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Interactive comment on "Downscaling the climate change for oceans around Australia" *by* M. A. Chamberlain et al.

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Review of "Downscaling the climate change for oceans around Australia" by M.A. Chamberlain, C. Sun, R.J. Matear, M. Feng and S.J. Phipps

The paper presents a framework for dynamically downscaling climate change projections from a coarse resolution AOGCM onto an ocean mesoscale resolving model. The paper outlines a method of downscaling onto the high resolution model, evaluates the role of air-sea feedbacks, and presents evidence that downscaling projections onto a high resolution model can result in some differences in the near surface environment.

The topic of the paper is certainly of interest, but I have several difficulties with appreciating its value:

i) Is the focus on the method of downscaling or on the effects of downscaling on the Australian region? If the authors feel they are presenting a novel downscaling method, then the methods need to be more clearly justified and much better explained. The explanation of the methodology is incomplete, and spread throughout the paper in various sections. As it stands now, a reader would find it very hard to evaluate the merits of the methodology or employ it.

RESPONSE: The focus of the paper is to present the method, with some basic results to demonstrate that the higher resolution downscaling model in general agrees with climate model projections, but there are some differences in the ocean response that are robust to slightly different implementations of the downscaling method. There has been rewriting in the abstract, introduction and conclusions to state this point more clearly. Within the Experiment Setup section, there is now a Methodology subsection that gives an overview of the approach before describing the details.

ii) If the focus is on the effects of the downscaling then the model simulations employed need to be better evaluated (see comments below).

RESPONSE: Further analyses of the downscaled projection is intended to be done in other work and some have been published (Sun et al., 2012; Cheung et al., 2012). Some results are presented here to demonstrate the results are sensible, there is consistency between different experiments, and results are stable.

iii) I think your experimental design is awkwardly described and hard to justify. CTRL, RELX, FREE and STRS should all be initiated from the same Spinup state and integrated for the same period of time. As it stands now different experiments are started from different control states and run for different lengths of time. I cannot see much value in the FREE and STRS experiments since they have been integrated for such a short period of time and are initiated from year 3 of RELX rather than a control state. This awkward methodology makes it very hard for the reader to see value in the results.

RESPONSE: We have modified the text to improve the presentation. We have also clarified that the focus of this downscaling study is the upper ocean. The high resolution model is expensive to run, and therefore unsuitable for investigating slow processes in the deep ocean. In the introduction of the downscaling we emphasise that the RELX ODM simulation represents what we consider is the more realistic way of downscaling the AOGCM climate projection. However, this methodology is a simplification of a coupled system and if we had unlimited resources we would do a coupled simulation. We included two additional experiments - FREE and STRS. The FREE experiment assesses the heat and freshwater feedback while the STRS experiment assesses the windstress feedback. These two experiments were not meant to provide new climate change projections but rather to assess the impact of ocean-atmosphere feedback parameterisation. We chose to start both FREE and STRS from year 3 of the RELX model to avoid the initial shock that appears in the RELX experiment when the initial state was perturbed with the climate change state. As demonstrated with the RELX experiment (Fig 11), by the third year of the STRS and FREE experiment a stable pattern emerges and we compare this to the RELX experiment to assess the robustness of the RELX climate change projection. The STRS and FREE experiments show a similar pattern of climate change to the RELX experiment revealing that resolving mesoscale features does alter the climate change projection in consistent way independent of the ocean-atmosphere feedback, however, the magnitude of the difference is sensitive.

So from my point of view the paper doesn't do a very good job of justifying the downscaling methodology and the experimental design makes it difficult to evaluate the effects of the downscaling. I find it difficult to justify the publication of this study.

RESPONSE: Thank you for your useful input and we hope that our responses to your questions clarify the focus of the paper and the choice of experiments. We emphasise that our approach is an approximation for a coupled atmosphere-eddy resolving ocean because of present computation limitations. Our simulations do show that resolving mesoscale features alters climate change from the AOGCM, and the difference in ODM projections produces a pattern that appears independent of the ocean-atmosphere

feedback parameterisation. Until such a time when we can do the fully coupled system with eddy resolution in the ocean, our approach provides one way to explore mesoscale features in climate projections and their effects on the marine environment.

