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Geoscientific Model Development Discussions

Interactive comment on "The weather@home regional climate modelling project for Australia and New Zealand" *by* Mitchell T. Black et al.

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Dear Dr Williams and readers,

Please find attached our revised manuscript with changes from the original version highlighted in red (see revised_manuscript.zip uploaded as part of the author reply to RC1). We found the reviewers' comments to be very helpful and have responded to these comments below

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Reviewer 2: Andrew Ciavarella

Reviewer's summary: This well written paper introduces a unique and valuable resource for event attribution over the Australia and New Zealand region, building on

the success of equivalent systems focussing on different regions. With regards reproducibility I have found some omissions in the model and experimental description that can be easily addressed. The system description is otherwise clear and well motivated. A decent attempt to explore model uncertainty is made through the construction of 10 estimates of the counterfactual boundary conditions while the limitations of the system are also discussed. The basic validation conducted is probably sufficient given that the studies that are performed with such a system generally require bespoke validation and are in fact already in print [Herring et al., 2015]. Altogether I found no issues that I consider major and so I would recommend this work for publication.

1. Reviewer's comment (specific): [p2] Use of phrase "internal ... climate forcings" While it is not unreasonable to refer to major modes of variability as forcing a regional climate akin to an external forcings it would be better to use a phrase like "internal climate variability".

Authors' response: Manuscript updated accordingly

2. Reviewer's comment (specific): [p2.7, also p2.20,23] "chaotic natural variability" is referring to variability generated internal to the climate system while in attribution we normally retain the phrase "natural variability" to refer to externally forced variability (i.e. by solar and volcanic forcing). "chaotic internal variability" would be better and is probably adequately distinguished from the major modes of internally generated variability in the context of this sentence. Later [p2.30] "natural forcing" is used in the normal sense so there is clear room for confusion.

Authors' response: Manuscript updated accordingly

3. Reviewer's comment (specific): Page 2.19-24 emphasises the importance of several major modes of internal variability in addition to ENSO (SAM, position of storm tracks, blocking) which are important in the Australia / New Zealand region but only ENSO is addressed in the remainder of the paper. I am not asking that additional validation be done for these other factors but could the authors comment on the relative importance

of ENSO with respect to these others? For e.g. some reference to the literature to give an idea of the share of variance explained in inter-seasonal or monthly regional means of the two variables presented? It appears Risbey et al., 2009 Figs. 15 & 16 (already cited by the authors) may contain enough to go on.

Authors' response: As Reviewer 1 correctly identified, the original body of text confused drivers of internal climate variability (e.g., ENSO) from manifestations of climate variability (e.g., blocking). The text was revised to address this point. As the text now stands, three drivers of climate variability are identified – ENSO, Southern Annular Mode and the Indian Ocean Dipole. While each of these three modes of variability have an important influence on Australian temperature and precipitation extremes (e.g., Risbey et al. 2009, Min et al. 2013), ENSO was examined in this manuscript as it is the mode of variability that has been examined in a number of event attribution studies to date (e.g., King et al. 2013; Lewis and Karoly 2013; Christidis et al. 2013) and there are a number of planned studies wanting to specifically investigate the role of ENSO (e.g., Black and Karoly, 2016; Karoly et al. 2016). For brevity, the analysis for IOD and SAM was not presented in the current manuscript but will feature in an upcoming publication. Analysis of blocking within the weather@home ANZ model is provided in the recent publication by Grose et al. (2015).

4. Reviewer's comment (specific): [p2.28, also p9.22, Fig. 13 caption] Use of "observed" climate in reference to historical climate. In a modelling context we would avoid using "observed" to refer to experiments with anthropogenic and natural forcings present as these are simply not observations but simulations whose climate is intended to reproduce an observed climate, but which may not do so. I advise substituting this with "anthropogenically forced", "historical" or similar. Elsewhere [p5.3] "historical ... climate scenario" is used.

