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

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Dear Reviewer,

Thank you very much for taking the time to thoroughly review my paper. Please find my answers to your comments in red below. My conclusion is that I do not have the time for the extra work needed to overcome the concerns raised by you and the reviewer. Therefore, and I will not try to submit a revised version of the paper.

With kind regards,

Andreas Sterl

This paper is on the drifts in the EC-Earth model hindcasts after full field initialisation with ERA-Interim atmosphere and ORAS4 ocean. The drifts are interesting to see although they are not presented in enough detail or with focussed enough diagnostics to really learn a lot about the causes of drift. Where these are discussed the paper appears very speculative and the results shown do not really justify the conclusions. I therefore think that the claim of the paper to have explained why decadal hindcasts appear to fail with this model (and by implication to the same deficiencies in other models) cannot really be justified. I cannot therefore recommend publication in the full journal The presentation is rather superficial level at times.

- Page 3 L2 full field initialisation the ocean state is not simply "constructed from Observations" The initial state is a reconstruction of the T and S fields that is based on observations, while in anomaly initialization only the anomalies are based on observations.
- L6-7 the drift occurring in full-field assimilation is exactly the same as in seasonal forecasting approaches which have accepted forecasting skill If the drift is to obscure forecast skill the system must become very non-linear After a few months the atmosphere is in equilibrium with the upper ocean and decoupled from the deep ocean. The latter can only be reached through intermittent deep convection. This is a highly non-linear process.
- L16 One might expect drift to at least have a seasonally dependent component (which is stated later top of page 4) I did not investigate whether the drift has a seasonal component, but it might be interesting to do so. - On top of page 4 I do not state that such a dependence exists. I just state that eq. (3) results in a $\hat{T}_{\text{signal}}$ that by construction does not contain an annual cycle, and thus must be

compared to anomalies. - More to seasonality: It might also be interesting to investigate the dependence of the drift on start month. All runs investigated in the paper were initialized on November, 1st. Would the drift look differently for runs initialized on, say, May, 1st?

- Section 3.3 Using the AMOC to assess drift relative to an observation based reanalysis truth is rather hazardous I would say. There is not a lot of evidence that the AMOC is not really robustly reproducable in these reanalysis products yet (as Karspeck et al note) so using it as a forecast target is insecure. Thank you for pointing this out. My main point in the discussion of the AMOC is, however, that its development is similar in all runs, irrespective of start data ("common drift"), and towards the model's own climatology.

- Page 5 The low natural variability in EC-Earth cannot be a surprise given this is such a low resolution model that has not really be tuned against low frequency variability signals. I do not say that I am surprised. Indeed this whole section 3.3 seems to end with the conclusion that a higher resolution version of the model might be better for the AMOC but there is no evidence presented for this apart from noting another model has found this. Indeed it would be better to have hi-res runs with the same model. Unfortunately, I had no access to any.

- Section 3.4 discussed vertical structure of drift in AMOC and Lab sea. The lack of predictability as well as the drift in the AMOC is then suggested to be related to the rapid decorrelation and drift of surface heat fluxes and upper ocean properties in the Lab sea in different model members. But the hypothesis is not really proved. I do not say that the drift is related to the rapid decorrelation of heat flux and upper ocean properties. It is the limited predictability that is. In addition there is no discussion of the way the Lab sea / SPG water gets out and at what depths it propagates down to influence the AMOC. I would argue this is an aspect which will be greatly affected by model resolution. Presumably, yes. As said above, I

had no access to such runs.

- Section 3.5 is very brief and discusses the obvious result that the atmosphere is un predictable on short timescales. Here I disagree. I show that the drift in the atmosphere occurs over a much shorter period than the drift in the ocean and thus that the atmosphere is not predictable on *long* time scales.

- The discussion section 4 then presents some speculative Lagrangian argument for the pathways of air from a colder SPG towards Europe. But none of the diagnostics are lagrangian and no attempt is made at budgeting the movement of heat content anomalies. The section concludes that this argument does not agree with the model results anyway and that therefore "obviously the mechanism does not work" I agree that a thorough Lagrangian heat budget analysis would improve the paper.