Reply to comments by reviewer J. Pálfy (reviewer #2) on the manuscript "Cross-continental age calibration of the Jurassic/Cretaceous boundary"

Comments by the reviewer have been copied and pasted in the *italic* blue font, and the answers are found immediately below in regular black font. The comments are in order of appearance in the reviewer's comments. We have taken the liberty of numbering the comments from 2.1 through 2.20. Subsequently, replies to technical comments are in order of appearance in the reviewer's comments, also in the same fashion as the generals comments. References are found here right after the "technical corrections" section.

We would like to thank J. Pálfy for accepting to review our manuscript kindly. His comments were incredibly insightful and will undoubtedly improve the quality of this manuscript.

Firstly, we appreciate the reviewer's enthusiasm towards our manuscript and also like to say that we share the reviewer's opinion that the JKB is a "hot topic" and we acknowledge that we should have made this point in the manuscript. As such, we feel that hot topics are better communicated shortly and concisely, in a way that is more appealing to the general public. We believe that other Phanerozoic system boundaries have overshadowed the JKB. In this sense, it is essential to produce publications that are easily readable by the general public without too much unnecessary information, by removing excessive description and revision of readily available information that could otherwise be easily accessed in previous publications. Additionally, concise writing allows for a faster turnaround time during the reviewing process, which is a big attraction for submission. Therefore, we would like to answer a few of the reviewer's comments that directly related to the above statement.

General comments

2.1) "… I take several issues with the interpretation, and may suggest guidance for a revised version which could better avoid the pitfalls of confusing regional and global biostratigraphic correlation issues. Instead, a refocused discussion should emphasize the

obvious significance of the radioisotopic dates in highlighting problems and contradictions in biostratigraphy."

This is a significant point, and it highlights the importance of dating the stratigraphic record using high-precision geochronology to unravel its subtle nuances. If we have interpreted the reviewer's advice correctly, our ages clearly show that assuming time-equivalency of biostratigraphic zones can lead to erroneous correlations regarding the numerical ages of FAD and LOD. Possibly, this difference can arise from the migratory rates of these species resulting in the diachroneity of FDA and LOD. This is an interesting point to explore and discuss in the revised manuscript and will be incorporated into the revised version. Nevertheless, we feel that the essential aspect of our data is how younger the age of the JKB is with regards to the long-lasting age of 145 Ma. This is the most crucial contribution of the manuscript, and the discussion around how the age of the JKB in both sections favorably agree is still central to the manuscript.

Specific comments

2.2) The paper needs a proper "Geological and stratigraphic setting" chapter to augment and replace the "Studied areas" in the current version. Formation names, i.e. the bare bone lithostratigraphy should be complemented with brief characterization of basin evolution and depositional environments, to provide context for assessment of stratigraphic completeness and sedimentation rates in the section, the latter being crucial in the authors' arguments in comparing the JKB age of different sections.

In the "Studies areas" chapter, we chose simply to give a brief description of where the studies sections are located and cite important publications relevant to where the sections are exposed. There are numerous publications on the tectonic architecture and basinal evolution where the sections are that are cited in the manuscript. As it stands, the manuscript is 4626 words long, which we feel is an adequate length for a publication. If we were to expand the "Studies areas" chapter with a detailed "Geological and stratigraphic setting" chapter, it would increase the manuscript to another 800-1000 words. Even then, it would not do justice to fully review the geological setting of both geological settings within 1000 words (e.g., 500 words each basin). The

reviewer claims that such an expansion of the regional geology would be useful to understand better the sedimentation rate in Mazatepec, which is an integral part of our discussion. However, we make it pretty clear in the manuscript that the sedimentation rate in the Mazatepec section is **unknown**, and we further use both a low and high sedimentation rate to back-calculate the age of the JKB in the section. Even with a thorough knowledge of the sedimentological and stratigraphical background, there is no hard evidence for the rate of sedimentation rate in the Pimienta and Tamaulipas formations. Ultimately, this would inevitably leave us with a subjective choice of sedimentation rate based on the depositional environment and sedimentological structures present. Moreover, we also make the case that the choice of sedimentation rate is not that important. Nevertheless, we would not oppose slightly expanding the "Studies areas" chapter, or giving it a new title if the reviewer feels adamant about the subject. We leave this option to the discretion of the Handling Editor, because it influences the format with which publications in Solid Earth are communicated.

