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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Using synoptic type analysis to understand New Zealand climate during the Mid-Holocene" *by* D. Ackerley et al.

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1 Responses to specific comments and suggestions.

The authours would like to thank the the reviewer (J.J. Gomez-Navarro) for the very constructive and supportive comments after having reviewed this paper. The reviewer brings up several points that require action or discussion, which the authours answer individually below.

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Interactive Discussion

Discussion Paper

1. I miss a comment in the introduction to justify the choice of the target period. Is this period representative somehow of the general condition during the mid-holocene? Is it used simply because there exists a good overlap of all datasets (simulations + reconstructions)? In addition to this, I do not think that the main aim of the paper is clearly stated in the abstract nor in the introduction: is it to gain insight into the understanding of the climate conditions during this period, or is it to develop a methodology in principle suitable for other periods and target areas, so this period is chosen just by convenience?

   Response: The target period is chosen as it is the period in time that the PMIP2 experiment was run for (cited through Braconnot et al., 2007, in the text) and so allowed us to potentially tap into a wealth of model data. Also, there are a lot of palaeoclimate studies using proxy-data from New Zealand for this period, as cited in the text. However, the availability of model data for this period was the driving force behind the period chosen. The study is also attempting both of the aims stated by the reviewer. We would like to find a method that can downscale coarse-resolution GCM data into high-resolution data. This is especially useful in New Zealand where the surface topography interacts with the prevailing westerlies at much finer scales than a GCM can feasibly resolve. Also, as Kidson (2000) has developed and tested this method thoroughly (also updated in Renwick, 2011, as cited), and Lorrey et al. (2007) applied this method to decadal variability and palaeoclimate studies (as cited in the text) the next logical step would be to apply the Kidson (2000) method to a period of the past with available model data (where uniformitarian principles may apply). If the application is successful then this method can be applied to other areas of the world; however, success can only be evaluated with dynamically downscaled data (using an RCM), which is beyond the scope of the current work. The authors do feel that with the citations and discussion presented in the introduction that the two aims of the paper are stated and justified (second to last paragraph of the introduction).

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2. In the methodology, the description of the models looks heterogeneous. Some of the models (like ECHO-G) are more deeply described than others (MIROC). Further, the format of the description given for the spatial resolution in degrees is different in every model. I think this part should be rewritten to make it more homogeneous.

   Response: We have included a new table (Table 1) that describes the resolution of the models and notes the important references for background to the models. We have included the important details of each model in a list in section 2.1, which is more homogeneous than before.

3. Regarding the description of the simulations, I think it is not clear enough the difference between the orbital forcings used in the simulations. Is it the same in all the models? How are exactly estimated these variations? This has also to do with Fig. 2. It is only briefly referred in some parts of the main text, but it is not discussed or clearly explained referring to the configuration of the simulations. Is this figure representative of the four simulations? In general terms, the modifications in the orbital forcings with respect to the present, and how it is implemented in the four models, is not very well stated in the paper, although it is an important part of the description of the work.

   Response: We have completely updated section 2.1 to include a full description of what orbital parameters were changed in these simulations and we refer the reviewer to the paper by Braconnot et al. (2007) (see www.clim-past.net/3/261/2007/cp-3-261-2007.pdf, which is open access) for the actual values used. The values are used by the radiation scheme in each model to calculate the insolation at the top of the atmosphere based on time of day, season and the value of the solar constant. We have also stated that the insolation changes given in Figure 2 are representative of the differences in insolation at the top of the atmosphere in each model as the solar constant is specified by the PMIP2 experimental design and therefore identical. The insolation differences between the Mid-Holocene and pre-industrial simulations will be identical to Figure 2 in the CSIRO, HadCM3 and MIROC models as they were all set up in an identical manner to the PMIP2 runs (which are described in Braconnot et al., 2007). The ECHO-G model, being a transient simulation, will have a time varying value for eccentricity, obliquity and angular precession, but as the data were taken from 6000 - 5950 ka, the variation in the insolation characteristics will still be very similar to those of the other three models.

4. The chosen periods taken from the simulations to perform the analysis are different in different simulations: CSIRO and ECHO-G consider a fifty-years period, whereas HadCM3 and MIROC consider 100. Also the exact selected years is not clear in some models. This aspect should be clarified, and the reason why the periods have different length discussed.

