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Peer Review File

Prolonged $^{187}Os/^{188}Os$ excursion implies hydrothermal influence after the Chicxulub impact in the Gulf of Mexico

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I'm not a geochemist, but enjoyed reading this paper. The authors have provided very compelling evidence of hydrothermal activity at the Chicxulub site initiated by the massive force of the impact. What caught my attention in reading through the manuscript was the quantification of Phosphorous (P), a limiting nutrient in primary productivity. In my reading and assessment of Figures 2 and 6, the record of P concentration above the K/Pg boundary, and the declining values at 614 mbsf and further at 613.2 mbsf coincide with major assemblage changes in the calcareous plankton: calcareous nannofossils and planktic forams (see red rectangles added to these two figures on the annotated ms). Maybe this is coincidental, but maybe it suggests that recovery of high productivity after the extinction event at the Chicxulub site was in fact driven by this hydrothermal activity, the latter being the major premise of the paper. In other words, I wonder if there is biosphere recovery story here as well. There is some discussion about "hydrothermal supply and marine ecosystem change", but I would argue that there is some compelling parallels between the hydrothermal data presented here and the productivity record reconstructed by Lowery et al. 2021.

I have provided some editorial suggestions for the authors to consider. Awesome science; I recommend publishing with some consideration of the arguments I've advanced about hydrothermal activity driving the early recovery of productivity at the Chicxulub impact site.

Specific comments:
Line 358: is the limestone discolored, especially in the lower portion, or does it display a color change from the base to the top?

Line 405: and maybe facilitated the very high productivity documented by Lowery et al. 2021.

Line 406: red rectangle (~614-613.2 mbsf) = interval of fundamental changes in calcareous plankton assemblages (nannos and forams). P has a 2-step decline at the bottom and top of the red rectangle, and Mn flat-lines above the red rectangle at ~613.2 mbsf. I wonder if this decline in hydrothermal activity and decline in P facilitated the decline in productivity and observed changes in calc plankton assemblages. And it was the hydrothermal activity triggered by the impact that drove high productivity at this location, compared with the sharp decline observed at other locations. This scenario was not presented in Lowery et al. 2021, but may be impactful and relevant here in the context of how variable the productivity record (recovery of the biological pump) is across the globe as documented by a number of authors (e.g., Hull et al.; Bralower et al.).

Mark Leckie, Univ. of Massachusetts Amherst

Reviewer #2

(Remarks to the Author)
The K-Pg impact event caused an anomaly in the concentration of Os and a decrease in Os isotope ratios. The reason why it took ~700 kyrs for the isotope ratios to return to their pre-impact values is discussed in the article. While other HSEs, such as Ru and Ir, which have the same chemical properties, quickly returned to their pre-impact values, only Os took a longer time to recover. Also, the duration for the recovery was significantly longer than that of distal pelagic sites, where similar Os

isotopic variations were reported by other researchers. The authors carefully considered the reasons for the deviations, and one possible scenario is the continuous supply of unradiogenic Os by post-impact hydrothermal activity. This scenario seems plausible, because it is consistent with many of the geochemical records presented here, and is reinforced by consideration of previous studies. I do not reject this scenario, but I feel strongly that some reinforcement is needed to support the authors' conclusions.

Of all the HSEs in the hydrothermal scenario, only Os appears to have been supplied to Chicxulub and the Gulf of Mexico sites by hydrothermal activity. The other HSEs quickly returned to pre-impact levels after the impact, and the hydrothermalism did not cause any enrichment for the other HSEs. Why and how was only Os supplied by hydrothermal activity? The processes that produced selective enrichments of Os need to be discussed in terms of differences in their chemical behavior from the other HSEs.

In relation to the above comment, I am confused by the explanations for the difference in the post-impact decreasing patterns of Os and Ir, which are not consistent: in Line 322-330, the authors assumed that Os was supplied from airborne microscopic dust and impact vapor condensates, which partially dissolved into seawater, and that the subsequent weathering of these materials caused the different supply process for Os and Ir. That is, the processes in the atmosphere and oceans, and the subsequent weathering are the cause for selective supply of Os. On the other hand, (Line 343 and later) the authors also argues that Os was supplied by hydrothermal fluids for ~700 kyrs after the impact. How are these two different scenarios related? Which is more plausible should be explained.