2.3) Care should be taken to ensure consistency in terminology and usage of biozones. Much biostratigraphic information is presented both in the text and in Fig. 4. However, it is not clear to the reader what, if any of these is new here, what is taken unchanged from the references cited, and what is revised from published sources

In the caption for figure 4, there is ample information on the information that is new and what is cited from other publications. We will try to make the figure 4 clearer at the request of the reviewer well as its caption.

2.4) Cases where there is controversy in either the zonal subdivision of sections or their correlation, based on ammonoids, calpionellids and nannofossils (e.g., between Riccardi 2015 and Vennari et al. 2014) and the stance of the authors should be more clearly stated.

Ammonoids: There is no discrepancy among the biozonation of Riccardi, (2015), the Vennari et al., (2014), and the present manuscript regarding the sequence and names of index species of each

biozone. It is worth to mention here that Riccardi explicitly states: "*There is no attempt to deal here with the precise definition of the Jurassic-Cretaceous limit, and therefore the use of terms such as "Tithonian," "Berriasian," "Upper/Late Jurassic" and "Lower/ Early Cretaceous have been kept to a minimum and is usually adopted when quoting other sources. It is considered that once biostratigraphic correlations are well-established definition of Stage and System boundaries will follow by convention*" (Riccardi 2015, p. 24).

Calpionellids: The data from this manuscript has been published by López-Martínez et al., (2013) for the Mexican section and López-Martínez et al., (2017) for the Argentine section.

Nannofossils: The data from this manuscript has been published by Vennari et al. (2014) for the Argentine section. The data presented here for the Mexican section is new, and a systematic paper is in preparation (Lescano et al. in prep.).

2.5) The reader might suspect that calcareous nannofossil occurrences are newly obtained as Supplementary Fig. 3 is promised to present them (p. 3, l. 26), but this figure is missing.

Yes. Unfortunately, we have not placed the Supplementary Figure 3 (distribution chart for the calcareous nannofossil species) in the Supplementary Materials as stated in p.3, l. 26. We apologize and promise to rectify.

2.6) Details of reporting of the error and age interpretation would be better placed in the main text's Methods chapter rather than in the Supplementary Material.

The detailed account of the geochronological data is intended for full disclosure of its meaning and interpretation; however, this would only be appealing to a specific subset of the geochronology community. The average reader, drawn by the interest of knowing the age of the JKB, in our opinion, would be distracted by an excessively detailed description of the geochronological U-Pb data in the main text. Moreover, this information is not further referred nor directly used in the discussion and conclusion chapters, i.e., the meaning of a depositional age for

the ash beds, number of grains selected for weighted means, etc. These are not information that is central to the discussion of the data and conclusions. This is why we decided to keep it in the Supplementary Materials. Nevertheless, we leave it at the discretion of the Handling Editor do choose what best fits the format of the journal because it would be an easy adjustment to make to the revised manuscript.

2.7) For the aimed global relevance in time scale studies, the most conservative error (i.e., that including the tracer calibration and decay constant errors) needs to be quoted and used for each U-Pb dates throughout the paper. This is typically still within 0.2 Ma, a commendable high-precision.

The reason high-precision ages are reported with three errors (as explained in the Supplementary Materials) is to allow for an appropriate propagation of errors when comparing different geochronological datasets that been acquired through different geochronological methods (e.g., $^{39}Ar/^{40}Ar$, U-Pb (SHRIMP, LA-ICP-MS)). In this manuscript, we do not directly compare datasets from other studies. We do, indeed, aim to challenge the JKB recommend age in the ICS is ~145 Ma, which mainly highlights the lack of precision and accuracy towards the JKB age. In any case, the JKB age in the ICS is based on the $^{39}Ar/^{40}Ar$ age of Mahoney et al. (2005). Nevertheless, our ages are so much younger than that of Mahoney et al. (2005), making precision, not such a big deal for the sake of challenging the ICS age. Hopefully, other sections that span the JKB will be dated in the future and most likely use U-Pb CA-ID-TIMS since it has become a gold-standard in dating the stratigraphic record. Therefore, how we quote precisely in the manuscript is not that big of a deal.

