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Interactive comment on "Precise age for the Permian-Triassic boundary in South China from high precision U-Pb geochronology and Bayesian age-depth modelling" *by* Björn Baresel et al.

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The manuscript "Precise age for the Permian-Triassic boundary in South China from high precision U-Pb geochronology and Bayesian age-depth modelling", by Baresel, Bucher, Brosse, Cordey, Guodun and Schaltegger, first and foremost presents high quality chemical abrasion ID-TIMS U-Pb zircon ages for tuff beds and volcanic sandstones within strata of the Nanpanjiang Basin of South China. Second, the authors use these data, facilitated by the use of Bayesian age model construction, to interpret the timing and correlation of biotic and environmental events across the Permo-Triassic transition. These efforts confirm the high-resolution event stratigraphy proposed by Burgess et al. (2104) from the Meishan GSSP, but also establish the tempo of environ-

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mental change within the less condensed, more rapidly accumulating, deeper basinal records of the Dongpan and Penglaitan sections. The results are used to discuss the relative fidelity of the different biotic (conodont fauna) and environmental proxy (organic carbon isotope) records preserved in the three sections.

The broad interest and the quality of the data certainly warrant publication, and the manuscript is generally well-written. Nonetheless there are a number of issues I'd like the authors to consider in their subsequent revision of the manuscript.

a) Line 69: The authors state that "...applying Bayesian age modeling (Haslett and Parnell, 2008) based on these high-precision data sets allows us to detect sedimentary gaps and variations in sedimentation rate..."; certainly the age models establish the latter variations in sedimentation rate, however by definition the Bchron algorithm assumes constant sedimentation, thus its use cannot detect hiatuses and unconformities.

b) Line 162: The authors should provide a more detailed description of the lithostratigraphy and sedimentology of the Penglaitan section, particularly describing and interpreting the depositional characteristics and environments of the volcanogenic sandstones, which make up a significant amount of rock accumulation. Are these interpreted as turbiditic event beds? Gradual accumulations of sandy facies? This matters when it comes to the age modeling.

c) Line 192: The description of the Bchron model might be better stated as "The model is based on the assumption of random variability sedimentation rate, yielding a family of dispersed piecewise monotonic sediment accumulation models between each dated stratigraphic horizon."

d) Line 208: The Bchron algorithm doesn't necessary require the thickness of the investigated ash beds, in fact as noted subsequently it might be a mistake to use those thicknesses as input (see note to Line xxx).

e) Line 213: I would suggested that the authors provide their R scripts for their model runs as an Appendix.

f) Line 319 and Figure 3: There is a wide swing to lower ages in the Bchron age model for the Penglaitan section between PEN-70 and PEN-28, however it is not clear what is causing that excursion, unless perhaps it is because of the large input thickness for PEN-28. This highlights the question of how to handle the thickness of geologically instantaneous event beds like volcanic ashes. Using a thickness in the Bchron algorithm introduces a random uniformly distributed uncertainty in stratigraphic position for that dated horizon. Is this appropriate? In the case of a radiocarbon sample integrating a sampling thickness with an unknown duration of accumulation that might be appropriate, however a volcanic ash bed is deposited geologically instantaneously, e.g. there is no uncertainty in the duration of accumulation. One might argue instead that you should rescale your lithostratigraphy to remove the thickness of the volcanic horizons. In this way you might create a more accurate model of the deposition rates of the background sedimentation and rock accumulation between the dated volcanic events. I would suggest that the authors experiment with alternative age model construction; this might particularly impact the Penglaitan section given the thickness of the "volcanogenic sandstone" beds.

g) Line 340: The recalculation of age and uncertainty for Meishan Bed 25 sanidine (data for sample C-2 of Renne et al., 1995) using the method of Kuiper et al. (2008) should yield a result of 251.6 $\pm$ 0.6 Ma.

h) Lines 399-403 and Figure 3: The authors state, "When projected onto the age-depth models of Dongpan and Penglaitan, this UAZ1 is artificially expanded and even crosses the PTB in Penglaitan (Fig. 6). In Penglaitan, the last Permian UAZ2 projects correctly above UAZ1 without overlap but is completely within the Triassic. The cause of these contradictions stems from the irreconcilable conjunction of i) extreme condensation in Meishan, ii) high evolutionary rates of conodonts, and iii) the ca. 30 ka precision of the last generation of U-Pb dates." These conclusions appear to stem from equating

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the PTB at Penglaitan to the formational boundary, however this is an assumption that isn't necessarily accurate. In fact from a sedimentological perspective as well as the character of the age model for Penglaitan is seems likely that there is an unconformity at the top of the Permian strata, e.g. at the top of the volcanogenic sandstones. I would encourage the authors to reexamine their age model construction for Penglaitan considering the possibility of a hiatus across the PT transition; although beyond the scope of this manuscript it raises an important question for future work-how would you add possible unconformities at bed contacts into a Bayesian framework for age model construction?

i) Lines 447-449: The comparison of zonal construction using unitary associations versus first occurrences is not discussed here, thus this conclusion isn't substantiated by the contents of this manuscript.

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