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Interactive comment on "Amelioration of marine environments at the Smithian–Spathian boundary, Early Triassic" *by* L. Zhang et al.

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1) This manuscript reports stable isotope and elemental records across the Smithian-Spathian boundary from shallow carbonate platform section of South China. Main arguments are (1) sedimentary flux decreased during Smithian-Spathian transition, (2) redox environment was oxic to suboxic and not anoxic, and isotope ratios of carbon and sulfur of carbonate vary in inverse relationship. Their data and discussions are worth. But, I would like the authors to improve their discussions.

Response: We would characterize our main arguments somewhat differently than here expressed, but thanks for the reviewer's assessment of our contribution as "worth(y)".

Action: None requested.

2) First, authors suggest sedimentary flux variations across Smithian-Spathian focusing on clay and carbonates. Probably they calculated the sedimentation rates using thickness of lithologic column, elemental ratios such as Th/Th*, and absolute age referred from previous reported radiometric ages. But as their calculation process is not shown in the manuscript, readers can not evaluate their calculation results. I ask authors to explain their methods of sedimentary fluxes estimations including calculation formula, assumed ground with some literatures. Further, authors should mention that most of the absolute age numbers have some error ranges. So, they should discuss uncertainness of absolute numbers of their calculated fluxes and/or their estimations are maximum or minimum estimate. Similar points are found in their conodont occurrence ranges in Figure 1. They can not place Nv. pindingshanensis zone at the base of Bed 14 of the study section because of lack of fossil occurrences. I recommend authors to show such fossil barren horizon as spaces (no color in Figure for example) or "transitional zone" in the Figure.

Response: The reviewer discusses two issues here. The first relates to the methodology of calculation of sedimentation rates and sediment fluxes, and the second to the presentation of conodont ranges in Figure 1.

The methodology of calculating sediment fluxes is described concisely in the caption of Figure 3. More detailed algorithms were presented in Algeo and Twitchett (2010), which is cited in the same caption. There is no need to repeat this material from that earlier publication, to which the reader can refer if interested.

The conodont zonation for the West Pingdingshan reference section in Figure 1 is well established (Zhao et al., 2007). The conodont zonation that is shown for Shitouzhai is not based on the limited (n = 4) conodont identifications of the present study. Rather, it is a "model" zonation scheme based on the detailed C-isotope correlations shown in Figure 2.

Action: We have added the citation to Zhao et al. (2007) and have clarified these points in the caption of Figure 1.

3) Second, the authors discussed redox condition of the Shitouzhai section using multi elemental proxies. (1) In the first sentence of this discussion part, they argue that redoxsensitive elements of Mo, U, and V are low. But we can not find their data in any tables, despite authors refered "appendix Tables" in sentence (Line 302). So their criteria of "low in all samples " are uncertain. (2) Authors should indicate their definition of low value. When doing so, enrichment factor (e.g. Tribovillard et al., 2006) would provide useful tool for comparison with each redox conditions. (3) Also, author's criteria of Mn enrichments is not clear. Relatively high Mn/Th in 20-37 m horizon are interpreted as evidence of sub-oxic conditions. But it is uncertain why interpretation as oxic condition can be ruled out. In this paragraph, they introduce Mn's pass way, "reducing deep water mass provide soluble Mn to neighbouring oxic water mass " and "Mn deposition occur in oxic-suboxic depositional condition". Using these facts and combination with other redox proxies, more organized explanations on redox environment are required.

Response: The reviewer discusses multiple issues, which we have numbered for greater clarity. (1-2) We agree with the reviewer's comments. (3) Typical detrital Mn/Th ratios are ∼55 (i.e., 600 ppm Mn/11 ppm Th for upper continental crust; McLennan, 2001). Two intervals in the Shitouzhai section have much higher Mn/Th ratios (∼1000-3000; Figure 4), which indicates strong authigenic enrichment of Mn. Strong authigenic Mn enrichment is a hallmark of carbonates deposited under suboxic conditions; we have cited multiple studies in Section 5.2 to support these interpretations.

Action: (1) This is an oversight on our part. We have added the missing data to Table C2. (2) We have clarified the comment of "low (concentrations of Mo, U and V) in all samples" by adding a statement that this means that there is little or no authigenic enrichment above the estimated detrital background concentrations of these elements. (3) None.

4) Third, authors discussed chemical weathering intensity using CIA (chemical index

of alternation). As previous researchers mentioned (examples are following), this elemental ratio can also vary reflecting changes in provenances of sediment. Authors might want to discuss on this possibility. Perhaps, effect of provenance variation could be discussed by Eu-anomaly and REE features.

