Clim. Past Discuss., 11, C2405–C2406, 2015
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Climate of the Past Discussions

Interactive comment on "A Bayesian hierarchical model for reconstructing relative sea level: from raw data to rates of change" *by* N. Cahill et al.

Anonymous Referee #1

Received and published: 19 November 2015

Cahill et al present an improved method for estimating relative sea level changes from salt-marsh microfossils, that combines transfer function uncertainty and chronological uncertainty with a process model. The paper is well written, and I have only a few minor comments.

The values for the priors used in the model are not discussed in detail in this paper. I know there are many and at least some are discussed in previous work. I view this as important as some previous papers presenting Bayesian transfer functions have used very informative priors on the reconstructions, perhaps leading to artificially inflated cross-validation performance. This paper should make it clear that this is not case here.

Figure 2 would be more informative if the SWLI were given, and the observations sorted

by SWLI. I presume the observations are currently sorted by cluster rather than SWLI, which gives a misleading impression of how noisy the data are. The clusters add little in anything to the argument.

I suspect the "optima" presented in figure 4 are actually WAPLS-2 beta coefficients. It is misleading to present WAPLS-2 beta coefficients as is they were optima as they include a correction that accounts for secondary gradients and (mostly) edge effects. The WA optima could be shown.

I don't understand how empirical probability of occurrence is being used when both the calibration and fossil data are relative abundance data.

Two of the species show an uptick in probability of occurrence in at lowest SWLI which are ecologically questionable. Could these be an artefact?

Would it be possible to include information on salinity to further constrain the transfer function? Or are there insufficient data to do this well?

Interactive comment on Clim. Past Discuss., 11, 4851, 2015.