Initial replies to major comments and suggestions by John Scinocca and two anonymous reviewers

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General

We sincerely thank all three reviewers for their thoughtful comments and suggestions which we will be happy take into account in a revised version of this article provided we are given the opportunity to do so. We are confident that their very relevant points would help us to improve the paper substantially. Several points raised by the reviewers are similar. For example, all reviewers insist on the need to more clearly relate our work to previous studies and to clarify methodological aspects (and we agree on this point), and all request more specific information on the chosen nudging timescale. However, for clarity, we will address the points separately below for each reviewer.

We will focus on the reviewers' major points here. This set of replies is intended as a contribution to the open discussion of this article, not as a set of "final" replies. We suggest to address the reviewers' minor points in our detailed replies to their comments, along with detailed replies to their main points building on the initial replies provided here, and a potential revised version of the article. We will sometimes use the acronym "ERBC" for "empirical run-time bias correction" in the following.

Main comments by Reviewer 1 (John Scinocca)

Main comment #1: Dependence of results on value of tau in the classical method. The reviewer suggests to identify the optimal value of the nudging time constant τ for our configuration, and to discuss the chosen configuration in the light of these elements.

Reply : This is indeed an important point, and it is also raised in similar terms by Reviewer 2. We have indeed carried out simulations with varying nudging time constants and found that both in a classic (non-iterative) approach and in the case of iterations (point of the paper here), the selected time constant $\tau = 1$ d is the best choice in our model. We will mention this, but we intend to discuss this in more detail in upcoming work (in preparation, to be submitted soon) where we will compare systematically the "classical" approach, a revised implementation of the CABCOR approach in LMDZ (see one of the following comments), the iterative approach described here, and a state-dependent variant of the "classical" approach. In the present work, we think that we should therefore only briefly mention these results and, in response to similar comments by Reviewers 2 and 3, discuss why iterating the "classical" approach is not identical to simply reducing the nudging time constant.

Main comment #2: Out-of-sample validation. The reviewer makes the point that the climate system was not stationary during the period considered (1981 – 2020), raising concerns whether it is appropriate to "tune" the model for the period 1981-2000 and evaluate the effect of the bias corrections for the period 2001-2020. He suggests to include at least a discussion of this point in the present work.

Reply : This is an excellent point. Indeed the climate system has been undergoing rapid change, as we all know. Therefore, as the reviewer states, there can be some confusion between the effects of out-of-sample-testing and climate change. One could indeed devise experiments such as calculating the bias-correction terms for all pair years between 1981 and 2020 (i.e., 1982, 1984, 1986,...2020), and testing the effect of these correction for all odd years (i.e., 1981, 1983, 1985,...2019). Or one could calculate the bias-correction terms for the latter half of the entire period (2001-2020) and evaluate the effect of the bias corrections for

the first half of the period (1981-2000), and compare with the current setup. However, the main motivation for our use of various ERBC approaches is to eventually use it in climate change simulations, and as the reviewer states, we do have arguments to use it for that kind of applications. In that sense, testing the effect of the bias corrections in a changing climate is not necessarily a drawback; it is, to some degree, a prerequisite for the intended use of the ERBC approach. However, potential interference of the climate change signal with "pure" out-of-sample effects cannot be excluded, and we will mention this point.

Main comment #3: The relationship of ERBC and Model tuning. The reviewer makes the point that other models with less of a temperature bias than LMDZ might not suffer the same degradation in convective behaviour and so benefit from T runtime bias correction, and that we should clearly state this point where appropriate.

Reply : This is a fair point. More generally, the parameters of the ERBC approach (most importantly the nudging time constant τ), the set of corrected variables, the temporal and spatial resolution, the place of the ERBC within the GCM's time stepping scheme, and possibly other choices, are all to some degree model-dependent. And it is clear that, if we hadn't encountered problems with temperature ERBC in LMDZ, we would probably be using these. We are happy to clearly state this. However, there is still value in conceptually separating atmospheric circulation structures from other variables such as temperature and humidity in the sense that the effect of misplaced circulation features (for example, a misplaced storm track) often cannot be corrected a posteriori (see Maraun et al., Nature Climate Change 2017, https://doi.org/10.1038/nclimate3418, a posteriori bias corrections of "physical" variables such as temperature and precipitation are quite usual.

