Clim. Past Discuss., 8, C1690–C1693, 2012
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Climate of the Past Discussions

CPD
8, C1690–C1693, 2012

Interactive Comment

Interactive comment on "Temperature response to external forcing in simulations and reconstructions of the last millennium" *by* L. Fernández-Donado et al.

Anonymous Referee #1

Received and published: 25 September 2012

The authors describe the simulations covering the last millennium performed with GCMs before the CMIP5-PMIP3 coordinated experiments as well as a comparison of those simulations with reconstructions. The description of the models, experimental set up is very precise and helpful to propose hypothesis explaining the different results of the different models. In particular, the discussion of the influence of the magnitude of the solar forcing is instructive. I would have been happy of course to see also a comparison with the new CMIP5-PMIP3 simulations but the paper is already quite long and adding new experiments would have certainly make it harder to follow. The present work could then be considered as a basis with which the new simulations can be compared. On the same lines, some parts of the manuscript may have been included in supplementary material to shorten the text but, as the structure is clear, the reader can go quickly to the parts s/he is interested in, so I am fine with the present way of presenting the information. I suggest thus only minor changes before the final publication.

From the disagreement between the model results and reconstructions, the abstract suggest a large role of internal variability in the spatial distribution of the changes between MCA and LIA. If it was the case, by chance, a few members of the whole ensemble should have a pattern similar to the one reconstructed. This does not seem to be the case (see Fig. 7). We could of course not rule out the fact that the real observed pattern is related to the occurrence of a rare event but the disagreement could also be due to a bad representation of the response to the forcing in models, to uncertainties in the forcing time series, uncertainties in the reconstruction, or to a bad representation of internal variability in models. This should be indicated in the abstract (and where needed in the main text) to have a fair balance between the hypotheses.

Page 4012, line 19. It is stated that the range of forcing goes beyond the one of the CMIP5-PMIP3 exercise. Is it still valid when all the forcing proposed in Schmidt et al. 2012 are included?

Page 4014, line 5. How the radiative forcing of land use changes is calculated? Does it include only the albedo effect or also the role of modifications in exchanges between land and the atmosphere (of water, momentum)? See also page 4020, line 21.

Page 4016, lines 3-7. CMIP5-PMIP3 protocol suggests several reconstruction of the TSI, not just one.

Page 4022. I do not understand well the last sentence, please rephrase.

Page 4024, line 24. I think it is important to explain here how the uncertainties are obtained.

Page 4027 and Figures3-4. I do not see why changing of reference period so I suggest keeping the same one.

Page 4028, line 4. Personally, I do not see the minima in the Wolf, Spörer, Maunder and Dalton intervals in all the simulations so this sentence should be more precise or the amplitude of the minima quantified to convince the reader.

Page 4028, line 23-25. The agreement between data and simulations using different reconstructions of the solar forcing should be quantified. Sentence like "sTSI ensemble seems to follow most closely the reconstruction ensemble average" are not precise enough. Furthermore, is the agreement better for all the members of the sTSI ensemble or just for a few? For instance, a comparison of the variance over the pre-industrial period, in an additional table, may be instructive. The discussion of those points could also be related to the one of figures 5 and 6 and to section 6 (last sentence of the section for instance).

Page 4029, line 1. The role of internal variability is mentioned several times in the manuscript but maybe it could play a role in this discrepancy too.

Page 4029, line 2. Large discrepancies are also found during the 20th century.

Page 4031, line 14. Do the models also underestimate the variability compared to instrumental observations during the last 150 years?

Page 4031, line 21. The difference of variance in the 10yr timescale between models and observations does not appear to me as outstanding compared to other periods. Could you please quantify this point?

Page 4032, line 20. Is a word missing in this sentence?

Page 4034, line 16. "Teleconnection" is probably not the best word in this framework. Maybe "pattern" would be better.

Page 4034, line 28-29. Why choosing those two models compared to the others? This does not seem obvious to me that they are clearly different.

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Page 4035, line 12. Why selecting one member of EC5MP-E1 ensemble?

Figure 7. For me, it is necessary to show the figure from Mann et al. (2009) for a better comparison. It would also be instructive to know if the spatial structure of the changes is more consistent between the models or between any model and the reconstruction, performing a spatial correlation for instance. A short discussion of the robustness of the spatial structure displayed in Mann et al. (2009) should be included too as it is an important element to judge of the realism of the models behavior.

Page 4036, line 22. The cooling in CNRM model should be briefly mentioned here.

Page 4037, section 6. I personally do not like the term "paleo transient climate response" as this may give the feeling that only one "paleo" response is possible while I am sure that the values would be different if one looks at changes during the deglaciation or the Holocene for instance. I would thus prefer if the authors do not introduce a new term here and simply describe their results as the correlation between forcing and temperature without proposing a new concept which is not general enough to my point of view and thus confusing. This correlation should also be dependent on the timescale of the forcing. It is the reason why I am not convinced by the discussion at the end of page 4038. The correlation is indeed performed on instantaneous fields, if I understand well, but the influence of the forcing at one particular time will be the sum of the instantaneous response plus of the delayed one. If a forcing acts in a similar way during decades, the response will thus be larger than the one of another forcing that has higher frequency changes. The analysis proposed induced then for me a mixture of response at different time scales. It does not mean that it is not interesting but this must be clearly discussed in the text.

Page 4037, line 19. What is exactly meant by "major levels of forcing"?

Table 2, line 1 of the caption, "forcing" is missing after "natural"

Interactive comment on Clim. Past Discuss., 8, 4003, 2012.