2.8) The chapter "Results and discussion" needs to be split into two, allowing results to be clearly separated from the interpretation.

In the same vein as the reply to comment 2.6, we wanted to make a concise manuscript. In this sense, we feel that the nitty-gritty dissection of the geochronological data should not be moved to a separate "Results" chapter in the main text. Instead, we describe the data along with the discussion, which in our opinion reads better and is not unusual in scientific communications. As

far as the Solid Earth's author guideline goes, it does not mandate that results be separated from the discussion. Moreover, we think that the lack of a specific "Results" chapter does not compromise any of the discussion or conclusions in the manuscript. Therefore, we thought it might be better to leave the results and discussion together. We believe that this comment is more of a personal preference of the reviewer than a weakness of the manuscript. Nevertheless, we leave it at the discretion of the Handling Editor do choose what best fits the format of the journal because it would be an easy adjustment to make to the revised manuscript.

2.9) Even though it is widely accepted that magnetostratigraphy is very useful for global correlation in the JKB interval, projecting the magnetozones identified in the Arroyo Loncoche section in the Neuquén Basin (Iglesia Llanos et al. 2017) introduces additional confusion (p. 5, l. 1-9, Fig. 4) to the already complex web of stratigraphic correlation of the three studied sections. The new results from Las Loicas do not appear to be closely correlatable with Arroyo Loncoche, Fig. 4 reveals that the placement of the JKB is offset by nearly one ammonoid zone, being near the base or at the top of the Substeueroceras koeneni zone, respectively. It would suffice to say that magnetostratigraphy of the Las Loicas section will be desirable to enhance the utility of the newly obtained U-Pb ages and clarify contentious biostratigraphic correlation issues.

In the manuscript, we do not project the magnetozones of the Arroyo Loncoche section to the Las Loicas or any other section. We merely attempt to correlate the JKB in the Arroyo Loncoche to the Las Loicas section using the Alpina Subzone and the M19.2n, which are the most compelling evidence for the JKB in either section. We admit that there is a mismatch between the ammonite zonations, which is clearly stated in the manuscript (p. 5, l. 8-9). Additionally, we also cited a discussion on the matter in López-Martínez et al., (2018). Nevertheless, the thickness of biozones changes as a function of facies, randomness of finding markers in the field, the latter hugely influenced by preservation, and paleogeographical position within a sedimentary basin. Therefore, although we do see that better understanding the mismatch between both sections as an incentive for future research, we do not, however, see this as a significant issue to be explained.

One needs to keep in mind that another principal aim of this manuscript is to try to show that the age of the JKB in ICS is too old. In an idealized case, one would find the age of the M19.2n and the base of the Calpionella alpina Subzone to be the same age (assuming these markers are exposed in different sections as is the case in this manuscript). This would require that both of these markers have a datable horizon very close by. However, in the real world, this scenario is quite hard to come by, and we need to try and reconcile the available data despite its shortcomings. In the context of trying to show that the ICS age of the JBK (145 Ma) is too old, the data from Las Loicas and Arroyo Loconche seems to be in reasonable agreement, in our opinion. That is, if we consider that the most trustworthy markers for the JKB are the M19.2n and the base of the Calpionella alpine Subzone, even with the mismatch of the ammonite zones between Las Loicas and Arroyo Loconche (which would be a couple 100 ka), the age markers for the JKB of these two sections would not be off by 5 Ma. **Furthermore, it is important to point out that in the absence of a reliable biostratigraphic framework, such as the case of Arroyo Loconche, magnetostratigraphy is just a floating scale (very important to bear in mind).** Therefore, from this perspective, even with the ambiguity in the correlation between these two sections, the age of the JKB at ~145 Ma is hard to reconcile. We do understand that the M19.2n in Arroyo Loconche might seem older than the base of the Calpionela alpine Subzone in Las Loicas when compared against the Substeueroceras koeneni biozone as a relative timescale. Nevertheless, this discrepancy would not allow, for instance, the interpretation that the age of the M19.2n in Arroyo Loconche to be as old as 145 Ma and the age of the Calpionella alpina Subzone in Las Loicas to be at ~140 Ma, which would be the alternative to invalidating our conclusion. Furthermore, our age in the *Virgatosphinctes andesensis* biozone (Early Tithonian) would certainly not allow this interpretation. In closing, the explanation above only exposes the how poorly constrained the current age of the JKB is, that even with a crude correlation (which is what is available at our disposable at this conjecture) the age of the JKB at 145 Ma seems implausible.