Main comment #4: Relevance to Scinocca and Kharin (2024). The reviewer objected against a problematic statement about "unsatisfying results" obtained with LMDZ using the CABCOR method.

Reply : The relevant sentence in the submitted version of the article was clumsy at the very least, and we apologize for possible misunderstandings this may have caused. After submitting the current version of the article, we realized that our initial implementation of the method was not exactly equivalent to the original method described by Scinocca and Kharin (2024). After correcting the implementation, the results obtained using the CABCOR method are substantially improved, and a strong sea-level pressure bias induced by the alternative initial implementation was strongly reduced. New analysis shows that in LMDZ and without temperature ERBC (and this caveat is certainly important), the iterative method described here yields results that are broadly equivalent (but overall slightly better) than the corrected CABCOR method. These results will be presented in more detail in the upcoming article in preparation that we have already mentioned. We therefore propose to mention the CABCOR method here, state that it yields broadly comparable results in terms of bias reduction, and refer to work in progress for a detailed comparison.

Main comments by anonymous Reviewer 2

Main comment #1: Insufficient articulation of the motivation for the present study. The reviewer request a more explicit discussion of prior work in the introduction to help readers better understand the scientific context, value, and fundamental advances underlying the exploration presented in this study. In addition, they state that it is important to clearly distinguish the target of the bias-correction problem addressed in this study—namely, the correction of long-term systematic errors—from the weather-scale bias correction typically associated with classical nudging.

Reply : We agree that a clearer motivation of this study, placing the objectives more explicitly within the framework of existing literature, is required. We thank the reviewer for suggestion of additional work to cite (in the following point they made). We will also more clearly distinguish this work, aiming at developing bias-correction methods for climate change applications, from another cluster of published literature aiming at bias corrections in a weather and seasonal prediction context.

Main comment #2: More detailed discussion of the nudging technique. The reviewer requests us to provide details about technical choices made, and to discuss how the choice of the nudging timescale τ can influence our main conclusions, e.g. concerning the optimum number of iterations.

Reply : This comment is similar to points made by the other reviewers. We agree that we need to explicitly mention this aspect in a revised version. As stated before, we have carried out simulations with different nudging constants and will present results in more detail in ongoing work that aims at comparing different ERBC methods. Again, the main of the present article is to present the iterative method as such, for which the chosen time constant is the most appropriate in our setting. Therefore, as stated before, we think that it would be appropriate to mention the fact that various time constants have been tested, that $\tau =1$ d is overall the most convincing choice in our case, and refer to work in progress for a more detailed discussion.

Main comments by anonymous Reviewer 3

Main comment #1: Request for more precise presentation of methods and metrics. Most of the metrics presented and parts of the methodologies are loosely defined textually, leaving significant room for uncertainty in interpretation.

Reply : We will try to improve the presentation of the metrics and methods by making them more precise, and provide mathematical definitions where appropriate. The reviewer has made several concrete suggestions and requests in detailed comments. We will follow these suggestions.

Main comment #2: Nudging timescale. The reviewer requests us to discuss the effect of the nudging timescale and the reasons for our choice.

Reply : This point has also been raised in similar terms by the two other reviewers. As stated before, we are currently finalizing work that compares the different ERBC approaches ("classical", CABCOR, iterative, and an implementation of conditional ERBC) that also comprises a detailed evaluation of the effects of nudging time constants in the "classical" and iterative approach with the LMDZ model. In addition, the reviewers have indicated references to relevant published studies that, together with our experience, provide a rather complete picture of the question, including the finding that optimal nudging timescales can be strongly model- and application-dependent. We will be happy to include a clear discussion of these aspects, but suggest to refer to our work in preparation for detailed results concerning the LMDZ model.

Main comment #3: Repeat analysis with other models or model versions. The reviewer suggests, as an outlook, to carry out similar studies with other models to obtain more robust results.

Reply : This is very true, and we will mention this need in the conclusions of the article, as a consequence of model-dependency of such results.

Conclusion

For the first author of this study it is a rather rare and very pleasant experience to receive feedback from three reviewers that is highly consistent. We are confident that we will be able to address these comments to the reviewers' and editor's satisfaction, following the preliminary responses indicated above.