Related to the above comment, I am not sure what the authors want to say in the sentence (Line 325-328) “In contrast, lager fragments of melt droplets or sheets enriched in projectile components are distributed and deposited concentrically around the impact site, possibly with larger amounts of downrange direction to the southwest”. Are they trying to explain the process of Ir supply? If so, a more careful explanation is needed as to what in this process leads to the supply of “negligible amounts of Ir” in the next sentence.

In the discussion in Line 320-330, it seems that the supply process of Os and other HSEs (represented by Ir) during the post-impact period was different. If so, does this mean that Os and other HSEs are contained in different mineral phases? The consideration and verification of what mineral phase each of HSEs is contained in is needed. My greatest interest is that the selective dissolution of the mineral phases by diagenesis, especially the dissolution of Os-bearing mineral, and the release and remobilization of Os, may explain the observed depth profiles of Os isotopic ratios and Os concentrations presented in this paper. The authors discuss the possibility of remobilization and reworking of HSEs (Lines 244-250). They suggested the possibility of post-depositional contamination of fine meteoritic dust particles, but other possibilities were not discussed. The possibility of selective dissolution and remobilization of Os-bearing mineral phases should be clearly rejected with evidence.

The main scientific question of this study, whether there is versatility of Os isotope clocks between the distal and proximal sites (Line 39-40), has not been finally judged. The authors' conclusion can be read as a denial of versatility. This point should be clearly stated in the concluding remarks.

Specific comments
Line 162, Caption of Fig. 2 “HSE concentrations stratigraphy” should be re-considered.

Fig. 2, It may be better to add an axis that shows numerical ages (Ma).

Version 1:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
I have read the revised paper, and find that most of the problems have been corrected and improved.I concur in accepting this manuscript in this version.

Response to reviewers

We appreciate the time and effort that you dedicated to providing feedback on our manuscript and are grateful for the insightful comments on and valuable improvements to our paper. We have incorporated/addressed all the suggestions/comments. Please see below for a point-by-point response to the comments and concerns. The comments are in black, and the responses are in blue.

Reviewer #1 (Remarks to the Author):

I'm not a geochemist, but enjoyed reading this paper. The authors have provided very compelling evidence of hydrothermal activity at the Chicxulub site initiated by the massive force of the impact. What caught my attention in reading through the manuscript was the quantification of Phosphorous (P), a limiting nutrient in primary productivity. In my reading and assessment of Figures 2 and 6, the record of P concentration above the K/Pg boundary, and the declining values at 614 mbsf and further at 613.2 mbsf coincide with major assemblage changes in the calcareous plankton: calcareous nannofossils and planktic forams (see red rectangles added to these two figures on the annotated ms). Maybe this is coincidental, but maybe it suggests that recovery of high productivity after the extinction event at the Chicxulub site was in fact driven by this hydrothermal activity, the latter being the major premise of the paper. In other words, I wonder if there is biosphere recovery story here as well. There is some discussion about "hydrothermal supply and marine ecosystem change", but I would argue that there is some compelling parallels between the hydrothermal data presented here and the productivity record reconstructed by Lowery et al. 2021.

We thank the reviewer for providing useful comments and suggestions to improve our manuscript. We agree with your suggestions, and we have added the discussion of the linkage between hydrothermal activity and recovery of high productivity within the Chicxulub impact basin in the revised manuscript. Please refer our responses to your specific comments below.

I have provided some editorial suggestions for the authors to consider. Awesome science; I recommend publishing with some consideration of the arguments I've advanced about hydrothermal activity driving the early recovery of productivity at the Chicxulub impact site.

We thank the reviewer for his constructive comments and suggestions. We have added a discussion of hydrothermal activity driving the early recovery of high productivity within the impact basin in the revised manuscript (pages 29-30, lines 437-469), referring to Bralower et al. (2020; *AGU advances*). We have also revised our manuscript following your editorial suggestions in the attached file.

Specific comments:
Line 358: is the limestone discolored, especially in the lower portion, or does it display a color change from the base to the top?

There is no color change from the base to the top of the section at Site M0077. Therefore, we revised manuscript (pages 24-25, lines 368-370) as follows:

"Although early Paleocene limestones from Site M0077 do not show a significant change in color (Supplementary Fig. S2), a strong positive correlation between Os/Al and Mn/Al (R^2 = 0.90) was observed for our data during the delayed recovery of $^{187}Os/^{188}Os$ values (614.32 to 616.57 mbsf)."