Additionally, in the manuscript, we cite many references that have also dated the JKB and found ages similar to ours. Furthermore, our goes for the base of the Berriasian are much easier to reconcile with the ages for the Early Cretaceous ages (see page 8, lines 10-10 in the manuscript). In closing, there is substantial evidence from different fields that point to an age of the JKB that is

much younger than in the ICS (We would also like to refer the reviewer to the reply on comment 1.2, i.e., in reply to reviewer #1).

Having said this, we realize that both reviewers took issue with our attempt to correlate the M19.2n in Arroyo Loconche and the base of the Alpina Subzone in Las Loicas in an attempt to build a more solid case for our age of the JKB. If our arguments remain unconvincing, we will not oppose removing entirely this from the discussion and figure 4. Hopefully, our explanation was satisfactory. We would, in this case, value comments and advice from the handling Editor on the matter for the revised manuscript.

2.10) Discussion on the age of the JKB in the Mazatepec section includes an assumption on the FAD of a nannofossil taxon, Nannoconus steinmannii minor, not actually found in the section (p. 5, l. 31 – p. 6, l. 3). Such speculation is best avoided.

Our consideration of the *N. Steinmannii* is speculative, very short. We certainly do not substantiate any conclusion on this comment. Nevertheless, the N. steinmannii defines the base of the biozone and is the main bioevent, and the others are defined as close and secondary with regards to this bioevent. Therefore, this was just to give the reader food for thought, as so to speak.

2.11) Beware of the lack of formal definition of base Tithonian. There is no agreed-upon GSSP decision yet, contrary to what is implied here (p. 6, l. 25). The attendant uncertainties of stage boundary placement and its correlation with the Andean sections make the time scale calibration use of La Yasera U-Pb date more problematic than admitted here.

Indeed, there is no agreement on the GSSP for the Kimmeridgian-Tithonian boundary. We will remove the sentence in brackets that suggested otherwise (p.6, l 25), and will make it clear that the KmTB is not formally defined.

2.12) The discussion on the duration of the Tithonian is interesting but contains a factual error and misses some further opportunities. The Geological Time Scale 2016 (Ogg et al. 2016) is misquoted, it assigns 150.8 Ma to the base of Tithonian Stage and 145.5 Ma to the JKB.

In the ICS chart 2018, the age of the KmTB is 152.1 ± 0.9 Ma. Please see http://www.stratigraphy.org/index.php/ics-chart-timescale. Additionally, in the compilation of Ogg et al., (2016), Chapter 12 – Jurassic, Figure 12.1 page 152, and Figure 12.4 page 157, the age quoted is 152.1 Ma for the base of the Tithonian.

2.13) It would be useful to compare two other, independent duration estimates. The Pacific M sequence of magnetic anomalies has long featured in time scale calibration. The recent work of Malinverno et al. (2012) (the MHTC12 scale) suggests 6 m.y. for the Tithonian, i.e., between magnetochrons M22An and M19n2n.

We thank the reviewer for pointing this out to us, and we will undoubtedly discuss and compare Malinverno et al., (2012) timescale for the Tithonian in the discussion, especially since it is very close to our estimate for the duration of the Tithonian.

2.14) The cyclostratigraphic analysis of Kietzmann et al. (2015; not cited by Lena et al.) identifies 10 long eccentricity cycles for almost the entire Tithonian, starting with the Virgatosphinctes mendozanus zone dated here at La Yesera, hence a duration of c. 4 m.y. The discussion should emphasize that the duration favored here is longer these previous estimates using other methods and offer possible reasons to explain the difference, perhaps considering biostratigraphic correlation issues.

