

Interactive comment on “Technical Note: Probabilistically constraining proxy age–depth models within a Bayesian hierarchical reconstruction model” by J. P. Werner and M. P. Tingley

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The authors propose an extension of the BARCAST algorithm previously presented by Tingley and Huybers (2010) in order to account for chronological uncertainties. The objective is to provide joint posterior estimates of temperature and age–depth relationships, based on fairly generic hypotheses: AR1 climate process + independent Gaussian errors on observations (already in BARCAST), spatial correlation of climate signals (*idem*) and a transfer matrix between sample index and time (new to this study). The resolution algorithm relies on coupled Markov Chain, and the authors

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take great care of assessing the algorithm in two 'perfect model experiments', one with two records with one having a certain time scale, and one with 8 records + a number of control locations. The authors conclude on the potential of the model for effectively improving reconstructions, though this potential still needs to be assessed in actual scenarios where the BARCAST hypotheses may be challenged.

If, in the context of this "Technical Note", we are happy with the restrictive framework of perfect model experiments (albeit reasonably realistic), then the present contribution seems to this reviewer to be pretty faultless. A part from the minor comments below, the main substantial request would be to ask the authors to provide us with more explicit details about the coupled-Markov chain implementation, in particular the value of the heating coefficients and number of chains (it is written "at least" 6 chains). In summary, enough details for the reproduction of the experiments should be given. Electronic supplementary material can be an option (e.g. .RData files and source).

Minor comments:

p. 4502 I. 14: 'global-scale climate events' : I would delete the word 'climate', so that tephra layers can be included.

p. 4506 II. 19-20 : delete 'for details' at the end of line 19.

p. 4510, I. 5 : 'Although formally the full state of the MCMC needs to be swapped, it is equivalent to only swap temperatures'. I could not follow the argument, since the MCMC state also includes the ADM state? Please develop.

p. 4511 : I. 2 : 'this leaves enough data for the evaluation'. The casual MCMC user may be lost here. Does this mean that the scores such as CRPS, or Bar Reli are only evaluated far enough from swaps ? One or two more lines of explanation would make the text more accessible to the less specialised audience.

p. 4512, II. 4 - 20 : similar remark as above : a reading guide to appreciate what are acceptable CRPS, CRPS_pot or bar Reli coefficients would be welcome, otherwise the

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message of Figure 7 will remain cryptic to many readers.

p. 4513, l. 5 : "weighting by first the prior". Wasn't it the case that the prior is uniform across all the generated ADM samples?

p. 4517, l. 21 : the authors propose an adaptation of the algorithm to benthic sediments. The proposal would identify every depth level as a weighted average of states at different times. This is an interesting idea. Wouldn't this require, though, such a dense discretisation of the time domain that the resolution would be intractable?

p. 4518, l. 25 : extra 's' at increases.

Legends: Table 2 : clarify CRPS, Reli, etc. , at least by referring to the text.

Interactive comment on Clim. Past Discuss., 10, 4499, 2014.

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