Line 405: and maybe facilitated the very high productivity documented by Lowery et al. 2021.

We have added this possibility in the revised manuscript (pages 29-30, lines 437-469). Please also refer to the reply to the next comment.

Line 406 (Fig. 6): red rectangle (~614-613.2 mbsf) = interval of fundamental changes in calcareous plankton assemblages (nannos and forams). P has a 2-step decline at the bottom and top of the red rectangle, and Mn flat-lines above the red rectangle at ~613.2 mbsf. I wonder if this decline in hydrothermal activity and decline in P facilitated the decline in productivity and observed changes in calc plankton assemblages. And it was the hydrothermal activity triggered by the impact that drove high productivity at this location, compared with the sharp decline observed at other locations. This scenario was not presented in Lowery et al. 2021, but may be impactful and relevant here in the context of how variable the productivity record (recovery of the biological pump) is across the globe as documented by a number of authors (e.g., Hull et al.; Bralower et al.).

Regarding the relationship between hydrothermal activity and the early recovery of productivity at the Chicxulub impact site, we have revised the manuscript (pages 29-30, lines 437-446) in accordance with the reviewer's comments as follows:

"Interestingly, the time scale for complete recovery from the impact induced Os isotope excursion coincides with a transition in the marine ecosystem from eutrophic to oligotrophic conditions (Fig. 6e, f). … Indeed, the declining pattern of P concentration in early Paleocene limestone at Site M0077 at 614 mbsf and further at 613.2 mbsf coincides with major assemblage changes in the calcareous nannoplankton and planktic foraminifera trophic group (Fig. 6c, e, f). While this timing may be coincidental, these covariations suggest that the hydrothermal activity in the impact basin led to large-scale shifts in the dominant nannoplankton and planktic foraminiferal trophic groups, and that the hydrothermal supply of phosphorus, a limiting nutrient in primary productivity facilitated the early recovery of diverse life, as speculated by Ref.[17].
Our study infers that there are some intimate relationships between the impact-induced hydrothermal activity and the biosphere recovery within the Chicxulub impact basin. However, in order to test the scenario that hydrothermal supply of nutrients and energy was a key driver for changes in the biota of the marine surface environment, it is necessary to clarify how these nutrients were supplied by the hydrothermal system and entered the euphotic zone to drive enhanced primary production. Because there are no large-scale hydrothermal systems in the oceans today comparable to that formed by the Chicxulub impact, sedimentary records from variable sites in nearby oceans are critical to examining the extents of hydrothermal effects on the marine surface environment. For example, there is the problem of why similar trends in export productivity (e.g., Ba/Ti ratio) are observed in the Caribbean Sea[63], far from the hydrothermal system, which cannot be explained if we assume that the supply of nutrients from the hydrothermal system was limited to the Gulf of Mexico. Thus, further research on Os isotope and other geochemical profiles in different sites such as the Caribbean Sea and the Pacific Ocean are clearly required to confirm the versatility of the Os isotope clocks and to reconstruct the recovery patterns of the marine ecosystem."

Comments and suggestions in the attached file:
Line 1: and driver of high productivity??

We added a description of the possibility of hydrothermal influence as a driver of high productivity within the impact basin, but we kept the title of our manuscript because the title is required to be 15 words or fewer.

Line 42: biogeochronological scales?

In our manuscript, we used the term “biochronological scales” based on planktic foraminifera and astrochronological calibrations following Arenillas et al. (2022).

Line 57: of the impact site?

We modified “for the impact site” to “of the impact site” in the revised manuscript (line 57).

Line 110: ~~the~~

We modified “based on the P zones” to “based on P zones” in the revised manuscript (line 110).

Line 111: ~~the~~

We modified “relies on the Dan zones” to “relies on Dan zones” in the revised manuscript (line 111).

Line 121: biozone~~s~~

We modified “biozones” to “biozone” in the revised manuscript (line 121).

Line 123: 0.125 cm/kyr

We modified “0.125cm/kyr” to “0.125 cm kyr^{-1}” in the revised manuscript (lines 122-123).

Line 125: ~~the~~

We modified “for the Biozone Dan3a” to “for Biozone Dan3a” in the revised manuscript (line 125). Following this suggestion, we also corrected “the Biozone” to “Biozone” in lines 126, 435, and 440 of the revised manuscript.

