

Interactive comment on “Cross-continental age calibration of the Jurassic/Cretaceous boundary” by Luis Lena et al.

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Reply to comments by reviewer W. Wimbledon (reviewer #1) on the manuscript “Cross-Continental age calibration of the Jurassic/Cretaceous boundary”

Comments by the reviewer on his report have been copied and pasted in the italic blue font, and our replies are found immediately below in regular black font. The comments/replies are in order of appearance as in the reviewer’s report. We have taken the liberty to number them 1.1 through 1.14 to facilitate proofing from the Handling Editor and the reviewer himself. Subsequently, selected (the majority) comments made by the reviewer in the “supplementary comments pdf” (i.e., comments made made directly on to the manuscript) are also replied to here in order of appearance referencing

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the page and line where they were made. The remaining replies to some of the comments made by the reviewer in his “supplementary comments” (mainly just language usage corrections) have been replied directly on reviewer’s “supplementary comments pdf” attached to the discussion. There, our replies are found in red background with black font. References are found here, right after the replies to the “supplementary comments” section.

First, we would like to kindly thank W. Wimbledon for accepting to review this manuscript and say that we welcome his comments and corrections. Before directly addressing the reviewer’s comments, we would like to preface this reply by objectively restating the aims and goals of the manuscript. By doing so, we hope to clarify our objectives and use it to substantiate our answers, because we believe that some of the comments and suggestions from W. Wimbledon seem to reveal a misunderstanding of the aims of the manuscript. Briefly stated, we aim to measure the numerical age of the Jurassic/Cretaceous boundary (JKB). In the introduction of the manuscript, we briefly explain why the numerical age of the JKB has remained contentious throughout the years and thus highlight the relevance of the research. We refer to the opening comment J. Pálffy, which states: “This contribution is significant and timely, as the calibration of the JKB has remained uncertain due to a dearth of reliable numeric ages around it”. Subsequently, we describe how we aim to tackle the issue. In our opinion, the best way to measure the numerical age of the Jurassic/Cretaceous boundary (JKB) is to date the age of the base of the Calpionella alpina Subzone, which is the primary marker for the JKB. To avoid local and regional biases, we cross-calibrate the numerical age of the JKB by dating it in two independent sections, in a distinct geological context where the JKB is well-defined by more than one marker. The geochronological tool of choice was high-precision U-Pb zircon geochronology on interbedded volcanic ash deposits found in each section., which recently has become the best geochronological tool to date the stratigraphic record. Furthermore, we also present calcareous nannofossil results to better anchor the JKB in the Mazatepec section, previously only calibrated by Calpionelldis. One of the conclusions of the manuscript is that the reasonable agreement

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between the age of the JKB in both sections represents the age of the boundary, and can be used to date the JKB in other sections that contain the boundary. More importantly, the main contribution of this manuscript is to provide evidence that the quoted age for the JKB, as currently found in ICS 2017 at ~ 145 Ma, is substantially older when compared to our geochronological dataset. Lastly, in the manuscript, we point to the fragility of the biostratigraphy on which the current age of the JKB (~ 145 Ma) is anchored. For instance, comparatively, the biostratigraphy presented in Mahony et al. (2005) pales in comparison to the biostratigraphy of both sections investigated in this study. Additionally, Mahony et al. (2005) use $^{39}\text{Ar}/^{40}\text{Ar}$ geochronology, which over the years has gradually lost its usefulness as a geochronological method for dating the stratigraphic record for the lack such accuracy and precision required to calibrate stage boundaries. In closing, W. Wimbledon's comment that we only talk about geochronology is to some extent quite right. The primary aim of the paper is to study the JKB from a geochronological perspective, and we leave that very clear from the very beginning. This makes this study especially important because geochronology has been a void surrounding the topic for many years. This leads us to the first comment by W. Wimbledon regarding magnetostratigraphy.

General Stratigraphic remarks – Magnetostratigraphy

COMMENT 1.1) The text should perhaps say that there is no possibility of magnetic calibration of Las Loicas with the many Tethyan sites where it has been documented.

REPLY: A subset of the authors are pursuing the magnetic calibration in Las Loicas. The preliminary sampling has already been done, and some results are available yet not published. The main obstacle is that the basal facies in Las Loicas which makes it difficult to have a dense suitable sampling for magnetostratigraphy. Therefore, efforts are being made to overcome this issue. Please see reply to comment 1.2 and 2.9 (i.e., in reply to reviewer #2 J. Pálfy) for further clarification on our attempt to correlate the magnetostratigraphic data from Arroyo Loconche with Las Loicas.

