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Discussions

Interactive comment on "An Ensemble Observing System Simulation Experiment of Global Ocean Heat Content Variability" *by* Arin D. Nelson et al.

Anonymous Referee #2

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This work uses the Community Climate System Model (CCSM) 3.5 to investigate uncertainty in variability of ocean heat content (OHC) due to observation distribution for the years 1990-2013. The paper breaks down OHC into a mean, seasonal cycle, secular trend, and anomaly. The paper further breaks down the anomaly into high and low frequency anomalies in time with the purpose of quantifying uncertainty in the OHC anomaly and setting a time period cutoff to minimize high frequency noise. The paper finds that the Argo period (2005-2013) coverage leads to lower uncertainty than the pre-Argo period (1990-2004) and that yearly or longer time periods have sufficiently low signal-noise ratio to confidently estimate the OHC anomaly.

The paper is innovative in its approach breaking down the OHC and using a model run to investigate uncertainty in the anomaly, The investigation of uncertainty in the anomaly is thorough. The main conclusion of this exercise (that the Argo period has

lower uncertainty and that OHC estimates for the yearly or longer time period should be used due to higher uncertainty at higher time frequencies) are not groundbreaking. Most estimates of OHC change use yearly or longer time frequencies, and it is well documented that the Argo data coverage is better than previously. That is not to say that there is not merit in these conclusions from rigorous statistical analysis.

The authors do not look at uncertainty in the secular (non-cyclical) change from the model, rather looking at variability around a secular linear trend. The authors note (on page 2, top) 'the variability about the warming trend has yet to be reliably quantified due to the underlying uncertainties'. I am not sure the authors have done quite that here. In their breakdown of OHC, they represent secular change as a trend (deltaQ/deltat). Not much is said about how this is done, but secular change represented by a trend will unavoidably influence the OHC anomaly [if the trend is overestimated, anomaly will be biased negative, understimated, biased positive, etc]. The obvious solution would be to run the model without secular change - meaning without anthropogenic change. Then only cyclical natural (non-human induced) variability would contribute to the OHC anomaly. But the authors note that the model they use is 'run without variability in anthropogenic forcing'. Unless that constant forcing is zero, there will still be a secular change and a contamination of the OHC anomaly. The other conclusion, validating the ISAS13 OHC change 1990-2013 will be discussed in a following paragraph.

I also think the authors could use some better terminology in the paper. This is a mild suggestion only, the paper could be published with the current terminology, maybe with some more prominent definition. The authors use the word 'truth' throughout to mean results from the geographically complete model run as opposed to the 'observed' results from the geographically subsampled and objectively analyzed fields. They also occasionally refer to the 'real' ocean. Not being a modeler, it is somewhat incongruous to hear the model results referred to as 'truth'. Maybe 'model truth' or 'complete model' juxtaposed against 'subsampled', whereas the 'real' ocean would be the 'truth'. The authors define 'natural variability' as 'the variability of the ocean not forced by multidecadal or slower linear climate change and seasonality'. But there is no term for mult-decadal climate change in their OHC equation, except the anomaly term. It would make sense to equate more closely the term 'natural variability' to the OHC anomaly, as the terms are used interchangeably in the paper. The final term which could use amendment is 'ISAS13 observing strategy'. ISAS13 is a set of gridded fields of temperature and salinity. It does not have a set plan for sampling the ocean, an observing strategy. What ISAS13 has is an 'observed data distribution' from which it calculates t and s fields.

Section 5.5 of the paper is not easy to follow, but it provides the results which lead to the conclusion that ISAS13 OHC trend is not significantly contaminated. First, there is little in the way of description of the ISAS13 field calculation. The first time ISAS13 is mentioned there needs to be a reference. This reference should include details of the objective analysis procedure and full details of the OHC calculations - including climatology used and XBT bias correction applied. The authors do not consider in this section any source of uncertainty except the uncertainty in the OHC anomaly in the estimate of the linear trend of OHC change. The authors further note twice in the paper, including the concluding sentence, that varying mapping method (a term which should be defined) will not affect the results of the present work. But previous work has shown that the method used to extrapolate and smooth irregular data does have a large contribution to uncertainty. So, even if mapping method would not affect the results of this work (a speculation in the authors conclusion), it does have an impact on the uncertainty of the trend in OHC change. Likewise XBT bias correction. So saying that OHC anomaly uncertainty alone is low enough not to contaminate the calculated trend is not sufficient. Other sources of uncertainty should also be factored in, or if the OHC anonaly uncertainty is an uncertainty which incorporates these other uncertainties in some way, this should be explained in detail. I would like to see more explanation of the uncertainty inflation factor. Is this a standard practice? Why is it statistically valid here? The authors note that ISAS13 variability is about 2.5 that of the model variability - but the model was run with steady anthropogenic forces which I would expect to

dampen variability (at least secular variability). So the low variability may be due to the unrealistic forcing. Finally, figure 7 raises a number of questions, starting with the units. It is impossible (for me) to see the power of 10 for the y axis in the figure. This, and the caption and accompanying text noting '(low frequency) observed global ocean heat content' make it hard to know what exactly is being shown. Is this Qbar which includes mean OHC, a huge number in comparison with deltaQ/deltat and QsubL, or is this just the latter two terms? The uncertainty of the anomaly is necessarily very small compared to the entire global OHC (as the anomaly is just a fraction of the global OHC), shouldnt just the deltaQ/deltat and QsubL be shown with the QsubL uncertainty? I am also not sure how the jumps in year 1997 has anything to do with the change in observing system. There were no Argo floats in 1997, and only a handful of profiling floats. I would also like to see more explanation for the peak around 2004 and subsequent drop in relation to the change in observing system, since the observing system was already dominated by Argo in 2004. Can the authors please add more explanation to the text and figures for Section 5.5?

The paper is generally well written and clear (with the caveats noted above). There is an extra 'non' in 'non non-uniform' at line 6 on the first page. There is a missing 'a' in 'anomlies on line 5 of page 15.

Figure 1 has incorrect labeling. Blue line should be profiling floats. In addition the counts for other instrument types are too low. A quick look shows there were more than 80,000 XBTs dropped in 1990 for instance, an average of more than 6,000/month.

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