Responses to Reviews of manuscript "A near-global eddy-resolving OGCM for climate studies" (gmd-2016-17) by X. Zhang, P. R. Oke, M. Feng, M. A. Chamberlain, J. A. Church, D. Monselesan, C. Sun, R. J. Matear, A. Schiller and R. Fiedler.
(The reviewer's comments are in back and our responses are in blue)

Referee Comment by anonymous referee #1:

Summary of key results
The latest version of the Ocean Forecasting Australia Model (OFAM3), a near-global high-resolution (0.1^o) ocean model is described, based on GFDL MOM v4p1d. Its grid extends from 75^{o}S to 75^{o}N. There is no sea ice, but the model includes the WOMBAT biogeochemical model. The model is forced by the JRA55 climatological surface fields. It is first run for twenty years with a repeat of the 1979 surface climatology to generate a three-dimensional restoration flux, based on that needed to maintain the model fields close to climatology in the first two decades. This flux is then applied to the ocean model to reduce drift during the subsequent integration with interannual forcing up to 2015, but allows the climate signal implied in the forcing to be reproduced.

General style
The paper is overall quite well written and structured, but it is occasionally evident that the text is for the most part written by someone whose first language is not English, so the manuscript would be benefit from further proofreading to check grammar and usage.

Thank you for your positive comments and a good suggestion. We have asked our native-speaking co-authors to double check the grammar and usage in the manuscript and we hope it reads well now.

Specific corrections
P2, L30: Any list of recent eddy-resolving ocean configurations should include a reference to the latest UK 1/12_ NEMO: Marzocchi et al, 2015: Journal of Marine Systems, 142.

We cited one NEMO based paper (Sérazin et al. 2015) and added the Marzocchi et al (2015) citation.

P4, L19: This is a common, but incorrect, use of the term "Boussinesq approximation". The latter, as (for example!) defined in Gill, 1982, refers to the neglect of density differences except where these imply pressure difference. This is not equivalent to ensuring constant volume.

Thank you for clarifying our statement, the paragraph has now been modified to
"The model adopts Boussinesq Approximation (Greatbatch 1994). The net global freshwater flux (i.e., sum of evaporation, precipitation and river run-offs) is balanced at each model time step, thus the global mean sea level is kept constant."

Reference:
Greatbatch, R. J., A note on the representation of steric sea level in models that conserve volume rather than mass, J. Geophys. Res., 99, 12767–12771, 1994.

General comments
The reasons for some of the choices made in creating the OFAM model configuration are puzzling to me, and need to be explained clearly in the paper. The development of a global ocean-only model whose grid reaches 75^{o}S (most of the Antarctic coastline) and has biology but no ice model, and which excludes most of the Arctic, leads to potentially severe limitations on the general applicability of the model to climate studies, and needs to be clearly justified. Such a model may well be useful for understanding recent climatic variability in the mid latitudes and tropics, providing that enough confidence exists in the forcing dataset used (which is not the case before the satellite era), but cannot inform about the interactions between the high and mid latitudes, and certainly can say nothing about

the climatically highly important polar regions. For this reason I believe it will have limited interest for the climate science community.

We agree with the reviewer that our mode domain and lack of a sea-ice model prevents this study from being used for interactions between the high and mid latitudes. However, we would argue our study is useful for many other research topics of climate science, as climate science is not necessarily limited to studies on the global scope. There is a need for climate impact studies at regional or national scales. The global warming hiatus over the recent decades, as one example of "hot" climate research topics, has been popularly explained by changes in the tropical and subtropical oceans. Another example is how the western boundary currents respond to climate change – a question that requires a model realistically represents the boundary currents and eddies there.

The immediate application of the model experiment is to understand the ocean's response to decadal climate variability and climate change. Based on the responses to this manuscript and recent seminar/conference presentations, there is plenty of interests from climate science community in our results. Please also see our General Response #1. A similar near-global model domain (75^{o}S to 75^{o}N) is used by the OGCM for the Earth Simulator (OFES) developed by JAMSTEC. The OFES model simulation is still one of the most popular eddy-resolving modelling to do climate-related studies (e.g., Masumoto et al. was cited for 330 times). We do think it is appropriate to recommend that "the climate modelling community should consider adopting the approach described in this study as **an efficient and short-term solution**" to use high-resolution models (1/10^o and finer) to investigate the climate variability and change on tropical and subtropical oceans.

Would it not have been more logically consistent to develop the model from the start as a coupled system, including a fully global ocean? A non-global ocean model cannot directly be reframed as a free-running coupled climate model.

In Australia, there is ongoing development of a climate model with 1/4^o ocean component. The development of a coupled climate model with 1/10^o ocean component is a long-term goal, subject to substantial increase of computation resources in the future. We started with this near-global OGCM and gained useful experiences in running it for climate applications. We have plan to set up a true global OGCM with sea ice model in coming years.

The Discussion and Summary Section mentions the intention of re-running the model with the merged out-put of an ensemble of CMIP5 coupled models, instead of the JRA55 forcing dataset. This is, of course, perfectly feasible, but would have the severe disadvantage of omitting the interannual and decadal climate variability, which would be averaged out in the ensemble mean.

The atmospheric forcing for future run is generated by combing high-frequencies (cut-off period of 7 years) from JRA-55 reanalysis product with the long-term climate change from the CMIP5 ensemble. The climate variability forcing from JRA-55 reanalysis has been intentionally included exactly for the purpose of including climate variability as pointed out by the reviewer. We are currently finishing a draft manuscript on the future run, including details about how we prepared atmospheric forcing, but it's a separate paper and out of the scope of this one.