Authors' response: The manuscript has been updated to replace 'observed' with 'historical'

5. Reviewer's comment (specific): [p4.33] Further initial condition perturbations are applied by a range of start conditions with difference large scale circulation and soil moisture patterns. No details or references are given for the selection criteria or pattern generation. Please elaborate. Are the patterns physical / taken from a control?

Authors' response: For the weather@home ANZ experiment, each model simulation is initialised using a restart file created in a control simulation for the previous year. For each experiment, 100 unique restart files are created (each with a different atmospheric state and soil moisture pattern/profile).

For example, consider the weather@home experiment for the year 2015. For this experiment, the model is run 100 times for the preceding year (2014) and the resulting restart files from these experiments are used to initialise the 2015 simulations. Therefore, there are essentially 100 'groups' of simulations created for the year 2015, with members of each 'group' containing the same restart file. For members within each 'group', slightly different initial condition perturbations are applied to the three-dimensional potential profile of the restart file (as described within the manuscript).

The manuscript has revised to clarify this (see page 5).

6. Reviewer's comment (specific): [p5.4] Lower boundary conditions are taken from a daily analysis product (OSTIA) while previous versions of the weather@home experimental setup [Massey at al., 2015, Mote et al., 2015] used interpolation from a monthly observational dataset (HadISST1). Was there a good reason for this new choice? For instance was it felt that the daily analysis provides more faithful sub-monthly variability than interpolation from monthly data?

Authors' response: All recent weather@home experiments have transitioned to using sea surface temperatures and sea ice extent from the OSTIA dataset (e.g., see Schaller et al. 2016, Mitchell et al. 2016). As the reviewer correctly identified, these daily fields provide a more faithful estimate of sub-monthly variability than is achieved from interpolation of monthly data.

7. Reviewer's comment (specific): [5.10] Halocarbon prescription: experimental progenitor [Massey et al. 2015] prescribe a single halocarbon value designed to give the radiative forcing corresponding to the presence of all (6 AR4 recommended) represented species. Is the same manner of prescription used in these experiments or are the AR5 individual species concentrations separately prescribed?

Authors' response: As per Massey et al. (2015), a single halocarbon value is used to give the radiative forcing corresponding to the presence of all represented species. The manuscript has been updated to include this information (see page 5, paragraph 2).

8. Reviewer's comment (specific): [5.11] GHG concentrations and aerosol emissions. Could you be explicit about the concentrations that are prescribed post-2005? Specifically, is one of the RCP scenarios followed (for e.g. from http://www.pik-potsdam.de/âĹijmmalte/rcps/index.htm)?

Authors' response: Post-2005, the greenhouse gas concentrations and aerosol emissions follow the RCP 8.5 scenario. The manuscript has been updated to include this information (see page 5, paragraph 2).

9. Reviewer's comment (specific): Section 2 does not mention if land use changes are prescribed. I can see (from Massey et al., 2015, section 2.2.4) that fractions of surface types are specified. Is this specification fixed or time dependent? Are the prescribed fraction of the natural simulations representative of preindustrial conditions? Section 2 I could not gather from this or Massey et al., 2015 what land surface scheme the models use.

Authors' response: The model uses the MOSES 1.0 land surface scheme with fixed surface type. There is not change in surface type between the historical and counterfactual climate scenarios. The manuscript has been updated to include this information (see page 4, paragraph 1).

10. Reviewer's comment (specific): Section 2 Spin down. Is there a spin down period allowed prior to creation of the experimental initial conditions or to the analysis period? Has any continued drift in climate variables such as soil moisture been seen over the 29 year historical experiment?

Authors' response: As indicated above, each model year is initialised from a restart file output from a model simulation for the previous year. Therefore, there is a 12-month spin-down period allowed. Continuous integration of the model over an extended period (1985–2014) has not revealed any continued drift in soil moisture.

