Clim. Past Discuss., 7, C846–C849, 2011
www.clim-past-discuss.net/7/C846/2011/

Climate of the Past Discussions

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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Received and published: 15 June 2011

General comment:

the paper analyses how the changes in orbital parameters during the mid-holocene could affect the synoptic conditions near New Zealand, and how these changes produce modifications in seasonal temperature and precipitation during this period. For this purpose, the authors use a set of four climate simulations performed with four AOGCMS of varying complexity, and focus in the conclusions that are shared among the models. They also compare the results of the climate simulations with proxy-based reconstructions, and find a relatively good agreement between both approaches, showing a coherent picture of the climate in this period. The methodology presented is inter-

esting because it is able to bridge the gap between the coarse resolution of AOGCMs and proxy-based reconstructions. The paper is well written and correctly structured. There are however a number of comments/suggestions that could improve the paper in my opinion.

Detailed comments:

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?

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.

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.

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.

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

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.

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

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

7. 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.

Comments on figures and tables:

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 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.

Figure 2 is nearly ignored in the main text, or only referred without a clear discussion.

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.

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.

Interactive comment on Clim. Past Discuss., 7, 1301, 2011.