Specifics

Pg 426 Line 4: A small point, but all global climate models resolve boundary currents. They may not do it well, but they are present nonetheless.

RESPONSE: Text modified to, "only poorly, resolve... boundary currents."

Pg 426 Line 5: Perhaps note here for clarity that the global ocean model is coarse resolution except in the Australian region, where a mesoscale resolving model is nested.

RESPONSE: Text modified, "... that resolves these [mesoscale] features in the Australian region, with coarse resolution far from here."

Pg 426 Line 13: Differences have been shown, but significance has not. Are the differences significant relative to internal variability or drift in the ODM?

RESPONSE: The differences are significant in the sense the differences are larger than variability of the simulations. "Significant" is swapped for "consistent." Sentences that follow clarify that our confidence that mesoscale resolution systematically alters the pattern of difference (from the consistency in the pattern from different experiments) while the ocean-atmosphere feedback modifies the amplitude of the difference.

Pg 426 Line 19: For the framework to be attractive to others it needs to be more clearly outlined and compared to others. As presented it is difficult to determine if the methodology is sound.

RESPONSE: The presentation of the framework has been clarified throughout the manuscript, including an outline here in the abstract.

Pg 426 Line 25: please clarify 'horizontal' rather than "spatial" resolution, or discuss both horizontal and vertical resolution.

RESPONSE: Text modified as suggested.

Pg. 426, line 28: mesoscale resolving models are integrated for multi-decadal time-scales. Perhaps modify text to centurial and millennial time-scales.

RESPONSE: Added centurial time-scales, millennial scale experiments are not used directly in this work.

Pg. 427, line 26: Specifics of the downscaling methods used by other 'limited-region' ODM studies should be discussed. How is your methodology for applying the climate change forcing to the ODM different? Is the only difference that your high resolution ODM is nested within a coarse resolution ocean model? Or is the surface boundary information applied differently?

RESPONSE: Some more detail has been added to the description other marine down-scaling studies. Also, text is added contrasting the setup of our model with this previous work, namely that there are no open boundaries in our model. However, this discussion is done in the overview of the "experiment setup" in order to keep the details of our model in one place. Importantly, our approach of a global model enables better representation of key Australia boundary currents that is difficult to achieve with a regional model. Second, our approach makes the system an atmospheric forced problem rather than an open boundary condition problem.

Pg 427, line 27: I would move the paragraph starting at line 22 on pg 428 to here

RESPONSE: This paragraph has been moved into the overview of the "experiment setup" in this revision.

Pg 428, line 24: again, coarse models have boundary currents, they are just not accurately resolved.

RESPONSE: Text modified to "better resolve."

Pg 428, line 27: "By utilizing a global- scale ODM the passage of information from the

AOGCM to the ODM changes from how we handled the open boundaries to how we initialise and force the ODM."

I don't really appreciate how much has 'changed' by using a global ODM with a high resolution nest. The high resolution nest still has a boundary with the coarse outer model. A discussion of the boundary between your high resolution nest and the outer model should be included. Are the surface forcings applied to both the outer model and the nest, or the nest only?

RESPONSE: We have clarified our explanation of the grid configuration of the ODM. It has a near-global domain with varying resolutions in different parts of the world ocean: 1/10 degree in the oceans around Australia (90E-180E, 72S-16N). Outside this domain, the horizontal resolution decreases to 0.9 degrees across the Pacific and Indian basins and to 2 degrees in the Atlantic Ocean. In the revision, in Overview of the Experiment Design, we have now clarified that "since our ODM is global without nesting, our climate change signals from the AOGCM are contained within the initial conditions and forcing fields." When describing the ODM grid, we have changed the sentence to "the model grid has variable horizontal spacing which is eddy-resolving around Australia."

