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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 #2

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This paper discusses the relative importance of both ocean heat transport convergence and atmospheric heat fluxes in driving variability in upper ocean (0-800m) heat content between the latitudes of 26N and 36N. Using output from a model, and from observations, the authors show that the atmospheric heat fluxes account for almost all of the seasonal cycle in upper ocean heat content, but that the interannual variability is driven by changes in ocean heat transport convergence/divergence. The authors then make some comments about the origins of recent changes in observed heat content.

While this paper presents some results that are interesting, especially the main drivers of the subtropical ocean heat content variability, and the discussion of the origins of the recent decrease in 0-800 heat content in the subtropical region of the North Atlantic, I have major reservations about the manuscript (detailed below). In particular, in its

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Discussion Paper

current form, it is very difficult to understand what the authors intend the reader to take as the main results. Therefore, I do not recommend publication at this time

Major comments

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 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.

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 to the reader.

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

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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)?

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 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?

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 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

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seeing the same set of words.

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