Clim. Past Discuss., 7, C1341–C1348, 2011
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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.

D. Ackerley et al.

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Received and published: 29 August 2011

1 Responses to specific comments and suggestions.

We would like to thank the reviewer (R. Newnham) for the constructive review of this work. We have addressed each of the points raised individually below, which have helped to improve and clarify several points of the work.

1. The analysis of synoptic weather types for the MH is presented on a seasonal basis and so it would be helpful to know how the 12 synoptic weather types varied seasonally in the original Kidson analysis and hence in the modern climate. Table 1 presents the seasonal frequency of occurrence of trough, zonal and blocking

regimes determined from the models for both the MH and a pre-industrial (PI) control. A strong match between the PI seasonal synoptic frequency data and the seasonal frequency of synoptic weather observed in the modern climate would strengthen confidence in the models ability to faithfully reconstruct MH patterns.

Response: The use of reanalysis data to indicate whether the models are performing well is important to provide a basis for the conclusions we draw. We have stated the frequency of occurrence of the synoptic regimes in Table 2 (formerly Table 1) for each season before the values for the models are stated. We also state in Section 3 (opening two paragraphs) that the models generally compare favourably with the NCEP reanalysis output. As we have specified these values here for comparison with the model output (and described the various merits / discrepancies of the models) we do not see the need to quote the values of Kidson (2000) specifically as the values in Table 2 (formerly Table 1) are the most up to date. However, the authors did notice that we had not specified in the caption of Table 2 (formerly Table 1) that the values derived from the NCEP data are presented. We have now included this in the caption.

2. The detailed description of model parameters presented in Sections 2.1 to 2.4 would be more effective as a table. This would enable a more systematic and accessible comparison across the models. On a related note, there is no attempt to relate differences between model outputs to differences in these model parameters. An implicit assumption is that coherency between models implies confidence in the results. Presumably the reverse also applies as well, unless differences can be explained in terms of different model parameters such as GHG input or spatial resolution. Also, please specify what orbital parameters were used in the models (in particular was regional insolation input on a seasonal basis).

Response: The authours agree that a table would be better for describing the model resolution and the number of years of data used. We have also included the main reference for the individual models in the table. We do not attempt to relate the differences between the models to the differences in model parameters as this is beyond the scope of this paper. While we accept that the differences will be influenced by different model parameterizations, identifying the fundamental reasons for the differences in climatology in these complex, fully coupled ocean-atmosphere GCMs, would require a separate paper that undertakes a full model intercomparison. Therefore we look for coherent patterns across the small ensemble and focus on those as robust signals associated with changing the models' insolation characteristics. The orbital parameters are specified in more detail now in Section 2.1 along with a citation back to Braconnot et al. (2007) where the PMIP2 parameters were defined. Insolation is also input based on each of the models' radiation schemes. In the case of HadCM3, the radiation is updated at all grid points every three hours and takes into account the daily changes in insolation throughout the year. Likewise, in CSIRO Mk3L full radiation calculations are performed every two model hours. The amount of radiation reaching the surface of the Earth (after specifying the solar constant) depends on the time of day and season along with the values for the eccentricity, obliquity and angular precession.

3. I would like to see a stronger and more coherent argument that the principal MH-PI climate differences can be attributed to changes in synoptic weather patterns reflecting changing atmospheric circulation (as strongly implied by paragraph 2 of the Abstract) rather than changes in seasonal insolation forcing. Beyond the Abstract, there is some ambiguity in the relative emphasis given these two factors (which may be linked of course). For example, p1317, para 2 states that Overall, it seems likely that the seasonal insolation changes may have had a larger influence on surface air temperature than the changes in the synoptic regimes and the next sentence suggests that the synoptic regimes were a secondary factor cf insolation changes. These statements seem at odds with the Abstract and

the conclusions (in particular top of p1322) that attribute temperature changes inferred from proxy records to circulation changes alone.

Response (a): The authors agree that there is no mention of the effect of insolation in the abstract but it is discussed in detail in the text. We have included the following in the abstract to highlight this point in the second paragraph after 'events': "however the circulation induced changes in temperature are likely to have been of secondary importance to the insolation induced changes." Also, the citation of Rojas and Moreno (2010), who discuss the changes in temperature from an ensemble of PMIP2 simulations, is now included to help provide more context for the temperature changes and why insolation is likely to be driving them. We have also changed the start of the first sentence on page 1322 of the discussion paper to read: "Overall, the circulation induced precipitation and temperature... (inferred from the synoptic regime analysis) in DJF..." to highlight that it is only the circulation induced changes that we have inferred from the Kidson analysis (**end of (a)**).