Line 133: ~~the~~

We modified “in the lower part of the Biozone Dan4c” to “in the lower part of Biozone Dan4c” in the revised manuscript (line 133).

Line 134: ~~the~~

We modified “In the Biozone Dan4c” to “In Biozone Dan4c” in the revised manuscript (line 134).

Line 146: ~~the~~

We modified “of the northeastern Mexico” to “of northeastern Mexico” in the revised manuscript (lines 146-147).

Line 150: ~~the~~

We modified “in the northeastern Mexico” to “in northeastern Mexico” in the revised manuscript (line 150).

Line 153: ~~the~~

We modified “in the southeastern Mexico” to “in southeastern Mexico” in the revised manuscript (line 153).

Line 156: ~~the~~

We modified “in the southeastern Mexico” to “in southeastern Mexico” in the revised manuscript (line 156).

Fig. 2: red rectangle = interval of fundamental changes in calcareous plankton composition (nannos and forams).

Following your comment, we modified the manuscript as described in our response to your specific comment.

Line 165: A21 (Arenillas et al., 2021) and W11 (Wade et al., 2011)

We presented references for planktic foraminiferal biozones in the revised manuscript (page 10, line 165) as follows:

“Planktic foraminiferal zonations A21 and W11 are from Ref.[29] and Ref.[31], respectively.”

Line 166: please define UCC here

We modified “UCC” to “upper continental crust (UCC)” in the revised manuscript (line 166).

Line 192: In other Gulf of Mexico sections,

We modified “In other sections of Gulf of Mexico” to “In other Gulf of Mexico sections” in the revised manuscript (line 192).

Line 203: please define CCU here

We modified “above the CCU” to “above the K/Pg impact-induced Complex Clastic Unit (CCU)” in the revised manuscript (line 203).

Lines 216, 219, and 229: Hole 1262B

We modified “ODP Site 1262B” to “ODP Hole 1262B” in the revised manuscript (lines 217, 220, and 229). We also corrected “ODP Site 1262B” to “ODP Hole 1262B” in the revised Fig. 1 and Fig. 5.

Line 217: ODP Hole 690C (Maud Rise, Southern Ocean)

We modified “ODP Hole 690C (Southern Ocean)” to “ODP Hole 690C (Maud Rise, Southern Ocean)” in the revised manuscript (line 217).

Fig.5: Hole 690C is in the Southern Ocean (not South Ocean)

We modified “ODP Hole 690C (South Ocean)” to “ODP Hole 690C (Southern Ocean)” in the revised Fig. 5.

Line 427: please see comments in Figure 6 related to the red rectangle...

Please see our response to your specific comment for Fig. 6.

Line 472: 65.983 Ma

We modified “65,983 Ma” to “65.983 Ma” in the revised manuscript (line 487).

Mark Leckie, Univ. of Massachusetts Amherst

Reviewer #2 (Remarks to the Author):

The K-Pg impact event caused an anomaly in the concentration of Os and a decrease in Os isotope ratios. The reason why it took ~700 kyrs for the isotope ratios to return to their pre-impact values is discussed in the article. While other HSEs, such as Ru and Ir, which have the same chemical properties, quickly returned to their pre-impact values, only Os took a longer time to recover. Also, the duration for the recovery was significantly longer than that of distal pelagic sites, where similar Os isotopic variations were reported by other researchers. The authors carefully considered the reasons for the deviations, and one possible scenario is the continuous supply of unradiogenic Os by post-impact hydrothermal activity. This scenario seems plausible, because it is consistent with many of the geochemical records presented here, and is reinforced by consideration of previous studies. I do not reject this scenario, but I feel strongly that some reinforcement is needed to support the authors' conclusions.

We thank the reviewer for very useful and constructive comments and suggestions to improve our manuscript. Regarding the chemical properties of HSEs noted by the reviewer, it is known that Os, Ir, and Ru have similar chemical properties in high-temperature processes such as mantle melting (>1300˚C). However, in low-temperature processes occurring at the Earth's surface environment, they show quite different chemical behavior. For example, the estimated residence times of Os and Ir in the modern ocean are ~40 kyr and ~2 kyr, respectively (Levasseur et al., 1999; Lee et al., 2003). There is thus a clear fractionation of Os from Ir in precipitation out of seawater in sedimentary sections. Since our Os and Ir profiles are consistent with their residence times, we have added the below sentence in the revised manuscript (pages 18-19, lines 261-264):

"Indeed, distinct declining trends between Os and Ir abundances throughout the Paleocene sediments are broadly consistent with their contrasting residence times estimated from the modern ocean of ~40 kyr for Os and ~2 kyr for Ir[23,36] (Figs. 2 and 3), indicating that the hydrogenous component dominates the sedimentary budget."