There are two issues here: First, in Kietzmann et al., (2011) the Tithonian was more than 210 m thick in Arroyo Loncoche; then in Kietzmann et al., (2015) the Tithonian is reported as 195 m thick; and finally in Iglesia Llanos et al., (2017) the Tithonian was reported with less than 160 m. This makes more inadequate the ten long eccentricity cycles for almost the entire Tithonian. The second issue is that following Vennari et al. (2014) and Riccardi et al. (2015), and in the present manuscript, the andesensis (former mendozanus) zone is correlated with the Tethyan ammonite zones, which are above the base of the Tithonian, i.e. the hybonotum zone is not represented in Vaca Muerta Formation.

2.15) Perhaps my most important criticism and suggestion pertains to the projection of a sedimentation rate-based JKB from the Mexican Mazatepec section into Las Loicas in

Argentina. The authors can make a much stronger case and build a more logical argument by projecting the actual U-Pb date, expressing the stratigraphic height from the age-model calculation as ~28.5 m and note the mismatch in biostratigraphies. Reading from Fig. 4, beds of the same numeric age thus appear assigned to nannofossil zone NJK-B vs. high in NJK-D, to calpionellid Crassicollaria zone vs. Calpionella zone (and its third subzone, the Elliptica subzone, and ultimately to lower Berriasian vs. upper Tithonian at Las Loicas and at Mazatepec, respectively. The discussion could thus be refocused to use the newly obtained high-precision and high-resolution U-Pb age framework to highlight biostratigraphic correlation issues, most likely due to diachronous FAD-LADs of certain key taxa.

We have partially addressed this inquiry in question 2.1. Nevertheless, we are happy with this comment because it further substantiates our arguments, especially for a JKB interval. We agree with J. Pàlfy that the mismatch in the age of the FAD-LAD in Las Loicas and Mazapetec is clear evidence that assuming age-equivalency of markers and stage boundaries is problematic when working at the sub-100 ka level and highlights the importance of high-precision geochronology to the stratigraphic record. Furthermore, in the context of the JKB, it stresses the importance of leaving **the age** of the JKB confined to an interval (we further explore this in reply to question 2.16). We welcome this comment and will surely incorporate this into the revised manuscript because we see this as an essential implication from our data.

2.15) To strengthen the argument for potential problems in biostratigraphic correlation, the authors might comment on the discrepancy of ammonoid-based correlation, and striking differences of thickness of zones in different sections even within the Vaca Muerta Fm. (e.g. Argenticeras noduliferum zone: ~27 m in Las Loicas vs. 5 m in La Yesera section).

There are important facies and thickness changes between Las Loicas and La Yesera sections due to their different paleogeographic positions within the Neuquén Basin. La Yesera section is further east (see paleogeographic sections for example in Kietzmann et al. (2015).

2.16) It the "Global correlation" chapter, the suggestion of understanding the JKB as an interval

(p. 8, l. 1-10) is conceptually flawed and needs to be rephrased. By definition, the JKB boundary (as any other chronostratigraphic boundary) is a time line. It does indeed carry an uncertainty of our numeric calibration but it cannot be equated with an actual time interval in which different "boundary events" took place.

The reviewer may not have understood what we meant by the term Jurassic/Cretaceous interval. We want to make it clear the JKB is not tantamount to JKB interval; in other words, they are not the same thing. We did not suggest that the JKB be understood as an interval (at least that was not our intention), but rather the age of the JKB be left within a bracketed interval, thus the idea of the JKB interval. This mainly stems from the fact that the age of the JKB in both sections do not overlap within our analytical uncertainty, and are offset by ~670 ka (± 335 ka). Furthermore, as pointed out by the reviewer in comment 2.1, the markers are offset in an age which, in our opinion, only builds a stronger case to leave the age of the JKB confined to an interval, the JKB interval. In other words, what we propose here is that the interval constrained by our geochronology is short enough that the JKB can be placed somewhere in that interval because a single age is yet out of our reach. We feel confident that this interval can get tighter as newer sections are dated in the future. Even though they do not overlap, the ages presented here highlight a discrepancy between the age of the JKB in the ICS and the ages that we have measured.