Later (on p1322, para 3), it is suggested that although the model outputs show no evidence of an overall shift to cooler temperatures (as can be inferred from the proxy records) this might be at least in part explained by the difference in Earths orbital parameters during the MH, in particular towards lower insolation during the growing season. But if orbital parameters were included in the models (see previous point) then why haven't the cooler overall temperatures been simulated? Previous Holocene climate reconstructions from NZ proxy data (principally by McGlone 1988; McGlone et al., 1993) have emphasised the link with precession-led changes in regional insolation which resulted in more moderate seasonality (cooler summers, warmer winters) in the early Holocene cf the late Holocene (warmer summers, cooler winters). New Zealand Holocene pollen records in particular are consistent with cooler summers in the MH (cf today) and/or a shorter growing season. It seems to me more likely that the temperature changes im-

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plied by the proxy data, if not the models (except in spring and summer), are most likely linked to seasonal insolation changes while the precipitation changes implied by proxy data and models are linked to circulation (and synoptic weather) changes. Perhaps this is what the authors think as well: if so, it is not clear in the current version.

Response (b): The authors agree with this point also as it does not fully reconcile with what is said in the results section. We have included the following “While there is no evidence that circulation changes alone give rise to cooler temperatures in the models, there is evidence for a cooler autumn in the South Island induced by the circulation changes. ”, which should clearly state that this is down to the circulation only as the synoptic regime analysis cannot account for the effects of insolation. We have also stated after ’further cooling’ (the next sentence): “however, the synoptic regime analysis cannot account for the effects of changing the insolation on surface air temperature and remains a limitation of this method”. It has also come to our attention that another paper has shown some of the differences in surface air temperature for seasonal and annual means (Rojas and Moreno, 2010), which we have now cited in the paper and used to provide further context to the results and further highlighting the limitation of the regime classification when attempting to reproduce temperature patterns of the past. The conclusions have been adjusted to reflect this and we invite the reviewer to look at the new version (and also the results). (**end of (b)**)

4. P1316, para 2 states that The higher incidence of trough events in MAM during the Mid-Holocene causes an increase in surface air temperature throughout the North Island (particularly in the east). This statement needs to be reconciled with that on p1305 (para 2) that states that trough regimes are associated with below-normal temperatures (implied throughout NZ).

Response: The results in the Kidson paper were for the annual mean and there are seasonal dependencies on how the different regimes may affect surface air temperature. We have included the reference to Renwick (2011) which shows this and we are aiming to include the Renwick (2011) paper as supplementary material, which should help provide a further illustration of how this method is applied. We have also changed the text on P1305 and P1316 or the discussion paper to include: "....associated with above normal precipitation throughout New Zealand and below-normal temperatures in the annual mean." and added the words 'annual mean' in the descriptions of zonal and blocking types below the edit given above. Secondly, in section 4.1 we have added:

"The warming in the North Island appears to be counter-intuitive with the annual mean results of Kidson (2000), however Renwick (2011) indicates that the surface air temperature anomalies associated with the trough regime have a strong seasonal dependence that leads to an overall warming in the North Island during the colder seasons (such as JJA or MAM), which are consistent with the results presented here."

5. P1318, S4.2. The definitive paper on Ascarina and mid-Holocene climate reconstruction is still McGlone & Moar, 1977; The Ascarina decline and post-glacial climatic change. NZ J Bot 15: 485-9. As the MH Ascarina decline is pivotal proxy evidence in this discussion, that paper should be cited.

   Response: It is included now in Section 4.2.

2 Responses to technical comments.

P1311. L21, 'representataion' - **Response**: Done

P1317, L9, 'throught' - **Response**: Done

P 1318, lines 20-23 concerning environmental shifts between 7.5 and 3 ka. Should clarify that the authors cited were referring to shifts from the early Holocene to this period whereas elsewhere in this paper the term shift or change is implied to mean shift or change from the present.
Response: We have edited the text to indicate that we are referring to a shift during this period and not relative to the present day. It now reads in Section 4.2 as: "The interpretation provided by Li et al. (2008), indicative of reduced humid northerly flow and increased southwesterlies, is similar to that of McGlone et al (1993), who suggested that at some time between the early Holocene and 7.5 to 3 ka, key environmental shifts occurred in many northern and western areas of the country, with the onset of wetter winters, a slight cooling in climate, and an increase in southerly fronts and frosts in New Zealand."

P1319, L 24: a circulation occurred - circulation shift? - **Response**: Changed to circulation shift.

P1321, L10, I would suggest patterns between the 4 GCMs are mostly coherent. - **Response**: Changed as suggested.

P1321, L14, 'much colder SI temps in MAM'? Colder, yes, but only by up to 0.2 oC in the alpine regions or extreme south; elsewhere $<=$ 0.1 oC. - **Response**: The authors agree and we have changed 'much colder' to cooler.

P1322, L7, With reference to point 3 above (Specific Comments) I suggest insolation changes should be added as a causal factor (if not primary) in lower temperatures. **Response**: The authors agree fully with this point (also see response to point 3) and we have included the following at the end of the first bullet point: ", likely to be driven

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by the reduction in insolation from December to May."

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