Of all the HSEs in the hydrothermal scenario, only Os appears to have been supplied to Chicxulub and the Gulf of Mexico sites by hydrothermal activity. The other HSEs quickly returned to pre-impact levels after the impact, and the hydrothermalism did not cause any enrichment for the other HSEs. Why and how was only Os supplied by hydrothermal activity? The processes that produced selective enrichments of Os need to be discussed in terms of differences in their chemical behavior from the other HSEs.

In the original manuscript, we invoked that alteration and weathering processes resulted in the selective enrichment of Os, since Os is known to be highly mobile during these processes relative to Ir and Ru (e.g., Wallace et al., 1990; Jaffe et al., 2002; Wimpenny et al., 2007; Aiglsperger et al., 2021; Feignon et al., 2022). Therefore, we have modified the descriptions in the revised manuscript (page 22, lines 322-323 and page 27, lines 409-410) as follows:

"Considering that Os is known to be highly mobile during alteration and weathering processes[44,45], there are two possible mechanisms to acquire a large but continuous input of meteoritic Os with unradiogenic composition into the Gulf of Mexico: ..."

"These deviations can be attributed to the selective hydrothermal remobilization of Os but not for Ir."

In relation to the above comment, I am confused by the explanations for the difference in the post-impact decreasing patterns of Os and Ir, which are not consistent: in Line 322-330, the authors assumed that Os was supplied from airborne microscopic dust and impact vapor condensates, which partially dissolved into seawater, and that the subsequent weathering of these materials caused the different supply process for Os and Ir. That is, the processes in the atmosphere and oceans, and the subsequent weathering are the cause for selective supply of Os. On the other hand, (Line 343 and later) the authors also argues that Os was supplied by hydrothermal fluids for ~700 kyrs after the impact. How are these two different scenarios related? Which is more plausible should be explained.

In the original manuscript, we do not claim that airborne microscopic dust and impact vapor condensates enriched in HSEs were the source for the selective supply of unradiogenic Os only to the Gulf of Mexico seawater. This is simply because they were transported globally after the K/Pg impact. In addition, we mentioned that the stratigraphic variations in HSE concentrations for the Gulf of Mexico sites do not support the idea that the reduced Os isotope ratios during ~700 kyr were caused by reworking of the K/Pg deposits enriched in chondritic dust particles. As a result, we hypothesized that Os was supplied from larger fragments of melt droplets deposited concentrically around the impact site, or from the melt sheets or melt-rock deposits formed within the impact crater. For the process of selective enrichment of Os, please refer to the reply to the comment above.

For possible mechanisms for the continuous supply of unradiogenic Os into the Gulf of Mexico, we discussed (1) weathering of impact ejecta deposited on the continents, or (2) venting of hydrothermal fluids beneath the Chicxulub impact basin. We concluded that the first mechanism was not significant because the impact melt rocks enriched in HSE have only been discovered within from the Chicxulub crater (Koeberl et al., 1994). Furthermore, the larger $^{187}Os/^{188}Os$ excursion detected in the limestones from Site M0077 (within the basin) compared to those from other Mexican sites (nearby continents) seems likely the result of regional differences in the effects of hydrothermal fluids generated beneath the impact basin. The explanations for this interpretation were not clear in the original manuscript, we have modified and added the descriptions in the revised manuscript (pages 22-23, lines 333-346) as follows:

"Although their compositions and constituent minerals/phases remain unknown, it is possible that the incongruent weathering of these materials could have released significant and negligible amounts of unradiogenic Os and Ir, respectively. If such Os were transported with some time lag by riverine flow to the Gulf of Mexico[47], this could explain the delayed recovery of $^{187}Os/^{188}Os$ after the K/Pg impact observed in our studied sections. However, this mechanism may not be effective because the impact melt rocks enriched in HSE have rarely been observed on the continents surrounding the impact site, in contrast to their occurrence within the Chicxulub impact basin[45,48]. Furthermore, our data demonstrate that the Mexican sites distributed on the continental margin show smaller $^{187}Os/^{188}Os$ excursions than that of Site M0077, located in the impact basin (Fig. 5b). In other words, the largest $^{187}Os/^{188}Os$ excursion for Site M0077 among the study sites likely indicates that the crater hydrothermal mechanism is the dominant process for introducing the unradiogenic Os into the Gulf of Mexico seawater."