Please also see our General Response #2 and Fig. A for more information.

The method of applying the fluxes equivalent to the initial drift of the model is evidently effective in this case in reducing drift on decadal timescales, and is an interesting way to address the thorny question of spinup in climate models.

Thank you for your positive and insightful remarks about our method.

This has of course possible drawbacks, however: specifically, an implicit assumption that the drift is due to model deficiencies, rather than to errors in the applied surface fields or in the way the lateral boundary conditions are applied. In particular, the need for strong restoration of temperature and salinity at the northern boundary has serious implications for the performance of an eddy resolving

ocean model, since there is an abrupt mismatch between the resolutions of the model grid and of the forcing data: the consequences for the representation of boundary currents, which are vital for the exchange between the Arctic and the rest of the oceans, are not clear, and should be discussed.
The reviewer is right about the implicit assumption about the model drift and we have now emphasized in the text that this study is not suitable for studying the exchange between the Arctic and the rest of the oceans due to model limitations. We have incorporated the following information into the revised manuscript to explicitly acknowledge the limitations of using an artificial northern boundary in the North Atlantic.

"To ameliorate the impacts of northern boundary, we designed a 5^{o} buffer zone which has linearly varying relaxation time scale, increasing from 30 days at the boundary to 365 days in the interior. The "buffer zone" technique has been commonly adopted in many existing basin or near-global experiments, e.g., Smith et al. (2000) used a 3^{o} buffer zone at 72.6^{o}N in their $1/10^{o}$ North Atlantic basin model; Masumoto et al. also used a 3^{o} buffer zone in their $1/10^{o}$ near-global model domain from 75^{o}S to 75^{o}N. The buffer zone method comprises the use of the model simulation in the North Atlantic for circulation processes that influenced by the exchanges across this boundary like the North Atlantic Deep Water (NADW) formation."

I have difficulty with the recommendation at the end of the paper, namely that the climate modelling community "**can consider adopting the approach described in this study as an efficient short-term solution, at the same time also develop more sophisticated methods to address this important problem of model drift**". This method was indeed often used in the coupled climate models used at the end of the last century, and was called "flux correction", though this was mainly (but not exclusively) restricted to surface fluxes. By around 2000, though, climate models had improved to a state where they had realistic enough surface fields that they were able to be integrated without flux correction (for example HadCM3, Gordon et al, 2000). Admittedly, interior drifts remain an issue in coupled models, but work is ongoing to reduce this; for instance the use of isopycnal-type ocean models (e.g. Megann et al, J.Clim 2010, Dunne et al J.Clim 2011), that reduce spurious numerical mixing. Do the authors consider that interior drift is a serious enough problem to merit such an invasive engineering fix? This is not dis-cussed in the paper. In my opinion, the proposed use of flux correction in standalone climate models would be a serious step backwards.

We agree with the reviewer that significant progress has been made in coupled climate models to address climate drift. It would be ideal to tackle the interior drift by more scientific techniques other than our "engineering solution". The artificial drift is a still a challenge in the "state-of-the-art" climate models as shown by Sen Gupta (2013) for CMIP5 models. Their study showed drift is still a problem especially in the deep ocean where the drift is comparable to the forced response. Please also see our General Response #2. The drift problem is even more challenging for eddy-rich ($1/10^{o}$ and finer) simulations where we lack the computational resources to do multi-century simulations.

Clearly, we do not claim our model and approach is a "perfect" solution. On the contrary, our approach should be considered as an efficient and practical approach in the short term. Modelling groups need to develop better models with improved model physics, which do not suffer from climate drifting in long integration (> 50 years).

In any case, the question needs to be posed of the robustness of the tuning of the correction fluxes: would they still be appropriate for models used for future climate projections?

The same climatological restoring (correction) is repeated every year so that it doesn't contribute any forcing to the ocean on interannual and longer time scales. The climate change signals, such as ocean warming associated with anthropogenic climate change, can still be allowed to penetrate to the deep ocean. Such repeated climatological correction has been applied by others over long integration period (e.g., Ding et al. 2015), and our methodology is comparable to their approach. Whether the correction is still appropriate for future climate projections is worth further investigation, but it's beyond the scope of this paper.

For publication in GMD, the intended application of the model described should be clear, and I am not convinced that is the case here: the introduction section of the paper does not make the case strongly enough for the utility of the OFAM model in climate studies.

We modified introduction to clarify the intended application of this model. Please see our General Response # 1 in the beginning. The model is most suited for looking at climate variability in boundary currents and how their changes affect the meso-scale eddies they generate.

In particular, I do not really understand the inclusion of the word "forecasting" in the name of the model, since it is incapable of advancing beyond the limit of current forcing datasets. I would certainly like to see a clearer exposition of what the model is actually useful for.

Obviously our current model experiment is not about "forecasting", but the underlying model is adapted from the Ocean Forecasting Australian Model, version 3 (OFAM3, Oke et al. 2013). The model was primarily developed for short-range operational reanalysis and forecasting. "Forecasting" is used only when we referred to the name of the model (OFAM3) in the Introduction Section. To be consistent with previous literature we would like to retain the name OFAM3 for the model.

Recommendation

In summary, although the paper is overall well structured and well written, the lack of clear justification for the design of the model, nor of any clear, plausible statement of its intended application, means I cannot recommend publication in GMD of the manuscript in its current form.

We appreciate your positive remarks, and addressing your comments point by point has helped us improve our manuscript significantly. In particular, we revised the Introduction section significantly to state clearly the design and application of our model, and the Discussion and Summary Section to discuss implications of this work. We hope you will find our revised manuscript is now suitable for publication in GMD.