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Interactive comment on "Randomly correcting model errors in the ARPEGE-Climate v6.1 component of CNRM-CM: applications for seasonal forecasts" *by* Lauriane Batté and Michel Déqué

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Reply to interactive comment by anonymous referee #3 by Lauriane Batté

We wish to thank the reviewer for his/her constructive comments on our manuscript.

1) Reply to general comments:

"I believe there is a general problem with the use of the "initial" when τ is as long as 30 days. With such a weak nudging this term can not be said to represent initial tendency errors but rather long term secondary adjustments (that luckily seem to have some

positive impact). This is of cause because, on a monthly time scale, initial forcing in terms of e.g. potential vorticicy will show up far away via Rossby wave dispersion. As an example consider the right column of Figure 2: These corrections could very well be due to "real" initial errors in the tropics. It is therefore suggested not to use the expression "initial" tendency errors. One could, e.g., call it model drift error."

This is a very interesting comment. The use of "initial" in our tendency error estimations originates from the previous version of our method, which used a much stronger nudging. You are right that with a 30 day nudging strength, the differences will be more representative of longer term errors. We accepted and included the formulation you suggested (model drift error).

2) Reply to specific comments:

We include the minor corrections you suggested to our manuscript. More details are included below where appropriate.

"Page 4, line 4Âă: I presume you mean "not to perturb the divergent component" instead of "not to perturb the rotational component" (since vorticity represents the rotational part)."

Actually, line 3 of page 4 should read ÂńÂăstreamfunctionÂăÂż instead of vorticity! Sorry for the confusion and thank you for pointing this out. We corrected accordingly.

"Page 5 ff: Probably not only the magnitude but also the shape of the spectra are quite dependent on τ . A short discussion on this would be relevant."

You're right. We included a few lines on this in section 3.1.

"Page 8, Section 4.2: It would be relevant to show - or at least discuss - the bias in the initial nudged simulations as well. Ideally the mean error of these runs should be small. But with the large value of τ one would suspect that this is not the case."

A detailed discussion on the impact of the strength of the nudging on the quality of the nudged re-forecast runs is somewhat beyond the scope of this manuscript, in our opinion. Based on past work with the method using a stronger nudging, the nudged simulations have (by construction) a much closer mean climate to that of the reanalysis dataset used as a reference, however since this reanalysis is not based on the same atmospheric model as our forecasting system, the model error estimates are not truly representative of errors in forecast mode. Using settings from the previous version of the method described in Batté and Déqué 2012, we found some adverse effects on ENSO prediction skill with our new coupled system. This motivates the use of much "looser" nudging to let the model drift away from reference data; however it is true that the bias (and skill estimates, although with only one member) are degraded with this setting. Note however that the bias and skill of the nudged run is (in most cases) significantly improved with respect to our REF experiment. We have yet to test an intermediate solution as a trade-off between both nudging strengths (that discussed in this paper and in the 2012 GRL), to see the impact on forecast quality.

We included a sentence relative to the bias of the nudged reforecast in section 4.1.

"Page 11, line 32: "... not capture its interannual variability". One would guess that it could also be large if the model has a bias. Any bias could be subtracted before calculating RMSE. This would probably give considerably smaller RMSE's. Page 13, lines 17-21: Also here it could be relevant to eliminate the impact of bias."

This is done in our computation of the RMSE. I clarified this where the RMSE score is discussed.

"Page 14, line 3: You could provide a quantitative estimate of the uncertainties in the correlations!"

Based on bootstrapping over the years of the re-forecast period, the 95% significance intervals (with 10000 draws) for the NAO correlation are [0.119, 0.641] for REF, [0.009, 0.656] for SMM and [0.181, 0.797] for S5D. The interval is wider in the case of SMM and slightly shifted towards higher values in the case of S5D, however given the broad

intervals in this case it seems difficult to draw any firm conclusion.

"Page 14, lines 22-23: Why is there no SMM in Table 2 (and 3)?"

I have fixed this in the revised manuscript. As you will see, results are very similar between both versions S5D and SMM tested.

"Page 15, Section 4.5.3: I think this section can be removed. It does not add much to the findings already described."

We feel examining weather regime frequency prediction skill (or lack thereof) is the next logical step to assessing the impact of the perturbations on North Atlantic large-scale variability. Although results are very limited, this is why we chose to include these in the paper. We think the paragraph should be kept in the manuscript.

3) Changes to the manuscript

Changes to the manuscript can be tracked in the supplement to this comment, with red crossed text indicating suppressions and underlined blue text indicating additions with respect to the original submission.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/gmd-2015-270/gmd-2015-270-AC3-supplement.pdf

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