

## ***Interactive comment on “Improving global paleogeography since the late Paleozoic using paleobiology” by Wenchao Cao et al.***

### **Anonymous Referee #2**

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The authors attempt to produce a flexible, digital representation of Earth's plates through most of the Phanerozoic. This representation should allow testing paleogeographic features of the original dataset against other datasets, adopting different rotation models as used in the original dataset, among other things. The authors then use a comparison of their original distributions of land and sea to that implied by the distribution of fossil organisms, to get a more accurate picture of the distributions of land and sea through Earth's history. These 'improved' distributions are then used for various comparisons with eustatic sealevel curves and measures for continental weathering.

Although the attempt to build a flexible model of Earth's plate movements through time is fine and useful, most of the subsequent comparisons are, in my view, redundant, insufficiently interpreted and discussed. Also the methods section needs improvements. In the present state I can only recommend to reject the manuscript, and to encourage

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the authors to focus on the core of their work (the model), to improve the methods section, and revamp their 'testing' and their discussion.

Detailed comments by line number:

106-108, there is another important bias in the PBDB: the uneven entry of fossil data.

116-117, repetition

145-147, I have the feeling that the authors are trying to explain here which environmental types have gone into the gaps and overlaps, but I failed to understand it.

155-159, here the authors sometimes talk about 'fossil collections' and sometimes about 'fossils', though my impression is that they always mean 'fossil collections' – please be consistent here and throughout the ms in general.

187-190, unclear how it was decided which 'fossils' (by which the authors presumably mean 'fossil collection site') are included in such a cluster and which aren't. It is important to make clear how the boundaries of these clusters are drawn.

235-243, this entire test is redundant: if you're adjusting the land-sea boundary in such a way that most inconsistencies are removed, of course does your 'consistency index' improve.

Paragraph 245-257, it is not clear to me what the authors are getting at with this paragraph. They discuss various biases and inhomogeneities of the fossil data, but neither do they apply a coherent test to the problem, nor do they reach any conclusion (except perhaps for "fewer fossils = fewer possibilities for adjustments", but this again is trivial).

245-249, as for lines 106-108, uneven entry of data is another potential bias.

249-251, "shorter time spans contain fewer fossils" – it might be interesting to systematically test the fossil dataset for this.

253, "biological organisms" – organisms are biological by definition

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264-267, here I was wondering how much of the “areal change” might relate to the gap filling and overlap removal that the authors have done to fit the plate reconstructions. In their lines 144-145 they wrote that the total areal variations ranged from 5.8 to -2.7%. A comparison of these values through time to the extent of area change through time (or something along these lines) might provide valuable insights here.

281ff, unless I've overlooked it, there is a step missing here in the explanation of the method. So far, the authors explained that in their adjustments, they exchanged 'land' for 'sea' and vice versa. But now they start discussing the quantification of different habitat types (shallow vs. deep sea, mountains vs. low lands etc.). Does this mean that when the land-sea boundary was shifted, for example, the 'new sea area' was assigned the habitat type of the fossil collection that caused the change? For example, has an area previously classified as 'mountain' sometimes been replaced by 'shallow marine' and sometimes by 'deep marine'? If so, this needs to be explained in the Methods section.

310ff, this whole paragraph seems redundant. It is pretty obvious to any earth scientist that continental flooding and eustatic sealevel changes are linked. Not only is it obvious that eustatic sealevel changes cause continental flooding (what else should it be?); to make matters worse, the eustatic sealevel curves are inferred from the continental flooding history as recorded in the sedimentary record so you might be looking at circularity here.

332, the difference between 27.7% and 27.5% isn't really great, isn't it? The authors should be a little more cautious about the errors in their own model. Could this difference of 0.2% again result from their gap filling procedure? Or could it be related to the inconsistencies in their 'improved paleogeographies'? In their lines 238-241 they write that even their 'improved paleogeographies' are still 3-5% inconsistent, which is a lot more than the 0.2% difference mentioned above. I recommend that the authors assess these inherent errors in their model (gap filling and 'consistency' index) and then discuss only variations that exceed those errors.

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341, 3% of the world's continental area has disappeared in the Neogene? Where did it go?

350-351, the abbreviation CGM is not explained (and perhaps not necessary?)

363, I find it dubious to 'confirm that Sr isotope ratios have a good correlation with emerged land areas' when there is no such correlation in the Paleozoic. Doesn't this rather indicate that there may be something fundamentally wrong with this correlation? I have no solution to the problem, but it seems more scientifically to me to point out such inconsistencies rather than to uncritically reiterate some lukewarm 'conventional wisdom'.

366ff, the 'Conclusions' nicely sum up the good parts and the problems of this study. The first paragraph outlines the good part, the flexible, digital plate model that could surely be of use for a wide range of earth scientists. The second paragraph discusses the redundant correlation between emerged land and eustatic sealevel changes, and the third paragraph again 'confirms' a correlation between Sr isotopes and emerged land, which apparently doesn't exist in the Paleozoic.

Table 1. why is this awkward Sloss 1988 timetable used? As far as I can tell, it applies to the US only, and connecting it to the accepted ICS and GSA timescales and to the periods, series and stages that have been used by geologists for more than 100 years is confusing. Avoid this, it is of no use for geologists and paleontologists.

Table 2, I had difficulties relating this table to what's written in the manuscript. The table distinguishes three paleogeographies (shallow marine, landmass/mountain, ice sheet), whereas in the text and fig 8 five distinctions are made (shallow marine, deep marine, land masses, mountains, ice sheets). Please be consistent here.

Figure 5. colors and shapes are not explained; perhaps refer to fig. 4? And I presume you mean “fossil collection sites” rather than “fossils”? I don't see any fossils in this figure.

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