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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.

Anonymous Referee #1

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In this paper the authors aim to "identify to what extent the seasonal to interannual ocean heat content variability is of atmospheric or oceanic origin". They construct a box model of the subtropical North Atlantic, and force it with oceanic and atmospheric heat fluxes, obtained from a GCM simulation, and from RAPID observations. They conclude that on seasonal time scales the atmospheric contribution is dominant, while on interannual time scales the oceanic contribution dominates.

How much I wanted to like this paper, I came away disappointed. Frankly, I have trouble finding any merit in it. Based on the main points below, I cannot recommend it for publication in the current form. I would be happy to provide more detailed comments on a revised version, if the authors believe that they can address the points raised below.

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 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...

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.

Some other major comments:

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?); 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?).

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

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.

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.

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