Borges, J., Huh, Y., 2007. Petrography and chemistry of the bed sediments of the Red River in China and Vietnam: Provenance and chemical weathering. Sediment. Geol. 194, 155–168. doi:10.1016/j.sedgeo.2006.05.029

Price, J.R., Velbel, M. a, 2003. Chemical weathering indices applied to weathering profiles developed on heterogeneous felsic metamorphic parent rocks. Chem. Geol. 202, 397–416. doi:10.1016/j.chemgeo.2002.11.001

Response: CIA shows strong covariation with most detrital proxies, including Al ($r = +0.87$), total REE ($r = +0.81$), Th/Th* ($r = +0.81$), and LSR ($r = +0.93$), which is consistent with our argument that variation in CIA is related to changes in weathering intensity. CIA shows an insignificant correlation to Eu/Eu* ($r = -0.21$) and other REE ratios, which argues against a change in sediment provenance as an explanation for CIA variation at Shitouzhai.

Action: We have added a mention of sediment provenance changes as a possible control on CIA, but have also added the statistical arguments above in favor of our interpretation.

5) Fourth, they pointed significance of Smithian-Spathian Boundary as the turning point of oceanic structure from "hyper green house (they assume stratified ocean) to "over turning circulated ocean". But negative co-variation of dC and dS could already be recognized in late Smithian warm period. In fact, during negative trough of d13C in Bed 8-13, d34S increase, although resolution of d34S is not so high as authors mentioned. I doubt their argument of coincidence of transition of sulfur isotope profile and cooling trend. They need some explanations on this trend.

Response: We acknowledge that there is a degree of negative covariation between d13Ccarb and d34SCAS at Shitouzhai during the late Smithian warm interval (Figure 4). However, the range and rate of variation in d13Ccarb and d34SCAS during this interval are more limited than for the SSB proper. More data will be needed to fully assess d13Ccarb-d34SCAS relationships during the Smithian. At the SSB, however, there is a large and abrupt positive shift in d13Ccarb and negative shift in d34SCAS, leaving no doubt at all about the pattern of negative covariation in this interval.

Action: We have revised the discussion in this paragraph extensively to make note of the points raised by the reviewer, and to offer some alternative explanations.

6) Finally, I can not find this paper's contribution from the final section of discussion (5.3) on temperature, vegetation and Siberian trap volcanic activity. Authors should indicate the significance of sedimentary flux and features of oceanic condition in Smithian-Spathian interval, reviewing previous issues.

Response: We agree.

Action: We have integrated more fully the results and interpretations from Shitouzhai into the final section of the paper (Section 5.4. Causes and consequences of the SSB event).

7) Minor issues: Line 33: This paper can not discriminate the cause of Smithian - Spathian warm and following cooling condition.

Response: The sentence in question reads: "The ultimate cause of the SSB event is uncertain but may have been related to reduced intrusive magmatic activity in the Siberian Traps Large Igneous Province." Yes, the present study does not determine the cause of the SSB event, as noted here. We offer a bit of speculation at the end of the study, which is phrased accordingly.

Action: None.

8) Line 53: What is extreme environmental condition? temperature in concrete ?

Response: Thanks for this request.

Action: We have added specific temperature values to clarify what is meant by "extreme conditions".

9) Line 98: It is better to make an explanation of carbon isotope notations N3 in first.

Response: We had offered a brief explanation of this notation in the caption of Figure 2, as well as a citation to the source (Song et al., 2013) in both the text and the Figure 2 caption.

Action: We have clarified the meaning of this notation in the Figure 2 caption.

10) Line 100, 101,109; What are criteria of "grobally" "world-wide"? In fact, several sections support carbonate carbon isotope variations during Early Triassic.

Response: "Globally" is short-hand for "in coeval sections on multiple continents". We believe that this convention is generally understood.

Action: None.

11) Line 128: absolute age must have error ranges. "_252 Ma" means maximum assumption?

Response: The uncertainties for all reported radiometric ages were given in the original studies, which are cited in our paper. It is not essential to report age uncertainties here. The reader can check the original sources if interested in this information.

Action: None.

12) Line 189 and figure explanation of Fig.3: It is not enough for explanation of sedimentation flux calculations.

Response: The reviewer has already raised this issue in point #2 above.

Action: See response to point #2.

13) Line 202: Still needing explanation on N3, P3, and N4 by Song et al.

Response: The reviewer has already raised this issue in point #9 above.

Action: See response to point #9.

14) Line 324: suboxic trend is discussed by covariance of another redox indicator together.

Response: The intended meaning of the reviewer's comment is unclear.

Action: We are unable to respond.

15) In Figure 5-B, ammonite or benthic foraminifera (?) is drawn on the deep-seafloor. Ithink there are few evidence of that "bio-diveristy loss" of calcareous animals occurpelagic deep water region. This figure leads to misunderstanding. Biodiversity losswould be occurred on shallow platform at least.

Response: Figure 5 is schematic. We encourage the reviewer and readers not to read too much into placement of the ammonoid shell.

Action: None.

16) In Table S1, Mn/T should be “Mn/Th"

Response: None.

Action: Corrected.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C7876/2015/bgd-11-C7876-2015-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 11, 15361, 2014.