Pg 428, line 28: "This requires some consideration on how to incorporate the feedback of the ocean state on the atmosphere forcing fields in an ocean-only simulation.

Many ocean only models try to deal with the effects of atmosphere-ocean feedbacks on the surface forcing used to drive the ocean model. In section 2 of your paper you describe how you have chosen to handle these feedbacks, but this is a topic that has undergone much prior research. Some discussion as to how others have handled these feedbacks is required.

RESPONSE: In this revision, in the Feedback section (2.5), we have discussed the why and how ocean feedback to air-sea fluxes has been applied in prior work, namely, to approximate real feedback (Frankignoul et al. 1998) and to avoid thermohaline instabilities (e.g. Weaver et al. 1993).

Figure 2: The caption states “Model grids”, but the grids are not shown, rather the bathymetry is shown (missing units on the figure). I would also like to see an explicit figure of the high-resolution region. Also, if OFFAM is a global model than where is the Arctic Ocean?

RESPONSE: This figure has now been replaced. The domain in the new figure is just about the high resolution region in the ODM: the same longitudinal extent (90E-180E), and slightly smaller latitudinal extent (55S to 5S in the new figure, and the high resolution domain in the model extends from 76S to 16N). The new figure is added to show the output from the AOGCM and the ODM which helps the discussion in the text. Correct, there is no Arctic in this downscaling model, which we now clarify in the text and we modify the text to "near-global" ocean.

Pg. 430, line 15: I can appreciate that OFAM has been used previously by many to study Australian ocean region. However, a reader of this paper requires some evidence of the models skill in this area. Some discussion of the models strengths and weaknesses in simulating the Australian ocean is required and perhaps a Figure or two demonstrating the skill (e.g. surface KE and EKE, T/S comparisons with observations).

RESPONSE: New figures are added to this section to show samples of the circulation, SST and SSS from the AOGCM and ODM for the present climate. These demonstrate the very different character of the circulation between the models, the effect of mesoscale variability on SST as seen in the ODM and observations, as well as the substantial bias in SSS from the AOGCM.

Pg. 430, line 25: If the model was only spun-up for 16yrs then why does the 1990-2010 forcing need to be looped? Table 1 states that the CTRL was equilibrated for 26 yrs?

RESPONSE: The initial spinup used reanalysis products from 1991 to 2004 (14 years) and we used an ocean state 2 years into the 2nd loop. 1995 was chosen as the target year for the initial condition and for certain components of the forcing field construction.

The text had originally stated "1990s and 2000s" and this has been rewritten to be clearer. Also, as now clarified in the overview of the experiment setup, this spinup is distinct from the control experiment described. While the spinup used reanalysis products, including interannual variability, from climate observed over the last 2 decades, the control experiment is run with a repeat-year climatology in a method more similar to the setup of projection experiments with restoring of surface T and S in the spinup replaced by diagnosed heat and freshwater fluxes.

Table 1: The table information is difficult to understand. For example “End of year 3, RELX” – I assume implies the model was initiated from year 3 of the RELX. But why not start FREE and STRS experiments from the same initial state as RELX (e.g the CTRL simulation)? It seems odd that FREE has the heat and freshwater feedbacks of RELX, which are then just turned off – and run for another 3 years. This makes it very difficult to explicitly determine the effects of the feedbacks, since they were used in the initial conditions. The experimental design here seems odd.

RESPONSE: The table has been modified, in order to clarify that the control experiment is distinct from the spinup experiment. Also, some notes and cross references are added to clarify the table content. The reasoning for using a common branch/start point for the experiments, is explained in the introduction to the results section, "we find that most of the shock from the addition of the climate anomaly to the ODM ocean state is gone after the first 2 years," (NB. we focus here on the upper ocean). After this, we find the broad scale features are stable and are independent of which year is shown. We start STRS and FREE from this non-shock state but let the system run long enough for its own distinct pattern of change to emerge.

