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Ocean Science
Discussions

Interactive comment on "Changes in extreme regional sea surface height due to an abrupt weakening of the Atlantic MOC" *by* S.-E. Brunnabend et al.

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This is a paper with a good idea at its core, but the execution needs some improvement. The idea of examining potential changes in sea level extremes due to changes in the AMOC and the eddy regime of the North Atlantic is interesting and important, but the frustrating thing about this paper is that following a long preamble, the new results are treated rather briefly and superficially, almost as an afterthought. The effects of hosing on the North Atlantic circulation are reasonably well known, and unless the authors can add something new on the mechanisms (which they don't attempt) Figures 1-6 are not very interesting except insofar as they have a bearing on the sea level extremes. In

addition, the results on sea level extremes are hidden behind a somewhat non-intuitive procedure which undermines confidence in the results. The authors are selling their results and their model very short, and I believe with the right effort this could be a classic paper rather than just a standard one. Hence I recommend publication after major revisions.

My suggestion is to dispense with Figures 1-6 and lead with results on the sea level extremes – e.g. 10 year monthly extreme anomaly at each gridpoint with respect to the 10 year climatology for first and last 10 years for and their differences for LR and HR cases (control and perturbation runs). This gives the reader an intuitive and first order impression of the changes in sea level extremes due to resolution, and to changes in the AMOC. A second plot would be for return periods of a chosen representative extreme (e.g. average return period for a 10cm anomaly in the first 10 years). The three or more regions to focus on should then be selected on the basis of these figures rather than the mean sea level change – this is the right way round as you are interested in the regions where the extremes change, not the extremes where the mean sea level changes (although they might end up being the same).

The next figures could be distributions (i.e. histograms) of the monthly maximum in the selected region along with the fitted distributions.

On a procedural point, I am not sure why the authors subtract the area average mean and then detrend the values. It seems to make more sense to just take anomalies with respect to a 10 (or 20) year mean at each gridpoint and look at extremes in the average anomaly for the area, but I am willing to be persuaded on this point. Linear detrending looks problematic to me based on Fig 7b – OK for the 0.1Sv hosing, but maybe not ideal for the 0.5Sv hosing. Again, I am not sure why the analysis is done on anomalies from the control run. To me it makes more sense to do the extremes analysis on the separate runs and then compare perturbed runs with the control.

Having hopefully drawn in and convinced the reader that the authors have some new and interesting results, versions of Figs 7, 8 and 9 could then be presented. Finally, for the explanation of the results, selected plots of e.g. eddy KE, Gulf Stream separation etc. can be shown to back up the hypotheses for the reasons behind the changes in extremes.

Since I am suggesting a structural rewrite of the paper it seems unimportant to list minor comments at this stage, but I would like to draw to the authors' attention that although they place 95% confidence lines on their Figures 8 and 9, they do not use them to indicate whether the changes in return period quoted in the text are significant or not.

I would like to emphasize that there is definitely an excellent paper in the making here and would strongly encourage the authors to submit a new draft taking into account my comments – I would be happy to review a subsequent version.

Interactive comment on Ocean Sci. Discuss., 11, 1213, 2014.