2.17) Also in this final chapter, consider the significance of your argument for a significantly younger JKB together with Martinez et al. (2015) suggested age for the base Valanginian at 137 Ma. This would make for a shorter than previously understood Berriasian Stage of a -3 m.y. duration. This in turn contradicts with the astrochronology of Kietzmann et al. (2015), who identify more than 10 long eccentricity cycles in the Berriasian part of the Vaca Muerta Fm.

The issue with Valanginian boundary is presently in the discussion as well as the Hauterivian and Barremian ages by new high precision U-Pb CA-ID-TIMS dating together with cyclostratigraphy and the ammonoid and nannofossil biostratigraphy in the Neuquén Basin by

Beatriz Aguirre Urreta and Mathieu Martinez (in prep.). Some results already published also show several million-year discrepancies with the ICS Time Table.

2.18) The statement in chapter "6. Data availability" suggests that some of the raw data will be withheld until completion of the thesis of the first author. Instead, all data should be made available at the publication of this paper. Understandable practice is not to release data in a thesis prior to publication, but there should be no reason to justify an embargo the other way around.

We will remove this section since all the data is reported in the data table in the supplementary materials. The reported U-Pb table data can easily be copied and pasted on the excel sheet, where it can easily be manipulated in Isoplot in Excel and or RStudio, for instance. Or instead, we can state the latter in chapter 6.

2.19) Table S1 contains the essential data for the U-Pb geochronology, it should be placed in the main part of the paper.

We disagree with the reviewer to place the U-Pb data Table, T.S1 to the main text. With the aim of keeping the manuscript more appealing, we feel that by putting raw data tables cuts the flow of the written text and distracts the reader. Therefore, we think that the data table T.S1 is better viewed separately from the main text, especially when reading in a digital format (which we encourage). It allows going back and forth from the text to the data table more readily if the reader deems necessary. Nevertheless, we leave it at the discretion of the Handling Editor do choose what best fits the format of the journal, and also because it would be an easy adjustment to make in the revised manuscript.

2.20) *Fig. S is also worth transferring from the Supplementary Material to the main part. (However, its labeling needs re-coloring so it be legible in black and white print, panel C might be more informative to show the dated ash bed, D needs labels, and the figure needs a caption.)*

In trying to keep the manuscript short, concise and to the point, we have opted to leave field figures (Fig. S) in the Supplementary Materials. We feel that figures that do not directly support any of the discussion or conclusion and are best kept in the supplementary material. Nevertheless, we leave it up to the Handling Editor to advise us on what better suits the format of the journal. We thank the reviewer for pointing out that the figure was, unfortunately, left out the caption and we will incorporate his advice on how to better the figures such as recoloring for printing and better labeling.

Closing remarks from the authors

In closing, we would like once more to show our appreciation to J.Páfly for reviewing our manuscript and accepting it for publication after the revision. Many of the reviewer's suggestions we agree and will fully accept, with only a very few where we disagree or would not favor the change. For instance, there where two comments that, in our opinion, that standout and substantially add to the manuscript. First, the refocusing the discussion around the apparent mismatch between the ages of the biozones in Las Loicas and Mazatepec, which we address in question 2.1 and 2.14. This is the most critical comment from the reviewer, and we welcome it and assure we will incorporate this will be added to the discussion in the revised version. Second, the renaming of the "Studies areas" section for a "Geological and Stratigraphical Setting" and an expansion of both sections. On this comment, we argue as to why we felt it was essential to leave the Studies areas section short, but did not oppose to reviewer's suggestion. In any case, leave it to the decision of the Handling Editor for the revised version.

