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Climate of the Past Discussions

Interactive comment on "Historical droughts in Mediterranean regions during the last 500 years: a data/model approach" *by* S. Brewer et al.

Anonymous Referee #1

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General comments

This paper presents an interesting methodology to assess drought patterns from observations or simulations. I am a bit frustrated that very few explanations or mechanisms can be inferred from the paper to account for the climate change from the LIA to now.

I appreciate that the authors took my earlier comments into account, which did clarify the new manuscript.

This paper deserves publication after some clarifications are done and the conclusion is focussed on what can be understood on climate, and what can be done to improve models.

Specific comments

It is always a good idea to make comparisons between models, but here the inter model differences are not sufficiently explained to make the paper useful to reader who is not a specialist on models. For example:

-The authors compare simulations of two different models, which have alledgedly the same forcings ("all forcings"). It is not clear to me that the forcings are treated exactly in the same fashion for the two models. In particular, the ways volcanoes and solar irradiance are treated can be very model dependent. The authors should clarify in the text whether the simulations can indeed be compared.

-The authors conclude that the ECHO-G simulation is "better" (at least for PDSI) than the HadCM3 simulation. The ECHO-G model has a much lower resolution, and the forcings seem to be less complete that for the HadCM3 simulation. This is quite disturbing for HadCM3.

-What type of soil moisture models are used in both GCMs?

-Are their SLP/SAT patterns realistic?

The reason why "fuzzy logic" is used is not very clear. In particular, the notations are not completely defined.

The conclusions list points of agreement/disagreement between models and data, but little is said about climate. Could the authors state more clearly what SLP patterns were enhanced during the LIA?

The SLP reconstructions of Luterbacher et al. (2002) could be used instead of the model simulations. Would this give different results?

Technical corrections

p. 773, l.15, the authors claim that the LIA starts in 1650. Le Roy Ladurie (1983) places it in the early 15th century. Bradley and Jones (1992) place it in the early 16th century (1510+/- 10 years). Please check your references.

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p. 777: It is not necessary to provide 3 digits in the correlation estimates. Can the authors give confidence intervals on the correlations?

p. 778, l. 23: The authors claim that "the simulated PDSI becomes dry in the 20th century in both models". This is hard to see in Fig. 2 for HadCM3 "all forcings".

p. 779. I am not sure that it is necessary to remind the reader of the k-means algorithm.

p. 780: Please define the acronym LAI.

p. 796: I don't understand why the echonat simulation is shorter than the echoall simulation, and the opposite for hadall and hadnat simulations. The "all forcings" simulated PDSI do not behave in the same way, even at low frequency, despite the forcings are allegedly the same. Why is that?

p. 800: Fig. 6, col D: The regime frequencies for the ECHOG simulation perform rather poorly. This should be discussed.

Interactive comment on Clim. Past Discuss., 2, 771, 2006.