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COMMENT 1.2) the ammonite zonations applied at the LL and AL do not agree – a big problem.

REPLY: It needs to be made clear that we do not present any new magnetostratigraphic data, but instead use the magnetostratigraphy of Iglesias 2017 to aid marginally and back-up our age of the JKB in Las Loicas. Magnetostratigraphy is not the focus of the paper nor did we state in the manuscript that we aimed to do that, but rather an aside. Meaning, we use it as a reflection on how other substantial evidence for the JKB from the Neuquen Basin might agree with our data. We recognize that magnetostratigraphy is a significant component in calibrating and locating the JKB in sections that span the JKB. However, we are fully aware of the seemingly conflicting evidence from the ammonite zonation from Arroyo Loncoche and Las Loicas. We clearly stated that the ammonite zonation in Arroyo Loncoche is preliminary and also cited a discussion around the matter in López-Martínez et al. (2018). The main point discussed in López-Martínez et al. (2018) is that both discussions contain the different resolution of data. The ammonite biostratigraphy of Las Loicas is based on the bed by bed collection from 54 fossiliferous levels with 450 ammonite specimens. López-Martínez et al. (2017 Fig. 1) and Vennari et al. (2014) recorded 35 fossiliferous levels and studied 228 ammonite specimens. Therefore, we feel that the ammonite zonations in Las Loicas is well-defined and described. On the other hand, in the Arroyo Loncoche region there is not a single published section with the ammonite levels, or the number of specimens collected, which renders the definition of the biozones unreliable. It is also evident that the boundaries of the biozones in Arroyo Loncoche have been changing along the years, as well as unit thickness, the presence of sills, etc. We invite the reviewer to take a closer look at the discussion in Lopez-Martinez et al. 2018 but include here an extraction from the paper to illustrate the issue. Iglesia Llanos et al. (2017), p. 194 state that “The boundary between ammonite zones in Arroyo Loncoche was placed according to the first occurrence of the index species.” However, the range chart with vertical distribution of the taxa (their Fig. 2) and the ammonite biozones do not follow this criterion. For instance, the base of the *Corongoceras alternans* zone is placed at

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the first occurrence of *Corongoceras* sp. and the index species is not even recorded in this section. Furthermore, the base of the *Substeueroceras koeneni* zone is placed on the first occurrence of *Substeueroceras* sp. (at 150m of the base of the section) while the index species appears higher (above 180 m). This more than 30m discrepancy explains the different biozonation of the same section published by Kietzmann et al. (2011 Fig. 3) where they placed the base of the *Substeueroceras koeneni* zone at 190 m of the base of the Arroyo Loncoche section. Lastly, 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. In conclusion, the paper aims to calibrate the numerical age of the JKB using high-precision geochronology in Las Loicas and Mazatepec using the base of the *Calpionella* alpine zone as the primary marker for the JKB. In Figure 4, we correlate the JKB Arroyo Loconche and Las Loicas based on more compelling evidence for the JKB which is the M19.2n (Arroyo Loconche) and the base of the *Calpionella* alpine Subzone (Las Loicas). Therefore, we avoided normalizing the two sections based on ammonite. Incidentally, in his 2017 review of the JKB, W. Wimbledon (Wimbledon, 2017) has also normalized Las Loicas and Arroyo Loncoche disregarding the apparent mismatch of the ammonite zones in the working model for correlating the regions for the JKB. Furthermore, in our Figure 4, the correlation between the M19.2n in Arroyo Loncoche and the base of the *Calpionella* alpine subzone in Las Loicas is a dashed red line, which suggests that the correlation is merely conjectural. As W. Wimbledon pointed out, Phanerozoic stage boundaries are not dependent on geochemistry, magnetostratigraphy or geochronology. These are just tools used to aid the calibration of stage boundaries, and mismatches are commonplace. Therefore, we do not see this first point as a big problem. The reviewer #2, J. Pálfy, also took an issue with this matter. We kindly ask the reviewer to also read the reply on comment 2.9 (i.e., in reply reviewer #2). Hopefully, it will supplement this reply and vice-versa.

COMMENT: 1.3) The calpionellid assemblage noted at Las Loicas is anomalous: such a mixed assemblage (with apparently derived Tithonian calpionellids) does not

define or mark the base of the Berriasian. It should be made clear what is definitively lower Berriasian and what is not.

