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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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General comments

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

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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.

Specific comments

[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”.

[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.

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.

[p2.28, also p9.22, Fig. 13 caption] Use of “observed” climate in reference to historical

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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.

[p4.33] Further initial condition perturbations are applied by selection of 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?

[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?

[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?

[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/∼mmalte/rcps/index.htm)?

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?

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Section 2 I could not gather from this or Massey et al., 2015 what land surface scheme the models use.

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?

[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”.

[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.

[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.

[Figures 5 - 7] Can you confirm that the p-values are for a one-sided test?

[p7.21] ENSO as driver of “natural climate variability” would better be “internal climate variability”, again avoiding confusion with solar and volcanically forced variability.

[p9.30] What year or period are the “pre-industrial” GHG, ozone & aerosol levels taken from? Are these also CMIP5 recommended values?

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[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.

[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.

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.

[p7.14] “individual years” would better be phrased as “specific years”.

[p9.26] Insert word “lower” before “boundary conditions” to distinguish from lateral.

[p9.32] “boundary conditions common to both” is incorrect here, should be “forcings common to both”.

[p11.7] Unnecessary comma after “weather@home”

[Fig S1] “summertime” should be “wintertime” if genuinely June – August.

[p14.19 and elsewhere] Massey et al. “2014” should be “2015”.

References

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