   Response: This is beyond the control of the main authors of the paper as we did not actually run the simulations at NIWA and acquired as much available data as were possible from the modelling groups. The HadCM3_UB and MIROC modelling groups provided the data from their PMIP2 simulations that were run as 100 year time-slice experiments where the conditions at 6ka were held fixed. Details of the set up for these models can be found in Braconnot et al. (2007) as cited along with the references given in the new Table 1. The CSIRO model data were from 50-year time slice experiments (the same runs submitted to the PMIP2 database) and output SLP at 00z each day (provided by S.J. Phipps). The data from ECHO-G were taken from a transient simulation run from 7.5ka to 4.5 ka, and 50 years of data from 6000 - 5950 were available for us to use. The details of this simulation can be found in Wagner et al. (2007) as cited. We have now included the author that provided the simulation data (see section 2.1) to show that the models were not run 'in-house' at NIWA as this collaboration developed through contacting these modellers (there were also other modellers that we contacted who did not provide us with data) after the simulations had

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been completed. Also, the time scale for decadal variability in the ocean is of the order 20 - 30 years and therefore 50 years of data would be enough to capture the steady state properties of the climate. Beyond this, the point of the paper is to identify the similarities of the models and despite the differences in data length and models run, the results still show a strong coherency between the models despite the lack of identical data specifications, adding stength to our result.

5. In P1310 line 5, a brief explanation on how exactly the SLP was converted to 1000 hPa height using temperature could be added.

   Response: The geopotential height can be calculated by using the hypsometric equation which is a function of the depth between two isobaric levels (in this case the pressure at sea level and that of the 1000 hPa level) and the temperature throughout the depth of that level. We have stated that we use the hypsometric equation to do this in section 2.2. These equations can be viewed at the AMS glossary of meteorology at: http://amsglossary.allenpress.com/glossary/search?id=hypsometric-equation1.

6. In section 3, the number of subsections is just the months they cover. I think it would be more illustrative and would ease the reading of the paper if the name of the section includes somehow the name of the season. This is, instead of "DJF", "Winter (DJF)" or something similar. This minor change would clarify the discussion, avoiding the reader having to realise what season corresponds to every set of months.

   Response: Included.

7. P1315 line 6. "more disturbed" compared to what? Did you mean "different compared to the PI"?

   Response: The authors have included “compared to the pre-industrial simulations” at the end of that sentence.

8. P1315 line 22: "disturbed pattern". I do not understand. Do you mean that the SLP pattern is not consistent among models?

   Response: The authors see there may be some confusion with the use of 'disturbed'. We mean that the synoptic patterns are more 'disturbed' such as more unsettled synoptic patterns (troughs) or increased westerlies (zonal). We have included this statement in the first sentence of section 3.5: "(i.e. either strengthened westerlies or more unsettled 'trough' conditions)." We have also put inverted commas around disturbed to show that we are using the word in a specific context relating to the synoptic analysis.

9. P1316 line 1, I think it would be more coherent with the structure of the paper to describe the VCSN dataset in the methodology section, instead of delaying it to this section.

   Response: While the reference to the VCSN data could be put in the methods section, the authours think that as the reference to it is very brief that by introducing it as a separate section would interrupt the flow of sections 2 and 4.1. The description leads into the discussion of Figures 7 and 8 and is justified in this case. Also, we have included a reference to Renwick (2011) as supplementary material which gives a thorough and illustrative review of how the method is done.

2 Comments on figures and tables

1. Table 1 is very illustrative, and in my opinion it is very important for the explanation of the results, but it takes some effort to read correctly the results. For this reason, I would recommend the authors to somehow convert it (or include) a graphical version of it. For example, in the discussion it is clearly very important how the importance of different weather types varies within the annual cycle, and how accurately this cycle is reproduced in the different models and in the

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reanalysis. Then, I would suggest to make a figure showing the annual cycle so that you can easily identify visually what season, model or weather type performs better. There are many ways this could be achieved. For example through bars figures, one per each regime, with a colour bar representing a given model plus reanalysis, and the horizontal axis showing the four seasons.

Response: The authors do have a preference towards a graphical representation of data over tabulation. However, in this case as there are 4 simulations used for each season at 0k and 6k (plus NCEP values), this would make any figure very complex and difficult to interpret.

2. Figure 2 is nearly ignored in the main text, or only referred without a clear discussion. **Response**: Figure 2 is referred to in the introduction but as the reviewer correctly points out, it is not referred to in enough detail beyond that. We have now stated in Section 2 that the differences in insolation are representative for all of the models. We will also refer to Figure 2 when we refer to the effects of insolation (Section 4.1).

3. Figures 3,4,5 and 6 could be edited trying to reduce the blank spaces between maps, thus increasing their size and making them more readable. The authors could also consider using shaded maps with colours instead of solid/dashed lines for positive and negative values.

   Response: We have edited the figures to make them larger but this can also be edited further in the copyediting stage. As for the use of colours, we have tried this and the figures become very 'busy' when colour is introduced and we feel that the use of dashed and solid lines is the best way to present the data in this case.

4. Figures 7 and 8 could also be edited to remove blank spaces. In addition, the scale in the four season is the same, so they could be merged in one large scale somewhere in the side or below the maps.

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Response: Figures updated. Further changes can be made at the copyediting stage for the paper (sizes etc.).

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