REPLY: First, it is essential to recognize that sections containing datable horizons close to boundaries, such as the Las Loicas and Mazatepec, are extremely rare which is a significant hindrance in calibrating the age of stage boundaries in general. Tethyan and Mediterranean sections do not contain datable horizons, because these sections are deposited in passive margins far from plate tectonic boundaries where a considerable amount of acidic-aerial volcanism output is produced allowing for the deposition of ash fall deposits (ash beds). Therefore, even though the issues surrounding the JKB have been concentrated in the Tethys region, its age, on the other hand, will not. Although the reviewer claims that the Las Loicas contains “anomalous” calpionellid assemblages, if we are ever to advance in the knowledge of the numerical age of the JKB, we have to use everything at our disposal. Replying to the reviewer’s comments, the only reported “anomalies” in Las Loicas are a) the presence of *Tintinnopsella remanei* in the upper part of the Crassicollaria Zone. This is a none typical appearance in the Mediterranean Tethys, but usual in western Tethys as discussed in López-Martínez et al., (2017), and b) the record of *Crassicollaria massutiniana* in the lowermost part of the Alpina Subzone. Even when it can be unusual the presence of this species in the Lowermost Berriasian, this does not affect the biozonation scheme as the Alpina Subzone is defined by the acme of *Calpionella alpina* small and globular form and not the Last Occurrence of any species. Then, the Alpina Subzone is defined in the same way as in the Mediterranean Tethys and can be used as a marker of the JKB in Las Loicas.

COMMENT: 1.4) The nannofossil literature cited as the justification for some of the text’s discussion and conclusions is rather old - Bralower and Casellato references are now 10 -30 years old. Many Tethyan sites have since been documented, and that make some of the species FADS and the zones discussed obsolete. Some Italian localities cited in the text are seen as anomalous in the positions of their nannofossil FADs. Thus it is not clear why these localities are selected by the authors for comparison with the

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LL and M sites, especially when they are not the best/most representative.

REPLY: We, unfortunately, have to disagree with this comment. There are two standard calcareous nannofossils zonation for the studied interval. (Bralower et al., 1989) proposed a calcareous nannofossil zonation for the Jurassic and Cretaceous based on southern European land sections and the western North Atlantic, DSDP Sites 391C and 534A. (Casellato, 2010) proposed a new calcareous nannofossil biostratigraphic scheme for the Tithonian–Early Berriasian established for the Southern Alps in Northern Italy. Even though many recent papers deal with nannofossils of this time interval, there are no new zonations for this interval. Therefore, these two papers form the basis for newer studies, with many of the recent publications still citing the zonation in the classic papers of Bralower et al. (1989) and Casellato (2010). We agree with the reviewer that these publications might be considered old, but in no way, shape, or form, can they be considered outdated or overtaken since the zonation presented in them form the basis of the more recent works on calcareous nannofossils zonation of this period. To illustrate, we take the liberty of copying below excerpts from the newer publications on calcareous nannofossil of this period that promptly cite the work of Bralower et al. (1989) and Casellato (2010), as we have. a) Grabowski et al., (2017). *Sedimentary Geology* 360, p. 57, state: “For biostratigraphic purposes, the available biostratigraphic schemes of Bralower et al. (1989), Bown and Cooper (1998) and Casellato (2010) were considered. The latter was selected to apply for the Lókút section, as the most appropriate for nannofossil record in this Tethys location”. b) Hoedemaeker et al. 2016. *Revue de Paleobiologie* 35, p. 190, state: “CALCAREOUS NANNOFOSSILS (C. E. Casellato and S. Gardin). . . . Calcareous nannofossils are rare to common and poorly to well preserved, with overgrowth more pervasive than etching. Assemblages are of Tethyan affinity (and) the biostratigraphic schemes adopted in this study are those of Bralower et al. (1989) and Casellato (2010). . . .” c) Ogg et al., (2012). *A Concise Time Scale*, p. 170, state: Use for defining the JK boundary in the Mediterranean Tethys similar FOs of calcareous nannofossils as those used in our paper (fig. 13.2.). d) Schnabl et al., (2015). *Geologica Carpathica* 66, p. 491,

