

Interactive comment on “Partially coupled spin-up of the MPI-ESM: implementation and first results” by M. Thoma et al.

Anonymous Referee #1

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Review of “Partially coupled spin-up of the MPI-ESM: implementation and first results” by Thoma et al.

This manuscript describes an initialization procedure for the MPI-ESM model that uses anomaly wind stress nudging at the ocean surface. The impact of this nudging on the reconstruction of a number of climate indices over the 1980-2013 period is presented.

Main comments:

1. Initializing the ocean-atmosphere system in view of decadal predictions is a very active field and, due to 1) the lack of ocean data over the last decades, 2) model errors and 3) the mixed impact of internal variability and external forcings, is a very difficult exercise. In consequence, as of today, there are as many techniques as there

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are groups attempting this initialization. Hence, having groups document how their perform this exercise is important and should help understand the relative merits of these different approaches and make progress as a community.

2. In that context, the present manuscript does not provide enough elements to come to useful conclusions about the merit of the specific method proposed. Given the wide diversity of approaches (not listed in the introduction), the fact that 1) no sensitivity to the technique is explored and 2) no in depth discussion vs. the other approaches is proposed is a clear weakness. One then wonders if he/she should use this technique in his/her own model, which somewhat limits the interest of the manuscript.

3. The analysis is most of the time superficial, providing little physical insight of why some aspects of the climate and its variability (the long list of indices) are reconstructed and why some are not.

4. For instance, it is a classical result (for ocean modelers) that the wind stress constrains the SST much more in the tropics than outside. The resulting teleconnections most likely explain a large fraction of the reconstruction elsewhere and more analysis would have helped on this aspect. Conversely, the lack of buoyancy forcing at higher latitudes (that can be achieved via SST and/or SSS nudging) is a limitation and not discussed here (see for instance <http://link.springer.com/article/10.1007/s00382-014-2184-7>).

5. The very short record used (1980-2013) is also a limitation for decadal time scales as the robustness of statistics will be limited (and leads the authors to use a number of vague terms such as “better”, “reduces slightly”, “shows a clear improvement”, “larger”, “highly significant correlation... of 0.35”, “enormous”, “much less”, “good”, “huge”,...) . Another aspect not discussed is the quality of the wind stress reanalyses used. Wind stress reanalyses usually do not agree (see [http://journals.ametsoc.org/doi/abs/10.1175/1520-0442\(2004\)017%3C2526:EWSAFE%3E2.0.CO;2](http://journals.ametsoc.org/doi/abs/10.1175/1520-0442(2004)017%3C2526:EWSAFE%3E2.0.CO;2) for instance). The authors write that

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NCEPcsfr “does better” (it is unclear what the metric is here) than ERAI but is it because it better compensates model errors ?

6. As the authors note themselves, the high frequency (1.2 h) of the nudging is at odds with the coupling frequency (1 day) which is bound to have a large impact on the vertical stability of the upper ocean and clearly complicates the analysis of the impact of the nudging.

7. Last but not least, the skill of the hindcasts performed with such an initialization is not explored or compared to that using another technique.

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