Related to the above comment, I am not sure what the authors want to say in the sentence (Line 325-328) "In contrast, lager fragments of melt droplets or sheets enriched in projectile components are distributed and deposited concentrically around the impact site, possibly with larger amounts of downrange direction to the southwest". Are they trying to explain the process of Ir supply? If so, a more careful explanation is needed as to what in this process leads to the supply of "negligible amounts of Ir" in the next sentence.

Large fragments of melt droplets are estimated to have been deposited around the impact site (Artemieva and Morgan, 2020). Thus, greater amounts of HSE would have been deposited proximally on the continental shelf and central Mexican continental block, and Os leached out from these sources could have been delivered to the

Gulf of Mexico seawater. However, as described above, HSE-enriched impact melt rocks have rarely been observed in the K/Pg boundary sections of nearby continents.

We have added the explanation for the selective enrichment of Os in the discussion of the hydrothermal process. Please refer to the reply to your comment above.

In the discussion in Line 320-330, it seems that the supply process of Os and other HSEs (represented by Ir) during the post-impact period was different. If so, does this mean that Os and other HSEs are contained in different mineral phases? The consideration and verification of what mineral phase each of HSEs is contained in is needed. My greatest interest is that the selective dissolution of the mineral phases by diagenesis, especially the dissolution of Os-bearing mineral, and the release and remobilization of Os, may explain the observed depth profiles of Os isotopic ratios and Os concentrations presented in this paper. The authors discuss the possibility of remobilization and reworking of HSEs (Lines 244-250). They suggested the possibility of post-depositional contamination of fine meteoritic dust particles, but other possibilities were not discussed. The possibility of selective dissolution and remobilization of Os-bearing mineral phases should be clearly rejected with evidence.

It is difficult to hypothesize which mineral phases host the HSE in impact melt rocks because HSE-enriched melt rock samples are rarely recovered from the K-Pg boundary sites. However, it should be emphasized here that selective leaching of Os from silicate matrix is common process as described in Feignon et al. (2022). Therefore, this question is beyond the scope of this paper.

The main scientific question of this study, whether there is versatility of Os isotope clocks between the distal and proximal sites (Line 39-40), has not been finally judged. The authors' conclusion can be read as a denial of versatility. This point should be clearly stated in the concluding remarks.

To assess the versatility of the Os isotope clocks between the distal and proximal sites, we need more data for stratigraphic variations of $^{187}Os/^{188}Os$ ratios after the K/Pg impact in other sites such as the Caribbean Sea and the Pacific Ocean. Therefore, we have added the following sentence in the revised manuscript (page 30, lines 466-469):

"Thus, further research on Os isotope and other geochemical profiles in different sites such as the Caribbean Sea and the Pacific Ocean are clearly required to confirm the versatility of the Os isotope clocks and to reconstruct the recovery patterns of the marine ecosystem."

Specific comments
Line 162, Caption of Fig. 2 "HSE concentrations stratigraphy" should be re-considered.

We modified the title of Fig. 2 to "Stratigraphic profiles of Re-Os isotopes, HSE concentrations, and Os/Ir ratios of the Paleocene white limestone at Site M0077." in the revised manuscript (lines 161-162).

Fig. 2, It may be better to add an axis that shows numerical ages (Ma).

We added an axis showing numerical ages (Ma) in the revised Fig. 2 based on new biochronological scales (Arenillas et al., 2021; Gilabert et al., 2022).

Response to reviewers

Please see below for a response to the comments. The comments are in black, and the responses are in blue.

Reviewer #2 (Remarks to the Author):

I have read the revised paper, and find that most of the problems have been corrected and improved. I concur in accepting this manuscript in this version.

Once again, we appreciate the time you dedicated to reviewing our revised manuscript. We also thank you for your concurrence with the acceptance of the revised version of our manuscript.