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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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1 Responses to specific comments and suggestions.

The authours would like to thank the the reviewer (M. Rojas) 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.

1. First, Rojas and Moreno (RM) have published a paper in Climate Dynamics in 2010, that analyses the PMIP2 Mid-Holocene simulations in the Southern Hemisphere, with emphasis in Patagonia and New Zealand (Rojas, M. and P.I. Moreno,

2010: Atmospheric circulation changes and Neoglacial conditions in the Southern Hemisphere mid-latitudes: insights from PMIP2 simulations at 6 kyr. Climate Dynamics, DOI 10.1007/s00382-010-0866-3). Some of the discussion of your results should reference that work. There are some coherent results in both papers, and others not. This should be addressed.

   Response: We thank the reviewer for bringing this work to our attention and agree that it should be included to provide context for our results. We hope that our inclusion of the work in our updated version is satisfactory.

2. Abstract: *Include in line 13: ..,we find at 6000 BP, increased.....

   Response: Changed as suggested.

3. Introduction: *Change in line 26: This model-proxy model intercomparison is an essential test to establish the ability of a climate model ... *page 1306, line23: you might want to use the word "robust" ?

   Response: We have edited the first paragraph of the introduction with the following in response to this comment: "Proxy-model intercomparison establishes the ability of climate models to simulate past climatic change, which is an essential step in evaluating the GCMs used to simulate future climate." We do also agree that 'robust' would be a better word to use than 'real' in line 23.

4. Models, data and method *I think the model description can be shortened with a table.

   Response: Done - we have also shortened the model description section to be more concise.

5. EOF procedure for synoptic classification (typo in procedure)

   Response: Typo corrected.

6. I am not sure I understand how the regimes were calculated and the precipitation and temperature composites for those regimes. From the text, it seems that you used "1000hPa geopotential height" for the regimes (1972-2009?), but for the temp. and precip. composites you calculated again 3 regimes with daily SLP ? Please clarify. Are the models forced to match one of the 12 Kidson's synoptic types? Couldn't you just calculate the with a cluster analysis 12 clusters (freely in the models) and compare those to the 12 clusters in the reanalysis? Maybe I am not understanding well here! Also, I believe that it would be useful to actually show the precipitation and temperature composite for each of the 3 regimes (not separated by season, just 1 more figure 3 panels, or 6 panels if you include the models composites). How are the climatological precip and temp fields in the models? Might the difference between the relative occurrence of the regimes in models versus reanalysis explain any observed biases in the model?

   Response: There are several parts to this and we will address them individually. Firstly, the calculation of the regimes, and the temperature and precipitation. The regimes were calculated simply by summing the frequency of occurrence of each synoptic type within the regimes specified in Figure 1, namely 'Trough', 'Zonal' or 'Blocking'. As the models could only provide us with sea level pressure, we had to convert those values to geopotential height so that in all cases geopotential heights (not SLP) were used to calculate the frequency of occurrence of the Kidson types. Also, section 2.5 (of the discussion paper) only gives a brief overview of how the EOFs were calculated, which can be seen in much more detail in Kidson (2000). (The following has also been included in the updated text) The daily VCSN fields were averaged by regime type and season to produce a mean field of temperature and rainfall anomalies (differences from normal) for the present day (as can be seen in Renwick, 2011, see supplementary material). To derive the changes in surface climate anomalies associated with modelled mid-Holocene climate, we calculated weighted averages of the individual regime mean fields. The weights were the changes in percentage frequency of occur-

rence of each regime (from Table 2 - DT, DZ and DB) between mid-Holocene and pre-industrial, from each of the model runs. Ensemble mean anomalies were calculated by averaging individual results across all the models, as shown by season, in Figures 7 and 8, for temperature and precipitation respectively.

   Secondly, The models are not 'forced' to match a synoptic type as we state that "The closest synoptic type is calculated as the minimum Euclidian distance to the mean height field associated with each of the synoptic types", meaning that the synoptic situation of the data is assigned as the type that resembles the synoptic situation in the model the closest. Finally, I understand your point about adding extra figures to the paper, however this may upset some of the balance of the paper. Instead, we would like to introduce the paper by James Renwick (Renwick, 2011) which provides a thorough review of the creation of these climatology maps and the associated changes in temperature and precipitation induced by changes in the synoptic regimes. We feel that it would be useful information for anyone reading this paper to have and would provide a more thorough and accessible background to the method we are using. If the reviewer / editor disagrees then we will update the paper accordingly. The Renwick (2011) work is cited in the updated version of the paper as supplementary material too.

7. Finally, my main problem is with the discussion, which is somewhat contradictory with your own results and sometimes contradictory with my results. In Rojas and Moreno we used 11 models, so part of the differences can be related to this, however robust results in both papers should agree. This should be conciliated.