11. Reviewer's comment (specific): [p6.9] After taking 29 year means "any differences between the obs. and model output may be interpreted as model deficiencies". This is not strictly true as, for example, even the means of output from two members of the same model will still be subject to "standard error" which will decrease as 1/SQRT(n) for n data points. In comparing the difference of two such means the errors will also add in quadrature. Nevertheless I would estimate that the discussed biases depicted in the figures 2 – 4 easily stand out from this level of noise. Also I acknowledge that p6.15 says that this can be regarded as "an indication of model bias".

Authors' response: As the reviewer indicates, the biases depicted in Figures 2–4 easily stand out from sampling noise and can be regarded as an indication of model bias. The purpose of these figures is to provide a general overview of model performance; we feel that these images are able to portray this required information in a simple and adequate manner.

12. Reviewer's comment (specific): [p7.16] A comparison of time series variances, power spectra or quantile plots would provide a more objective measure of agreement than simply eye-balling that the obs. time series sits mostly inside the envelope of the models, especially given that the objective measure provided (correlation coefficients) will not allow an assessment of overall amplitude of the series. However given the intended brevity of the validation and later closer focus on daily data we can probably

make do with this.

Authors' response: We thank the author for these suggestions. Given the brevity of the paper we too feel that the level of information currently presented is appropriate.

13. Reviewer's comment (specific): [p7.18] Precisely what series are the correlation coefficients between? Is it obs. and model median? This should go into caption to Fig. 5 also.

Authors' response: The correlation coefficients are calculated between the observation and model medians. The manuscript has been updated to include this, including the caption of Figure 5.

14. Reviewer's comment (specific): [Figures 5 - 7] Can you confirm that the p-values are for a one-sided test?

Authors' response: The p-values are for a two-sided test. The figure captions have been updated to include this information.

15. Reviewer's comment (specific): [p7.21] ENSO as driver of "natural climate variability" would better be "internal climate variability", again avoiding confusion with solar and volcanically forced variability.

Authors' response: Manuscript updated accordingly.

16. Reviewer's comment (specific): [p9.30] What year or period are the "pre-industrial" GHG, ozone & aerosol levels taken from?

Authors' response: The manuscript has been updated to include this information.

17. Reviewer's comment (specific): [p10.1] "cannot be known" could better be phrased as "cannot be observed", which is indisputable. We may dispute whether the counterfactual world is knowable.

Authors' response: Manuscript updated accordingly.

18. Reviewer's comment (specific): [p11.7-10] Use of an atmosphere only model is here portrayed as a limitation but the atmosphere only approach simply allows us to frame a different event attribution question than that provided by coupled experiments. Namely, we ask for the likelihood of an event subject to the lower boundary forcing provided by the precise phases of the various modes of oceanic (and cryospheric) variability at the time of the event, which is not possible with a coupled model.

Authors' response: The reviewer has raised an important point – the atmosphere only model is not necessarily a limitation. The text has been revised accordingly (see page 11, paragraph 3).

19. Reviewer's comment (technical corrections): [p6.12] Could insert the word "daily" to be completely clear (sentence could be interpreted as maximum of seasonal average over 29 years, 75 members.). Ditto the caption to Figure 2.

Authors' response: Manuscript updated accordingly.

20. Reviewer's comment (technical corrections): [p7.14] "individual years" would better be phrased as "specific years".

Authors' response: Manuscript updated accordingly.

21. Reviewer's comment (technical corrections): [p9.26] Insert word "lower" before "boundary conditions" to distinguish from lateral.

Authors' response: Manuscript updated accordingly.

22. Reviewer's comment (technical corrections): [p9.32] "boundary conditions common to both" is incorrect here, should be "forcings common to both".

Authors' response: Manuscript updated accordingly.

23. Reviewer's comment (technical corrections): [p11.7] Unnecessary comma after "weather@home"

Authors' response: Manuscript updated accordingly.

24. Reviewer's comment (technical corrections): [Fig S1] "summertime" should be "wintertime" if genuinely June – August.

Authors' response: Manuscript updated accordingly.

25. Reviewer's comment (technical corrections): [p14.19 and elsewhere] Massey et al. "2014" should be "2015".

Authors' response: Manuscript updated accordingly.

References

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