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11, C2898–C2903, 2016

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

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We would like to thank the reviewer for his/her valuable comments and suggestions. All of the comments are useful and valid.

The reviewer comments are in red, with our response immediately below in plain black font.

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

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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.

We agree with the reviewer and we will edit Section 3 of the manuscript to ensure that this is made clear throughout. The B-TF uses a uniform prior for unknown elevation, ranging from 0 to 160 SWLI, suggesting all elevations in this range are equally likely. This range is specific to this case study, and is realistic for the New Jersey sites. Other study sites would require an updated prior. We will edit the manuscript to explicitly state this. Section 3.3 will be updated to describe the priors for the secondary $\delta^{13}C$ proxy, noting that these are also specific to this case study. The priors in relation to the P-spline parameters are discussed in Section 3.1.

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.

We have produced an updated version of Figure 2. In the new figure, samples are organized by elevation as suggested by the reviewer.

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.

We would like to thank the reviewer for pointing this out. The values presented are the coefficients for WA-PLS-1. We have re-run the analysis to obtain the species optima and tolerances and included them on Figure 2. We will update Figure 4 accordingly. This update does not change the results, discussion or conclusions of the manuscript.

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

The B-TF does not use relative abundance data it uses the raw species counts. We apologise that this was not made clear and we will edit the text accordingly.

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

We suggest that, yes, the uptake in the response curve is an artefact of using the B-TF model. In the case of M.fusca and A.inepta the samples found at the upper end of the elevational gradient could be considered unusual. The B-TF does not assign a pre-determined response curve and is therefore more susceptible to these samples. It is left to the judgement of the researcher to decide whether or not to screen unusual samples or retain them. We did not remove the samples that appear to be responsible for the uptake because they were not removed from the original analysis presented in Kemp et al., 2013. We will edit the text in Section 5.1.1 to illustrate the options available when compiling a modern training set.

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

This is a good suggestion, unfortunately, salinity data wasn't collected and isn't available for this case study. However, we will refer readers to the Kemp et al., 2013 paper that includes a qualitative discussion about salinity and the foraminifera species specific to this case study.

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

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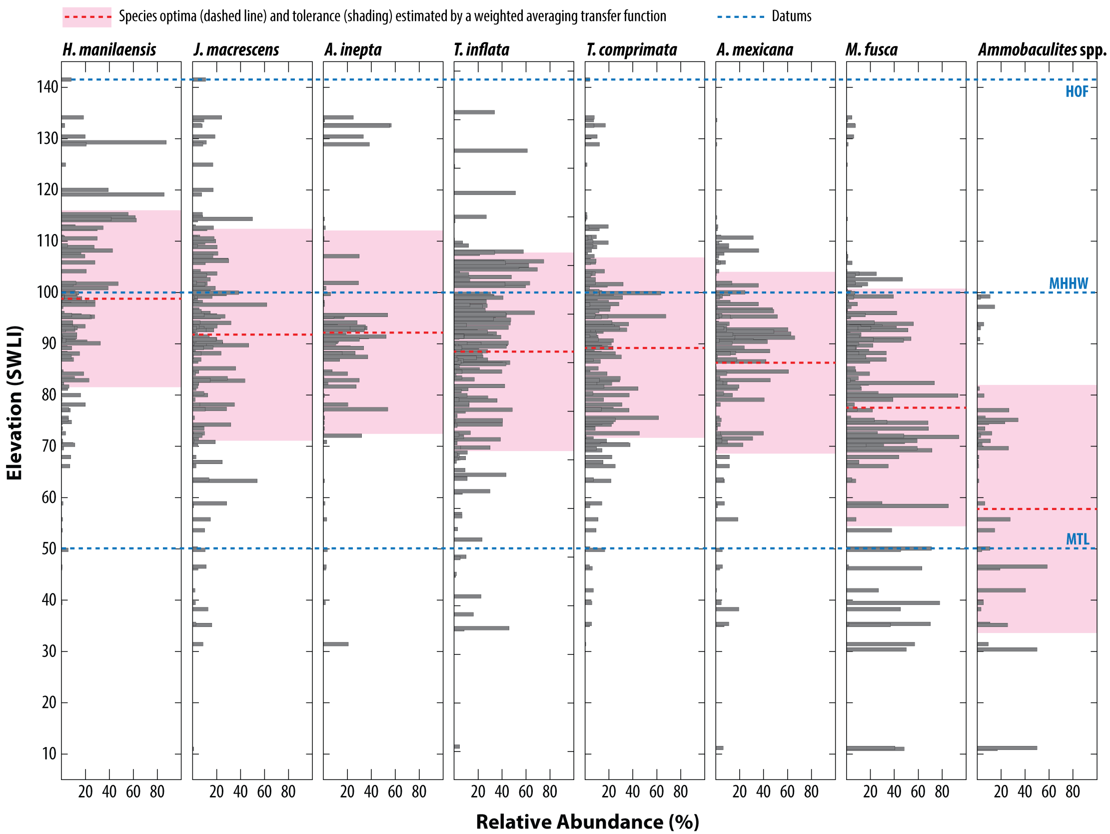

Fig. 1. Updated Version of Manuscript Figure 2

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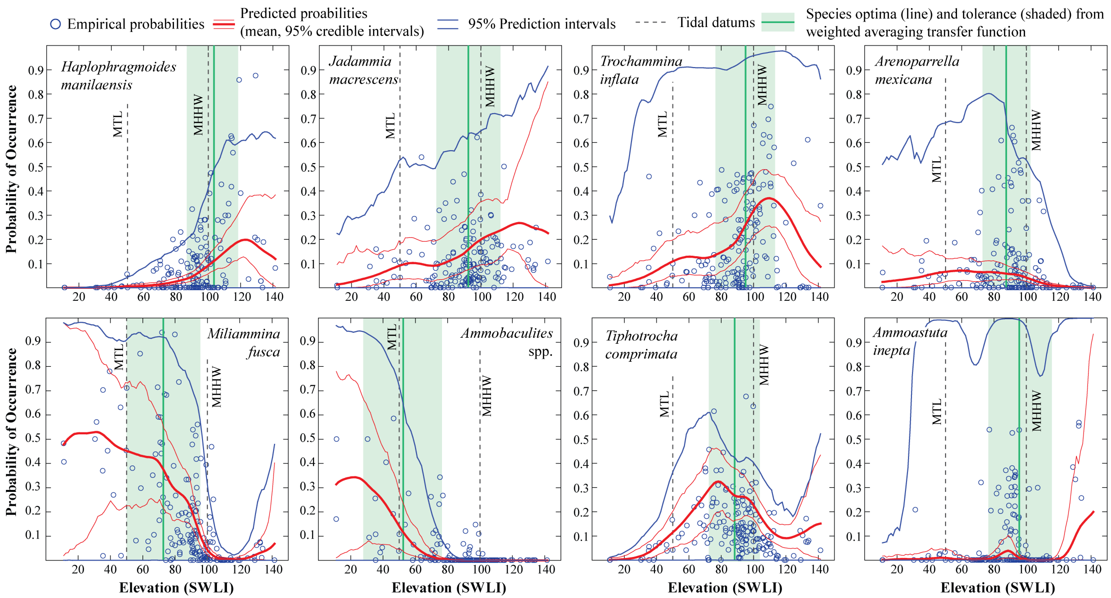

Fig. 2. Updated Version of Manuscript Figure 4

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