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Earth System Dynamics Discussions

Interactive comment on "Emulating Atlantic overturning strength for low emission scenarios: consequences for sea-level rise along the North American east coast" *by* C. F. Schleussner et al.

Anonymous Referee #1

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SUMMARY:

The manuscript presents probabilistic projections of changes in the Atlantic meridional overturning circulation (AMOC) and associated changes in regional sea level. It takes observationally constrained probabilistic temperature projections from a simple climate model (previously published), and propagates them through a box model of the AMOC calibrated to five different AOGCMs. It also converts the box model's AMOC projections to dynamic regional sea level rise projections, assuming a linear relation between the two derived from AOGCM output.

GENERAL COMMENTS:

The manuscript presents, to my knowledge, the first probabilistic projections of dynamic sea level rise along the North American Atlantic coast associated with AMOC changes, and so represents a useful contribution to the literature.

The manuscript uses admittedly simple models. But it calibrates them to more complex models, and emphasizes projections that can be made with such models without extrapolating far outside the range of the models' calibration (e.g., by considering low emission scenarios, and tuning the AMOC box model structure to optimize its transient dynamics rather than its hysteresis behavior).

The manuscript should review more of the related literature on probabilistic AMOC projections. A weakness is that it does not fully propagate uncertainties, as discussed below. This is particularly relevant since the main contribution of the manuscript is its uncertainty analysis. Fortunately it should not be difficult to propagate more of the relevant uncertainties, and so the manuscript should require relatively minor revision.

SPECIFIC COMMENTS:

The manuscript could use more citations, contrasting its methodology and findings to previous work on probabilistic AMOC projections. It advertises itself as "the first probabilistic assessment of the future AMOC behavior using a calibrated conceptual model and global mean temperature data for the RCP3-PD and RCP4.5 emission scenarios". This wording is dangerous, as it can be interpreted to mean that this manuscript is the first to present probabilistic AMOC projections. There is an existing literature on this subject. Some potentially relevant publications include:

* Knutti et al., Clim. Dyn. 21, 257 (2003) * Hargreaves et al., Clim. Dyn. 23 745 (2004) * Challenor, Hankin, and Marsh, "Towards the probability of rapid climate change", in _Avoiding Dangerous Climate Change_ (2006) * Schlesinger et al., "Assessing the risk of a collapse of the Atlantic thermohaline circulation", in _Avoiding Dangerous Climate Change_ (2006) * Yohe, Schlesinger, and Andronova, Integrated Assessment 6, 57 (2006) * Hargreaves and Annan, Ocean Modelling 11, 174 (2006) * Keller and

McInerney, Clim. Dyn. 30, 321 (2008) * Holden et al., Clim. Dyn. 35, 785 (2009) * Urban and Keller, Tellus A 62, 737 (2010) * Goes et al., JGR-Oceans 115, C12006 (2010) * Challenor, McNeall, and Gattiker, "Assessing the probability of rare climate events", in _The Oxford Handbook of Applied Bayesian Analysis_ (2010)

There is also additional literature that could be reviewed concerning the dynamic sea level effects of AMOC variations, such as Knutti and Stocker, J. Clim. 13, 1997 (2000).

It is not entirely clear to me how the predictive envelopes for the various projections are obtained, given the five different AMOC emulators. Are the projections from the five different emulators superimposed, assuming each is equally likely, and a common predictive envelope constructed for this mixture of models?

There are several points at which the manuscript fails to propagate uncertainties (or at least, fails to mention uncertainties being propagated). These include:

1. Forcing the AMOC box model with MAGICC temperature projections, without first adding noise to the temperature projections to simulate natural variability.

2. Fitting the AMOC box model to GCM output without propagating the parametric uncertainty in the fits.

3. Projecting the AMOC weakening from the box model output, without adding noise to the AMOC projections to simulate natural variability.

4. Projecting dynamic sea level rise from a regression of sea level rise on MOC weakening, without propagating the uncertainty in the regression. (This properly should use the regression prediction interval, not to be confused with the parametric confidence interval of the regression slope coefficient.)

Perhaps point 1 can be omitted from the overall analysis, since the interannual temperature variability is smaller than the projected warming, and it is not uncommon to ignore it. But it is worth at least mentioning in a manuscript advertising a "modular" chain of probabilistic climate projections. Point 2 might also be negligible, since the

AMOC projections are mostly "interpolations" (with forcing comparable to the calibration cases), and so the uncertainty in the fits may not be highly amplified when applied to RCP scenarios.

On the other hand, Figs. 2 and 3 indicate a large amount of noise in the GFDL model, and one could imagine a wider range of fits being possible, at least for that model. For example, the validation plot in Fig. 3 shows a predicted AMOC decline that is compatible with, but probably stronger than, the actual decline visible in the GFDL output. Could other, almost-as-good fits to the GFDL output in Fig. 2, produce weaker declines in Fig. 3 that are also compatible with the validation data?

I am more concerned about point 3, since (as suggested by Figs. 3 and 4) the stochastic variability in AMOC strength can be comparable to the AMOC trend itself. Adding "AMOC noise" to the projections would likely widen the predictive envelopes by a couple of Sv on either side, which could be relevant to the AMOC weakening expected in low-emission scenarios such as RCP3-PD.

I am also concerned about point 4. It is difficult to assess the scatter in the regression relationship, since the data are not plotted, and R-values by themselves are not highly informative. It is possible, in my opinion, that switching from a point trend estimate to a full prediction interval would add significant variance to the sea level projections, due to scatter in the linear relation.

A final question: what is responsible for the oscillatory behavior of the MAGICC temperature projections in Fig. 4b? Such behavior is not apparent in Meinshausen et al. (2009).

Interactive comment on Earth Syst. Dynam. Discuss., 1, 357, 2010.