state: “For several generations, apart from occasional aberrations, definitions of a J/K boundary have focused on one interval, between the base and top of one ammonite subzone (that of *Berriasella jacobi*), and, in the last thirty years, more and more, on the widespread and more consistently recognized turnover from *Crassicollaria* assemblages to small *Calpionella*. . . . Latterly this has been widely reinforced by the use of calcareous nannofossil FADs (references in Casellato 2010).” e) Sbodova and Kotsak 2016. *Geologica Carpathica* 67, p. 225 state: “Biostratigraphic data were interpreted with reference to the nannofossil zonation of Casellato (2010), commonly used for the Upper Jurassic and the Lower Cretaceous in the Tethyan/Mediterranean area”. Regarding some anomalous positions of the nannofossils. p. 231. “It should be noted, that the LO of *N. kamptneri* minor usually appears a little above the LO of *N. steinmannii* minor, but in this paper it occurs together with the LOs of *N. steinmannii* *steinmannii* and *N. kamptneri* *kamptneri* in bed 35. This anomaly can be explained by the very poor preservation and extreme etching of calcareous nannofossils between beds 32 and 34. Moreover, the appearance of these four species together suggests the presence of a hiatus.” f) Bakhmutov et al., (2018), *Geological Quarterly*, 62 p. 232, state: “The first appearances of species of significant calcareous nannofossils at Theodosia are shown in Figure 23. The appearances are not consistently equivalent to all records in western Tethys (Casellato, 2010; Schnabl et al., 2015), one reason being that in this preliminary study we did not sample beds below a level we believe to be assignable to the lower to middle part of M19n.2n. However, the FADs in M19n of *H. strictus*, *C. cuvillieri*, *N. wintereri*, *N. steinmannii* minor and *N. kamptneri* minor appear to be consistent with other regions.”

COMMENT: 1.5) The dating of the magnetozones needs to highlight and discussed at more length in the Discussion.

REPLY: Very confusing comment. Also on magnetostratigraphy, the reviewer comments on the dating of magnetozones. Nowhere in the manuscript, it is stated that we have dated magnetozones, or implied doing so. This is clearly beyond the scope of

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the manuscript, and additionally, not even possible since we do not present any magnetostratigraphic new data in any of the studied sections. To date magnetozones, one would have to present magnetostratigraphic and geochronological data on the same section, which we have not done nor said we had. It would not be scientifically sound to do otherwise. Therefore, we feel the request of the reviewer is rather odd and unjustified. 1.6) Also, the assumptions (as seen in most publications) about using the magnetostratigraphic scale as a time scale could be laid out fully in the Introduction. The reviewer also asks us to discuss the assumptions of using magnetostratigraphy, which again, is not the aim of the paper since we do not present any magnetostratigraphic data whatsoever. Therefore, to discourse about the use of magnetostratigraphy is beyond the scope of the dataset we present not to mention beyond the point of the problem we are trying to solve and the methods we use. Again, an odd comment from the reviewer.

COMMENT: 1.7) Notably, Ogg et al 2016 is not at all 'official' and is not attributable to ICS, but this is not clear from the text. REPLY: The reviewer does have a point and will make the distinction clearer between the ICS and Ogg 2016.

Structure

COMMENT: 1.8) The chronostratigraphic and biostratigraphic background should be made clear before consideration of any new data on radiometric dates.

REPLY: The issues and intricacies with fixating the JKB are well-documented in several publications which W. Wimbledon himself has authored and co-authored. The biostratigraphy in both sections we use has also been well-documented in other publications by some of the authors in this manuscript. Therefore, we feel that this request is somewhat unnecessary since the issue has been dealt with quite thoroughly in other publications and have been cited throughout the manuscript when necessary. Furthermore, we have taken Solid Earth's recommendation on the manuscript type, where manuscripts should be short, concise, and to the point which is a trend among high-

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impact journals. We feel that reviewing the biostratigraphic framework would make the manuscript unnecessarily long. In closing, the manuscript is not dedicated to reviewing any previous data, but rather presenting new data and building on pre-existing biostratigraphy.

COMMENT: 1.9) The extrapolation of the GMPTS to onshore localities is central. The paper is concerned with attaching radiometric dates to a biostratigraphic framework. But it says very little about how radiometric dates match the timescale used by, for instance, Gradstein et al 2012: a time framework linked to the oceanic magnetostratigraphic record, the GMPTS. The last is hardly mentioned.

