Ocean Sci. Discuss.,
doi:10.5194/os-2016-27-RC1, 2016

Ocean Science
Discussions

Interactive comment on "The implications of initial model drift for decadal climate predictability using EC-Earth" *by* Andreas Sterl

Anonymous Referee #1

Received and published: 20 July 2016

This paper presents an analysis of decadal prediction experiments conducted with the EC-Earth climate model which focuses on the drift in these full-field-initialized ensemble simulations. I found much of the analysis to be interesting and novel because it is rare to see an explicit focus on the drift in initialized climate prediction studies. In the abstract, the author proposes to "describe" the drift and then "relate it to the lack of ... predictability" in the North Atlantic. The paper succeeds well enough in describing the drift, with a nice set of figures and adequate writing (although there are numerous instances where presentation clarity could be improved, as noted below). However, I don't think the manuscript actually succeeds in shedding much light on the low predictability seen in the EC-Earth prediction system.

Part of the problem may be a lack of specificity throughout the manuscript about what is meant by "the lack of decadal predictability" (abstract). The EC-Earth v2.3 system

analyzed herein exhibits non-trivial skill at predicting SPG heat content at decadal lead times (Hazeleger et al. 2013). This author focuses on the evidently low predictability of the large scale circulation and subpolar heat fluxes (although no quantitative skill scores are given). The analysis of drift (up through section 3.5) does not automatically inform the lack of predictability, and the logic used to draw conclusions in sections 4 and 5 seems flawed (see my specific reactions below). The authors fail to cite and discuss their results in the context of published studies in which significant decadal prediction skill is seen in the North Atlantic in full-field initialized ensembles despite the presence of large drift. By the end, it seems evident that the real focus of the paper is skill at predicting air-sea heat flux, which should probably be clearly stated up front.

The "second reason for the low predictability" (abstract) presented here (that the deep convection required to communicate ocean heat content signals to the atmosphere is inherently unpredictable) is rather hand-wavy and not convincingly supported by the analysis. Figure 6 is an analysis of intra-ensemble spread in heat (and salt) content. By itself, this doesn't shed much light on heat flux skill scores, which could depend more on (presumably large) inter-ensemble (across start dates) heat content differences (ie, if there were large heat content differences in the SPG from before the mid-1990s to after the mid-1990s, as observations clearly show, then even average (and/or poorly predicted) winter mixing should tap that heat content signal and be apparent to some degree in heat flux). Jumping straight to a conclusion about Figure 7 from Figure 6 (P10.L28-34) ignores this, and hence obfuscates the interpretation of how this decadal prediction system is working (or, isn't working).

The conclusion that "the drift in the atmosphere is not caused by the drift in the ocean" (P14.L13), may well be true, but it hasn't been demonstrated. This statement is apparently based on the different timescales in Figs. 7 and 9, but these represent very different regional averages – what exactly can be concluded by this comparison? Certainly nothing so strong as "an ocean signal that far exceeds the internal variability of the model is not able to impact the atmosphere." On the contrary, I'd be surprised if the dramatic cooling in T2m off of the Grand Banks (Fig. 8, lower panel) were not related (indeed, driven by) the drift in the position of the model North Atlantic Current (Fig. 4). In short, strong and surprisingly general conclusions are drawn in this manuscript that do not really follow from what is shown.

A serious and major revision would be required, in my opinion, to transform this into a publication-worthy study with clear, strong, and adequately-supported conclusions.

Specific comments:

* Abstract: Awkward first sentence.

* P1.L8: "the" instead of "de"

* P1.L24: "Labrador Sea" seems too restrictive. Karspeck et al. (2015) found enhanced predictability associated with initialization in a broad subpolar gyre region, and Van Oldenborgh et al. (2012) report skill in the "northern North Atlantic."

* P2.L2: The analysis of CMIP3 models with and without volcanic aerosol forcing in Van Oldenborgh et al. (2012) *suggests* that initialized decadal predictions would be less skillful without foreknowledge of volcanoes, but doesn't actually demonstrate that.

* P2.L17: "were" instead of "where"

* Fig. 1: Why not extend the time axis to show the full plume of 2005-initialized ensemble? Thick colored lines (ensemble means) are almost impossible to see.

* P4.L15: This (unconventional) integration from the western boundary results in negative (positive) streamfunction in the subtropical (subpolar) gyres (see Fig. 3). Probably worth mentioning, if not changing the streamfunction sign in order to be consistent with common usage.

