Huerta-Casas and Webb, 2012.
p.47: Clarified in the manuscript.
p.52: Corrected.
p.53: This will be clarified in the context of the data provided by Dr. McDonaugh and Dr. King.

Lastly, we thank the anonymous reviewer again for the helpful comments towards improving the manuscript, and look forward to comments on a revised version.

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Interactive comment on Ocean Sci. Discuss., 10, 27, 2013.

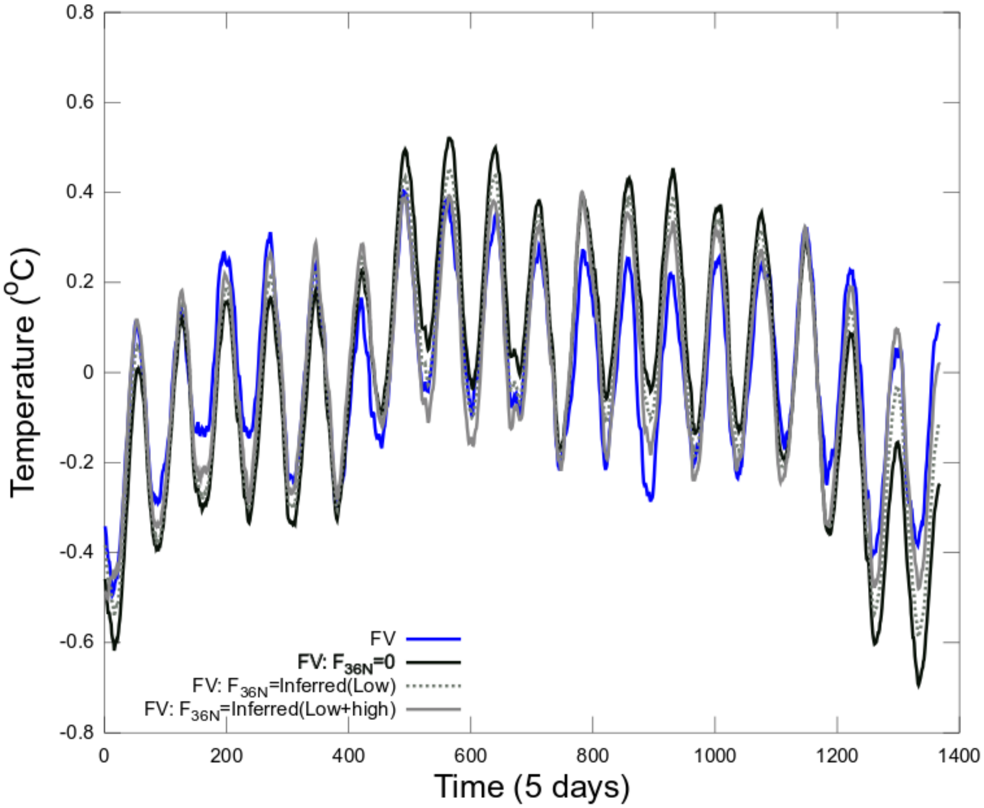

Fig. 1. FV(blue) forced w OCCAM data. We show the effect of F_36N=0 (black), using the inferred forcing using only the low freq component of F_26N (grey stippled) and the full F_26N (grey).

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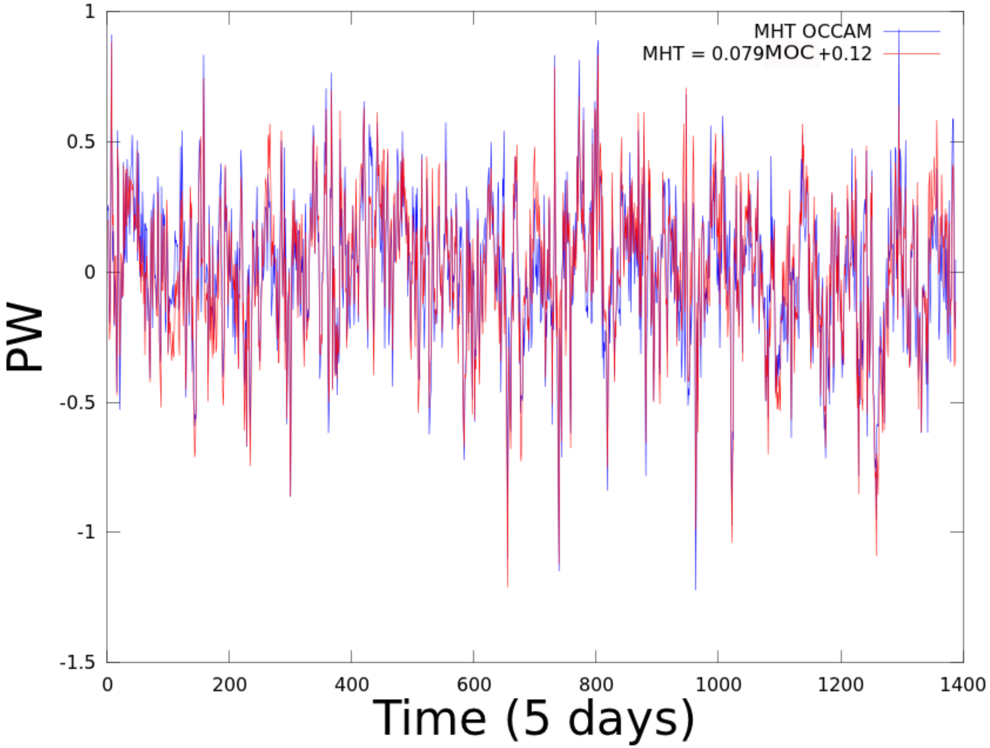

Fig. 2. Verification of eq. 9. Blue line is the MHT at 26N from OCCAM, while the red line is the MHT estimated using the relation between the MOC and the MHT: MHT = 0.079*MOC+0.12. Note correlation = 0.994.