Pg. 431, line 5: ‘quasi-stable solution’. At this point in the paper there is no evidence that the simulations are stable or near any sort of equilibrium. Given that some of you simulations are run for only 3 or 7 years, I doubt they are stable or near any sort of equilibrium. I actually have never seen such short simulations discussed in a scientific study.

RESPONSE: Text modified to emphasise that the upper ocean is the focus of the spin up and stabilisation. In the overview of the experiment design, we now make the point that while the ODM is certainly useful in resolving upper ocean processes, at this time they are not suitable tools for exploring deep water and long time scale processes.

Pg 431, line 10: Are you certain that the T/S anomalies are purely the result of anthropogenic forcing or is drift in the MK3.5 simulation T/S fields also a part of the anomalies? Are the T/S anomalies applied everywhere (3-D) in OFAM? How big are the applied anomalies compared to the OFAM T/S? I would like to see a plot of surface T and S (or surface density) in OFAM, MK3.5 cntrl, and the applied 2060 anomalies.

RESPONSE: Mk3.5 has been spun up so that the T/S are stable and the trends from the Mk3.5 experiment is due to anthropogenic forcing. Text is added to state this point, though these comments are relevant to forcing fields as well as initial conditions, so the text is placed in the 'global climate model' section. Anomalies are certainly global 3-D fields and this is clarified in the text. Surface plots of T and S anomalies applied are now shown in following sections where SST and SSS results are discussed. Where these figures are discussed, explicit statement is now made that they are the anomalies added to the initial condition.

Pg. 431, line 15: The reader needs to see the forcings applied and how big they are relative to the control forcings. Please provide a figure of the surface forcings in control and the anomalies.

RESPONSE: A figure is added showing the sum of the forcings from the present climatology, the correction fluxes and the climate change anomalies for each forcing heat, freshwater and momentum.

Section 2.3: If changing the surface fluxes are the essence of the the downscaling methodology, then the methodology you are employing needs some justification for others to employ it in their work. Section 2.3 tells us what you did, but not why. It also fails to show us the size of the relative terms in Equations 1-3. The use of the correction

terms (HC and FC) should be discussed more thoroughly. I also don't understand why the need for a separate diurnal variability term – is this not present in the control forcing?

RESPONSE: Some overview is now added to the start of this section to describe the "why" of the construction of the forcing fields, namely to reduce the interannual variability in the experiments and reduce the effects of model bias in the projections. Text has been added to explain that the correction fluxes are the fluxes diagnosed in the ODM spin up while restoring to observed fields. Text has been added to describe how the diurnal variability was lost in the construction of the present day climatology (due to using monthly climatology instead of daily climatology from present day climate), hence the extra step to add it back. The relative sizes of the flux terms are shown in a new figure.

Section 2.4: It is difficult for the reader to discern the experimental design when reading the paper – the methodology is discussed in various different sections (e.g Sections 2 and 3). Table 1 should be improved to clarify the differences between the spinup, cntrl and forcing experiments. It needs to clearly explain the integration timescales, initial conditions, forcings etc., and one section in the paper should clearly outline the complete experimental design.

RESPONSE: This "Control experiment" subsection is deleted and the content is now incorporated into an overview of all experiments at the start of the set up section, which outline the complete experimental design as suggested. Table 1 is expanded and is associated with this overview subsection.

Section 2.5, line 1: 'All simulations' except for the AOGCM coarse model.

RESPONSE: Text is corrected.

Section 2.5.1: Heat exchanges are a function of more than just SST. For example winds and various coefficients are involved in the process.

RESPONSE: In Section 2.5.1, the sentence is changed to "strong function of temperature, in addition to other properties such as wind speed."

You should show equations for how the Heat and FW Flux feedbacks are calculated and justify this equation. As it stands now, the reader could not replicate or understand the reasons for your methods. As I understand it, you restore the SSS and SST to 'guide fields' that include the AOGCM T/S anomalies. I think this connection between restoring terms and feedbacks maybe novel, but am uncertain if it is a sound method. You need to justify it to the reader better. Sometimes in the paper you interchange the use of 'feedback' and 'restoring' (eg. Pg 434, line 13) and this is unconventional to me. Show us how the restoring is calculated (equation) and show how large the restoring surface fluxes are in comparison to the AOGCM surface flux anomalies. A 30 day restoring timescale can produce quite strong fluxes.