* P4.L23: The "increase by more than 1 Sv during the first two years" seen in Figure 1 hides a substantial initial drop of $\sim$2 Sv, does it not? As noted in the caption, the 12mrm smoothing results in curves that do not start from ORAS4, but clearly there

must a sharp reduction in AMOC during the first 6 months of the predictions, followed by the increase. Can the authors comment on what the drift looks like without 12mrm-smoothing?

* P4.L28: What exactly is meant by "within observational constraints"?

* Fig. 2: Why are only two ensembles shown in bottom panel?

* P5.L9: To my eye, the SPG still flows into the Labrador Sea in PD.

* P5.L10: Define "GS"

* Fig. 3: Please specify the time intervals used for computing mean AMOC and BSF, for both ORAS4 and PD.

* P6.L1: Suggest "absent" instead of "lost"

* Fig. 4: I am confused by the averaging over the first two months for BSF (upper right panel). The rationale is that "the strength of the SPG declines rapidly in the first year." However, Fig. 2 (top panel) shows that SPG strength is always higher than ORAS4 at the start of the 12mrm plumes, indicating that SPG strength rapidly increases during the first 6 months. See also above related comment for AMOC at P4.L23 – why is annual mean OK for AMOC in Fig. 4 top panel given the rapid decline in AMOC strength in the first 6 months? Please clarify.

* P6.L9-P9.L7: This discussion regarding the importance of "maintaining the current structure in and around the LS", and the implication that eddy-resolving resolution is crucial for that, is too vague and speculative, in my opinion. It seems to me that maintaining convection in the LS is key to maintaining robust gyre circulation there, and apparently this model cannot maintain convection there (P9.L11). Plenty of other O(1-degree) coupled models are able to.

* P9.L9: "is" instead of "comprises of" ?

* P10.L8: "is" instead of "occurs" ?

* Fig. 6: I'm a bit confused about what exactly is being plotted here. Is SPG the region from Fig. 3? Does PCC show the pattern correlation over the SPG region at each time and depth? How is RMSD normalized? Is the "drift" substracted computed just from the 1995 ensemble(s) or is it the drift according to Eq. 2?

* P10.L20: I suggest coming up with distinct names for the boxes shown in Fig. 3 so that there is no confusion about what is meant by "SPG". As it stands, "SPG" means different things for different variables.

* P10.L21: I find it hard to see what's happening on seasonal timescales in Fig. 7.

* P10.L25: What is meant by "distinguished ... signal"?

* P10.L27: I'm confused. Presumably, Fig. 7 is showing net downward heat flux, with negative climatological values indicating that the atmosphere tends to cool the ocean in the SPG. Then, why would a drift towards more negative fluxes (Fig. 7 lower panel) indicate "less heat is extracted from the ocean"?

* P10.L29: explanation for the low predictability of what, exactly?

* Fig. 8: Why don't you actually show the difference from observed climatology from the PD simulation for T2m and SLP, to support the argument that the model drift represents a return to model's own climatology?

* Fig. 9: Anomalies from what climatology? I find this an unconvincing plot, because I see a lot of variation with time at all latitudes (and particularly in the 40-60N range). I do not see how this shows that "the drift takes place within the first year".

* P14.L8: Again, it's not clear to me that the drift represents a reduction in the amount of heating of the atmosphere (see comment on P10.L27 above)

* P14.L8-L20: I find the logic difficult to follow, and hence unconvincing, in this discussion. When I look at Fig. 8 (bottom panel), I see a large cooling signal in the vicinity of Grand Banks and throughout the SPG that is almost certainly related to the ocean

model drift (ie, the loss of a realistic North Atlantic Current pathway and the overall weakening of the overturning and gyre circulations that transport heat into the SPG). How exactly does the author come to the conclusion that "an ocean signal that far exceeds the internal variability of the model is not able to impact the atmosphere"?

* P14.L23: "casting doubt" instead "causing doubts"

* P14.L25: I don't think this conclusion is obvious at all, particularly since numerous other decadal prediction studies have demonstrated convincing skill in the North Atlantic (Robson et al. 2012, doi:10.1029/2012GL053370; Yeager et al. 2012, doi:10.1175/JCLI-D-11-00595.1; Hermanson et al. 2014, doi:10.1002/2014GL060420; Yeager et al. 2015, doi:10.1002/2015GL065364). Perhaps these results indeed cast doubts on the feasibility of using EC-Earth for predictions, but such a sweeping conclusion is wholly unjustified.

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