

Interactive comment on “Equatorial Atlantic interannual variability and its relation to dynamic and thermodynamic processes” by Julien Jouanno et al.

Anonymous Referee #1

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Summary: This paper considers a set of 4 regional ocean-ABL simulations with climatological extratropical ocean conditions and a variety of free atmosphere conditions. Two of the simulations are "reference" REF simulations, one with observed winds (REF) and one with climatological winds (REF-CLIM). The second pair of simulations try to reproduce the mean biased state typical of CMIP-class models, and again use one interannually-variable wind set (BIASED) and one climatological wind set (BIASED-CLIM)(although both based on coupled model fields).

I will comment on my ratings above. I rate the interdisciplinarity as only good, since the model used is not a fully coupled system. I rate the science merits as only good

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because the questions posed are very specific rather than generally applicable. I rate the technical quality as good, because I think the method can be improved upon (see below).

The primary claim of the paper is that the Tropical Atlantic variability in REF is absent in REF-clim, thus a wind-induced variability is responsible in contrast to a recent study by Nnamchi et al. [2015]. They go a bit further to insist that REF & REF-clim are more realistic than BIASED & BIASED-CLIM in terms of the mean state and basic variability. BIASED & BIASED-CLIM seem to be more similar to one another, which suggests that under these winds it is interannual thermodynamic forcing that might dominate the variability.

I find these claims and the experimental design moderately convincing that within the context of this model framework this analysis applies. However, a few important points should be clarified before publication.

1) I do not find these experiments convincing in the more general sense that coupled climate models necessarily behave like the BIASED runs and the real world behaves like the REF runs. This should be more clearly noted in the conclusion—i.e., a different evaluation framework is required in the coupled system is required (e.g., overwriting the coupled wind stress or coupled thermodynamic fluxes with a climatology), although the BIASED results here are suggestive. It certainly seems clear from this result that a biased mean state can affect ocean sensitivity & response.

2) Also, there is one flaw I see in the design of this experiment. The thermodynamic forcing—particularly sensible and latent heat fluxes—depend sensitively on the winds in the boundary layer. No attempt has been made to assess how much of the wind-stress change is actually realized as a thermodynamic forcing—not including the whole model, but just including the flux-windstress relationships—in the cases studied here. Within the mixed layer heat budget, I'd like to see what fraction of the For Nino-For Nina difference is explained this way. The hypothesis here is that a dynamical re-

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sponse to winds is what changes, but maybe the thermodynamic response to winds is what actually changes. For an example of such a diagnosis, see Bates et al. (2012, <http://dx.doi.org/10.1175/JCLI-D-11-00442.1>). To do a much more careful analysis of this problem, a third member of each run set (REF-climthermo) could be run where the thermodynamic flux formulae only use the climatological winds but the stresses still rely on the interannual winds.

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