All other comments from J. Pálfy, albeit pertinent, we feel that they are minor and straightforward to adjust. For instance, the reviewer suggests a "Results" section separate from the Discussion section, which would imply moving the description of the results found in the Supplementary Material to a new chapter entitled Results in the main text. Another similar request is to place the raw data tables in the Supplementary Material in the main text. Even though we oppose such changes in the structure, we do not see it as a significant modification to the manuscript, and we leave it to the Handling Editor to decide what would best fit the journal's format. Other requests pertain to improving the readability and clarity of the figures, adding a

caption to one of the supplementary figures. Modification in the grammar usage, word choice, style, and spelling will promptly modify since they will improve the manuscript. In short, we feel that we have dealt with all of the reviewer's comments adequately and hopefully, the answers fulfill the requirements for publications by both the reviewer and the Handling Editor.

Technical corrections

The comments below also include several suggestions for better English language, style and word choice.

p. 1, l. 12 (and elsewhere): age ! numeric age

We discuss this in reply to comment 1.10 to reviewer #1 W. Wimbledon. Since this is a paper that discusses the age of a boundary from a geochronological perspective age is necessarily a numerical age. In our view, it would be some tedious to specify age every time. In the introduction, however, we use the "absolute age" nomenclature to distinguish it from the more older ages derived from statistical interpolation. Therefore, in that context, we felt it was necessary to make the distinction. However, throughout the text when we mention "ages", it can only be numerical ages or numeric ages because what we present are U-Pb ages, which are numerical by definition. Therefore, we feel a distinction is not necessary.

elusive ! difficult to determine

OK. Agreed

l. 16: display ! contain

OK. Agreed.

l. 21: one of the last major Phanerozoic stage boundaries ! last Phanerozoic system boundary

OK. Agreed.

l. 23: absolute ! [delete, avoid "absolute age" altogether]

OK. Agreed.

p. 2, l. 3: Calpionella alpina subzone (cf. l. 16) [ensure consistency in zonal names and terminology]

OK. Agreed.

384 *l. 17: selected ! suggested*

385 OK. Agreed.

386 *l. 21: Kamptneri ! kamptneri*

387 OK. Agreed.

388 *p. 3, l. 6: spans ! exposes*

389 OK. Agreed.

390 *l. 12: out of sequence numbering of figures (not as they appear in text)*

391 OK. Agreed. We will modify.

392 *l. 26: optical images ! photomicrographs*

393 OK. Agreed.

394 *p. 4, l. 3: The section ! The Las Loicas section*

395 OK. Agreed.

396 *l. 29: impose ! may provide*

397 OK. Agreed.

398 *p. 5, l. 2, 6: fossil density ! abundance of fossils*

399 OK. Agreed.

400 *p. 6, l 24: Tithonian*

401 OK. Agreed.

402 *p. 9, l. 22: thank*

403 OK. Agreed.

404 *p. 10, l. 3: Neuquén*

405 OK. Agreed.

406 *p. 11, l. 11: [delete] February*

407 OK. Agreed.

408 *p. 12, l. 6: Potosí [+spell out journal name]*

409 OK. Agreed.

410 *l. 13: &amp [delete]*

411 OK. Agreed.

412 *p. 14, l. 4: Episodes [delete the rest of name]*

413 OK. Agreed.

l. 14: Aguirre-Urreta

OK. Agreed.

l. 22: [provide doi instead of URL]

References cited in text but not listed in reference list: Edwards, 1963 R Core Team, 2013

OK. Agreed.

p. 15, l. 3: Distribution of continents ! Global paleogeography

OK. Agreed.

l. 9-15 (Fig. 3): Give stratigraphic horizon of occurrence (e.g. m from base) fo each specimen photographed

OK. Agreed.

p. 16, Fig. 1: delete title, consider using different base map, do not show migrazion routes and sections not discussed in text.