   Response: The authors hope that the inclusion and discussion of the results of Rojas and Moreno now provides more balance to our argument. However, the differences between this study and Rojas and Moreno help to identify the caveats in our method and provide a platform from which we can undertake future work (in this case by undertaking RCM simulations to really see if this analysis using the synoptic types really matches up with the dynamically downscaled model data). Conversely, this study is able to make finer-scale inferences of the climate of New Zealand that the Rojas and Moreno paper cannot do due to the coarse resolution of the GCMs employed. In the case of topographically diverse regions (such as New Zealand and Patagonia), downscaling methods such as the one employed here, are potentially more useful than GCM output. Also, as the Rojas and Moreno paper (as you stated) used 11 models and our study only used 4 (including one not used in Rojas and Moreno), the differences could be due to us having a smaller sample size and the larger relative influence of a model not used by Rojas and Moreno.

8. Results comments on discussion in 3.1-3.5: From table 1 I see that in most seasons the increase/decrease of events in the trough regime is compensated by decrease/ increase in the zonal regime, but you choose to comment on 1 of them only depending on season. For example significant increase in trough regime in MAM is mentioned, but not the also significant decrease in zonal regime during the same season. Because I don't have a clear picture how the regimes project on precipitation and temperature, it is not clear to me what to expect from those changes in terms of precipitation and temperature. Why is the increase in zonal regime in SON (also seen in RM) related to decreased precipitation in NZ? Or are the colours in WSI positive? (I can't distinguish the colour scale around zero, in figures 7 and 8)

   Response: There are several references in the paper that show the effects of changing the frequency of occurrence of the climate regimes such as Kidson (2000) and Renwick (2011). We have included the Renwick article as supplementary material so that the climatologies for these regimes can be easily viewed by the reviewers and the scientific community in general. They were published in the NZ Met. Society journal, Weather and Climate, which is not currently visible online. We have changed the manuscript to note this supplementary material such that it can be referenced and viewed easily to demonstrate the effects of

each of the regimes on NZ climate. It also shows why we talk about the increases in a certain regime class as when a specific regime begins to dominate, it has distinct precipitation and temperature charcteristics associated with it. This is why we focus only on the positive changes but it is useful to know whether the increase in one regime results from decreases of just one or both of the other regimes as it may give us an insight as to how the general circulation around New Zealand may be different. As for the effect of the increased zonal regime in SON; there is generally higher pressure over NZ in 'zonal' events, which leads to reduced precipitation (see supplementary material). However, as the winds will be from a westerly or southwesterly direction, there may be some increase in the precipitation in the WSI. The colours do show that there is little change in precipitation, despite drying elsewhere, which indicates that the WSI remains relatively wet under zonal regime conditions.

9. Summary *line 8: increase in zonal types in JJASON. The PMIP model mean shows decreased westerlies in JJA, and increased only in SON.

   Response: The results do suggest an increase in what is termed 'zonal' conditions based on the work of Kidson (2000), however as stated in Section 3.3, the models seem to show a preference for an increase in the 'H' weather type for the zonal regime, which is associated with high pressure sitting over and slightly to the west of the North Island (see Ackerley et al., 2010, Fig. 1). While this type is defined as 'zonal' it is likely to be associated with weak westerlies over much of the country, which agrees with the results of Rojas and Moreno (2010). We have updated sections 3 and 5 to make this point and have cited the Rojas and Moreno (2010) paper here too saying that the patterns are broadly similar.

10. *page 1316: typo: resoution *page 1317, discussion on the temperature changes forced by insolation changes versus regime changes. You should comment on the MAM in the Northern Island, which shows an important warming! *page 1318,

line 1.4: this reduced seasonally is also shown in figure 7 of RM (except in the southern part of SI).

Response: Typo - resoution - changed. We do discuss the warming in MAM for the North Island on page 1316 of the original document, however, this warming does not overcome the effects of the reduced insolation in MAM (stated in section 4.2). This is consistent with the Rojas and Moreno (2010) paper which shows temperatures around the North Island were approximately 0.5C colder 6000 years ago. We state that the insolation is likely to be the main driver and that the temperature changes given in this paper (Ackerley et al.) are only circulation driven and take no account of the insolation. However, the authors feel that this would be a good place to state the results of the Rojas and Moreno paper as they clearly show that the circulation-induced changes in temperature are easily overcome by the larger-scale, insolation-driven changes in temperature.

11. Conclusions page 1322, line 19: figure 2 of RM indicates cooler temperatures in all seasons, except SON.

Response: The authours agree with this but our analysis does not show this as it takes no account of the overall large-scale, insolation-driven effects on surface air temperature. This is one of the caveats that we do state, however, again this would be a good place to cite the Rojas and Moreno paper to demonstrate the issues with using the circulation characteristics of the model to derive temperature. However, we have made a conceptual and methodological improvememnt to analysing raw GCM output by linking local-scale variables (temperature and precipitation) to variables that are respresented better in GCMs (such as the large-scale atmospheric circulation). The low-resolution of the GCMs used in Rojas and Moreno (2010) as well as the study presented here (Ackerley et al.) makes it difficult to infer regional-scale temperture differences across the New Zealand land mass due to its topographical diversity. While insolation may be driving the hemispheric changes in temperature and precipitation, local topographical fea-

tures and their interaction with the prevailing atmospheric flow will be important in governing the responses of our proxies (and the climate regime they indicate). This method of downscaling attempts to bridge the scale-gap that a GCM cannot achive (currently with the data we have available) and the differences between our results and those output directly from a GCM should be used to instigate further analysis without necessarily requiring the need for reconciliation.