RESPONSE: Equations that describe the application of restoring are added to the text. The value for the feedback coefficients are stated here, which compare favourably to 'observed' values. The reasoning for restoring is incorporated with discussion of previous use of feedback in ocean models. Yes we do use restoring and feedback synonymously for heat and freshwater, and this is now clarified at the start of 2.5.1

Figure 4: Please show the GCM-ODM difference for the control states in the upper right panel. It appears that in the control state the SST signature of the EAC boundary current transport is more pronounced (i.e. a larger southward extent) than in the ODM. How well is the EAC resolved in the ODM? It would be interesting to see the modelled SST and SSS compared to observations.

RESPONSE: The top row of this figure has now been moved to Fig. 3 in the revised manuscript, in the introduction of the models in the experiment setup section. SST and SSS fields from the AOGCM and ODM are now presented along with observations, which shows the ODM has a much more realistic SSS field. The EAC is well resolved in the ODM, but only resolved by a couple of grid points longitudinally in the GCM.

Detailed comparison of the GCM and ODM performance on resolving the EAC and its southward extension has been the subject of a couple of studies, such as Sun et al. (2012) and Matear et al. (2012). We now refer to those literature in the paper.

Pg. 437, Line 2; Figure 5: It is not stated which year the annual averages are taken from. If one assumes it is averaged over the last year of the simulations, than given that the simulations are all integrated for different time periods the anomalies in Figure 5 seem meaningless to me. It is also not clear that the anomalies are significant relative to the internal or interannual variability of the models.

RESPONSE: Text is added to clarify that it is the 3rd year from the branch point of the climate projections that is shown from each of the experiment. So the integration time shown for each experiment is actually consistent. The strong similarity of the difference patterns in the different experiments is evidence that these patterns are greater that the model variability.

Pg. 437, line 5: "The amplitude of the difference in climate change for SST, as measured by the standard deviation over the region shown in Fig. 5 ..." Please clarify the meaning of this statement. Perhaps you determined the spatial standard deviation of the SST patterns for the middle column of Figure 5 and found that FREE was more spatially variable. I don't understand how that relates to the "the amplitude of the difference in climate change for SST". Please clarify.

RESPONSE: Yes, this is the correct interpretation. This "difference in climate change" has always been an awkward term to describe. Hence, there is an introduction to the terms "projected climate change" and "difference in climate change" in the opening paragraphs of the results section, and also a direct reference to the centre column in the preceding sentence. The amplitude is introduced simply to enable quantification and comparison of the differences from the experiments. The average of any of these differences is about zero which isn't useful, hence the standard deviations are used.

Pg. 437, ln 12: "The retention of the warmer and cooler regions in the three different

ODM experiments suggests that the structure of the difference in climate change for SST is largely independent of feedback at the surface. " While the spatial patterns of change are similar between the experiments – the large difference in the amplitude of the SST anomalies between the FREE and RELX experiments in Fig.5 suggests to me that the feedbacks or "restoring terms" are very important. Please clarify.

RESPONSE: Our discussion had been focusing on the mechanism driving the difference in the ocean's response to climate, and the argument was that since the spatial distribution/pattern is consistent, then this mechanism of driving the difference is robust. But yes, feedback is evidently playing an important role, also modifying the impact which is now added in the discussion. We consider the projection with restoring/feedback as our best estimate of the future climate, and the other experiments give some indication of the sensitivity of the projection to the ocean feedback.

Section 3.2: the above 2 comments also apply to this section.

RESPONSE: The above responses also apply to this section.