OK. Agreed. We will consider just leaving only the two sections studied. However, it is quite common to add sections that are of the same age to a paleogeographical maps to give a to give the sense of the time equivalent between sections even though they are not discussed in the text.

p. 17, Fig. 2: Barriasian ! Berriasian

[J/K boundary interval – see comments about conceptual flaw here]

[fonts too small in the upper part, too large in the lower part]

OK. Agreed.

p. 18, Fig. 3: [it is redundant to show taxon names here, it is customary to give them in the caption only]

OK. Agreed.

p. 19, Fig. 4: [this is the key figure of the paper, already need to refer to in the Geological setting, so make it Fig. 2; A: show meters; put Las Loicas section to a separate panel B, making the others C and D; La Yesera: indicate placement of JKB; some lettering uses illegibly small font]

Here we disagree with the reviewer. There is no sedimentological or geological consideration is figure 4, but rather a comparison between the ages of markers from each section. Furthermore,

Figure 4 should be within the discussion chapter as the bulk of the discussion pertains to this figure. Therefore, we do not see the purpose of it being placed at the beging on the manuscript.

Supplementary Material

p. 1, l. 1: Ash beds were crushed ! Samples were crushed

OK. Agreed.

p. 3, part 5

Give weight of each sample so zircon yield can be assessed in this context.

Grains discarded as too old are erroneously quoted as >_150 Ma for each sample, provide true cut-off age of grains not included in age calculation.

Weight of the samples was not made because it is not customary to do so. Grains 150 Ma were discarded. The cut-off age for grains included in the weighted mean is sample dependent and are usually the youngest overlapping grains.

5.3 (p. 4): Ash bed LL10 has n=6 grains in Fig. 2, four in text

OK. Agreed, will change it to 4, not 6.

5.4. Ash bed LL13: include date of discarded grains in Table S1 (really older than 450 Ma?)

We do not see the point of reporting the age of grains that are significantly older than the weighted mean of the ash bed. It serves no purpose. Ages much older than the weighted mean are hard to evaluate if they are detrital of inherited from older basement rocks volcanic source.

5.5. "Due to its proximity to the Tordillo Fm." [it is from the Tordillo Fm.] inherited grains or detrital grains?

OK. Agreed.

5.6. MZT-81 (p. 5): check this descriptions, there are errors here. four discarded grains (not five), the grain numbers are in error (belong to sample LL10)

OK. Agreed. Thanks for pointing this out. Will be rectified.

Fig. S needs a caption and should be transferred to the main part of the paper. The labels of the figures need to be recolored so they are legible in black and white print as well.

OK. Agreed.

Table TS.1 is essential to assess the U-Pb dates reported so it should be transferred to the main part of the paper.

Please see the discussion to comment 2.19.

Sample LY5 in Table TS.1: why discard grain z67 and keep z10, when the first one is not older and its error is not larger? This and similar issues of only marginally different aged grains undermine the credibility of unbiased and rigorous selection of grains for the age interpretation.

Weighted mean ages are nothing other than the average mean value of set of dates (youngest grains). In this case, grain LY z67 has a mean value of 147.740 Ma and the precision with what we know the true age of the grain is 93 ka. In figure 2, it is quite clear that LY z67 does not overlap with the weighted mean age of the youngest grains, which means it has little to no chance of statistically belonging to the subset of youngest grains of the population. On the other hand, LY z10 has a mean value of 147.8 Ma and the precision with which we know the age of the 1.1 Ma (much lower precision), and from Fig. 2 it clearly overlaps with the weighted mean age of the sample, which implies that it does have some probability of being a part of the subset of younger grains. In short, LY z10 statistically has a better chance of belonging to the subset of the youngest grains than LY z67, even though the mean value of LY z67 is slightly younger than LY z10. This is just a question of precision, or how well-known is the confidence interval for a particular physical measurement. We draw the attention of the reviewer to compare the Pb* concentration of these two grains. Here, precision is mainly limited by the amount of sample. If the sample size was any bigger, the precision would be higher. Thus the confidence interval reduced. And in that case, grain LY10 would have possibly been excluded from the weighted mean age of the ash bed.

8.2 (p. 11), Table TS.2: Why is the age value of 2 m any different from the age of LL13 taken from this level?

This is because the stratigraphic height of LL13 is in fact at height three m and not two m. This will be rectified in the main text. Notice that the age of LL13 is 142.039 ± 0.058 Ma and the

age of stratigraphic height 3 m is 142.04 ± 0.06 Ma, which is because we have rounded the numbered to two decimal places rather than three. Thank you for pointing that out.

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