REPLY: Extrapolating the GMPTS would be over-interpreting our data and certainly beyond the scope of the manuscript. Nevertheless, we feel this is the main problem that the International Chronostratigraphic Chart of International Commission on Stratigraphy (2005 to 2018 versions) is facing. The 145 Ma age for the JKB boundary is based on the Shatsky Rise magnetozones. The main drawback for this assertion is the accepted age for the base of the Berriasian, supported by a poorly dated age of the Shatsky Rise in the Pacific Ocean. The radiometric Ar-Ar dating, even if only the best two samples were considered, have reduced plateaux that could indicate some ^{39}Ar recoil (Mahoney et al., 2005). The ages of these samples are 144.8 ± 1.2 , 143.7 ± 3.0 , and 142.2 ± 5.3 Ma, but 145 Ma was the preferred age as it coincides with the spreading rate assumed for this part of the Pacific Ocean floor (Ogg et al., 2012). Besides, when the biostratigraphic controls of the sediments of the Shatsky Rise intruded by the dated sills are taken in consideration, the results are not very well constrained (Mahoney et al. 2005 cited (Bown, 2005)): Quoting Bown (2005) : "Zone NK1; Berriasian (Site 1213): The Jurassic/Cretaceous boundary interval zonation of Bralower et al. (1989) is based on a distinctive succession of nannolith appearances, notably *Conusphaera* and *Nannoconus*; however, these taxa were absent in this part of the section and the former was absent throughout. In addition, a number of important marker species of the family Cretarhabdaceae (*C. cuvillieri*, *R. angus-*

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tiforata, and *Retecapsa octofenestrata*) and genus *Eiffellithus* (*E. primus*, *E. windii*, and *E. striatus*), although present, are rare and restricted to a small number of samples, and their first and last occurrences may not be biostratigraphically reliable. The lowest Cretaceous zones are thus identified using marker species, where present, together with alternative datum events and aspects of the entire assemblages. The lowermost productive samples (Core 198-1213B-27R) yielded *H. chiasia*, *L. carniolensis*, *Tubodiscus bellii*, and *R. laffittei*, indicating Subzone NJKc or younger. The nannofossils do not unambiguously indicate a Cretaceous age, but correlation with Zone NK1 is inferred based on the presence of the genus *Tubodiscus* and absence of *R. angustiforata*, *P. fenestrata*, and *R. wisei* (Bralower et al., 1989). Support for this interpretation also comes from radiolarian fauna that also indicate a Berriasian age for the lowermost cores (H. Kano, pers. comm., 2003). In conclusion, Bown (2005) makes it clear that the nannofossils do not unambiguously indicate a Cretaceous age but also the inferred correlation with zone NK1 is based in the presence of ONE GENUS and the ABSENCE of three species (negative evidence!). There are not markers, no first appearances, etc. Regarding the radiolarians, the data are based only in personal communication. The facts stated above discredit the JKB extrapolation of the Global Magnetic Polarity Time Scale (GMTPS) based on the Shatsky Rise to onshore localities currently in use. In any case, it is not the focus of this paper to criticize the age of the magnetic polarity time scale used to define the boundary, which needs a deep revision in our opinion.

COMMENT: 1.10) The core of the paper could usefully be a careful examination of the calibration of the dated ash horizons and the levels with the key biostratigraphic markers – listing them in sequence, level by level. REPLY: The detailed biostratigraphy of Las Loicas with an indication of the fossiliferous levels and relevant markers is indicated in Vennari et al., (2014) and López-Martínez et al. (2017). The objective of this paper is to date this biostratigraphy with accurate and precise ages in the relevant interbedded tuffs coupled with age-depth modeling. In the Mazatepec section, biostratigraphy based in calpionellids is found López-Martínez et al. (2013) is complemented by new

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calcareous nannofossil occurrences which are presented here and presented in figures and pictures.

Precision, accuracy, English Language

COMMENT: 1.11) There are numerous examples of rather problematic phrases and sentences which are not written in good English. But more critical is the lack of precision or looseness in language and terminology. This lets down the submission very badly. It is the thing that needs the most attention in a revision by the authors. We do admit that many of W. Wimbledon's suggestions on our English usage and grammar (or lack thereof) are correct and we welcome them. We incorporate all the suggested words, variations and rewrite all sentences pointed out that remain unclear and confusing. Names of species will be thoroughly revised. Specific replies to comments on the supplementary section are found below.

REPLY: Corrections on some spelling mistakes are just differences between American and British English. Spellings such as gray, meter, catalog, paleontological, memorize, analog, analyze, defense, color, aging, inquiry, license among many other words are a correct and legitimate form of spelling in American English. Therefore, since Solid Earth does not dictate which kind of English is to be used in their publications, we chose to use American spelling. Furthermore, we feel we were consistent with our choice of spelling thought out the manuscript. Therefore, we have decided to disregard the reviewer's comments on these spelling mistakes.