Section 3.2: "However, even with this large freshwater flux feedback the consistency in pattern of the difference in climate change SSSs suggests the SSS difference pattern is not very sensitive to air-sea fluxes." Perhaps you mean to say that it is not very sensitive to your feedback-restoring methods. SSS patterns are certainly sensitive to air-sea fluxes.

RESPONSE: This discussion had been referring to the spatial pattern, which is certainly the same for each experiment/feedback. However, the amplitude of the difference is quite sensitive. Text is modified, as for the SST section. Also noted is that the climate anomaly in the initial condition has a larger impact on the projected state for salinity.

Figure 7: The far right shows the AOGCM and ODM stratifications for the 1990s. Please show the Levitus evaluation mentioned in the text. Also given that the simu-

lations are run for a short time period (e.g. 3 years) the near surface isopycnals are likely to still be under sever adjustment. Are the patterns shown in Figure 7 the result of the T/S anomalies applied, or the surface flux anomalies, or simply the result of model drift at depth?

RESPONSE: The stratification from Levitus fields has been added to the figure. We find that most of the shock to the ODM has relaxed in the first 2 years, when applying the climate anomaly to the ODM ocean state. This is found to be true for all properties shown, including stratification. The broad patterns shown in this figure do not change when different years are used.

Figures 5-8: It is also hard to evaluate the Figures since the models have all been run for different lengths of time and they are initiated from different control states.

RESPONSE: We have now clarified that these figures are from model output at the 3rd year from the common branch point, which is the 6th year from the projected initial condition.

Figure 8: Please evaluate the ODM versus AOGCM transports relative to observations. Is the ODM doing a good job or not? Furthermore, as noted in the caption Figure 8 shows an average velocity over the upper 250m. This is not a transport.

RESPONSE: In the experiment setup section, there are now a couple of figures focused on demonstrating the "skill" of the ODM, and here we cite that Schiller et al 2009 found that transport in Australia's boundary currents were found to be consistent with the observations that are available. The text is modified to indicate that average velocity has been used as a proxy for transport in this discussion.

Pg 440, line 7: "Also demonstrated is the high spatial variability of transport in the ODM indicating the need for averages over long time or large spatial domains to quantify results." I agree completely. So why does the paper evaluate a 1 year average from a 3 year model run compared to a 1 year average from a 7 year model run?

RESPONSE: Considering the ODM is expensive to run, our longest integration is the experiment with realistic feedback to provide a high-resolution projection which can be used for further analysis. Other shorter experiments are to demonstrate that the differences found are robust and by focusing on the broad features or large spatial domains, we can still make comparisons that are significant. The consistency of broad patterns is backed up with good values for correlation coefficients.

Figure 9: It seems odd to me that model drift is not discussed until this late in the paper, and it is done so in a fairly abstract manner. Why not simply show a 2D plot of the linear trends salinity or temp? After reading the text describing Figure 9 several times I am still unclear if the methods to diagnose drift are robust. It certainly is not a common way of evaluating the role of model drift. This analysis also only pertains to the RELX simulation. It is difficult for me to believe that the SST and SSS in the shorter STRS and FREE simulations are insignificant.

RESPONSE: A 2-D plot is added which clearly shows that shock from the climate change anomaly added to the initial condition dissipates after the first 2 to 3 years. Text is also added to the beginning of the Results discussing stability in the ODM, with a cross reference to this section. The Taylor diagram is kept, since the there is no spatial information in the 2-D plot. The Taylor diagram demonstrates that the pattern of the SST fields also converges.

Figure 10: The EAC transports show large interannual variability in the simulations. Given that most of the figures compare 1 year averages taken at different years, it is difficult to determine the relative roles of internal variability versus the forcing response.

RESPONSE: This plot in particular shows considerable interannual variability since it is showing the results from a single location. Local transport is especially sensitive to mesoscale eddy activity. However, the experiments with longer integrations are long enough to show there are no trends in the transport here, which is the objective of the figure. In other figures, discussion and analysis focuses on broad features and

consistency of these demonstrate that change at these scales are significant, even when there is variability at finer scales.

References:

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