COMMENT: 1.12) The loose wording of the Abstract's and Introduction's first sentences. No, the age of the J/K boundary is very clear. Lena et al. talk only about radiometric dating. They should say that the start of Berriasian age/base of the Berriasian stage has been more or less fixed for some years [the authors actually quote several relevant papers that show this]

REPLY: This comment is quite confusing, and we are not sure what the reviewer meant by this. We hypothesize it might have to do with how different fields in the Earth Sci-

ences use the word “age” with subtle nuances, which is understandable. For instance, in the field of paleontology, an age of a fossil can sometimes be ascribed as an age of a stage. For instance, saying “fossil XY has a Tithonian age” or “is Tithonian” is perfectly acceptable when used in this context. However, in the matter of calibrating the numerical age of stage boundaries such usage of the word age is too loose because a stage boundary can last for millions of years; therefore, it lacks accuracy and precision. In the context of calibrating the age of a stage boundary, the word “age” needs necessarily to be taken as a numerical age (or radiometric age), usually arising from a physical measurement which carries a mean value and an error. Since this manuscript deals with the age calibration of stage boundary from a geochronological perspective, no other meaning of the word “age” is possible other than a numerical age. Therefore, every time the word age appears in the manuscript, it should necessarily be interpreted and understood as a numerical age. Singling out what type of age we are talking about as radiometric age is redundant and unnecessary since no other meaning is possible. The main aim of the manuscript is to dispute the (numerical) age of the JKB, therefore “Berriasian age” is meaningless and confusing with the aim of calibrating the age of a boundary. Certainly, the sentence suggested by the reviewer “The base of the Berriasian stage has been fixed for some years” is 100% correct, which one would correctly interpret as the base of the Berriasian has been fixed at the base of the Calpionella alpine Subzone and has been for many years. However, the sentence does not bear any relation to the numerical age of the base of the Berriasian, aka the JKB, which is the foremost purpose of this manuscript. As described in the introduction, there have been many (numerical) ages for the base of the Berriasian over the years, 135 Ma, 140 Ma, 144 Ma, 145 Ma. This represents a span of 10 Ma, which begs the question: What is the age of the JKB after all? Having an age of a boundary that is floating around a span of 10 Ma is less the ideal. Therefore, by any standards, the age of the JKB has been contentious over the past years. Sure, one could suggest it to be Berriasian, but this is too loose of a definition for the sake of numerical calibrating the geological timescale. Since 2005, the ICS has the JKB at ~145 Ma, which means approximately

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145 Ma. From a geochronological perspective this far from ideal for ascribing a numerical age to a boundary. Admittedly, for many outside the field of geochronology such nuance bears no meaning, but for an accurate division of the geological timescale it is imperative to find a more realistic age for the JKB, where geochronological data from many sections seem to converge to a similar age. We are confident we have demonstrated this in the manuscript. In short, geochronology is of the utmost importance to understand the rate of geological phenomena; for instance, duration magma magmatic processes, tectonic processes, duration of mass extinctions and recoveries. All this relies on the accurate and precise knowledge of the (numerical) age of rocks, paleontological markers, and stage boundaries. The latter two can only be resolved by using dating horizons that are close to boundaries using geochronological methods that are accurate and precise, which is methodology we have used in this manuscript.

COMMENT: 1.13) “JKB” is not standard terminology. It appears hundreds of times in the text. “J/K boundary” is the norm. Alternatives for use are: the base of the Alpina Subzone, base of Berriasian Stage, Tithonian/Berriasian boundary, or, less precisely, the J/K interval, the boundary interval. . Care is required is using the phrase J/K boundary.

REPLY: We feel that abbreviations can take any form, as long as it is clearly stated in the text and consistently used throughout. The use of the “J/K boundary” is just a personal preference of the reviewer as is our choice to use “JKB” just because something might be considered the norm hardly qualifies it to be mandatory. We see no problem with this abbreviation. However, if the reviewer or the Handling Editor feel adamant about this, we can certainly accommodate it since it is a frivolous matter and simple to adjust. We have deliberately chosen not to vary the term JKB with its many analogs to avoid confusion, especially to that reader that is not familiar with the various synonyms that the term JKB takes. Since we aim to draw attention from a broader audience, we feel that the term JKB should stay fixed for clarity, even though it might come across as repetitive.

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COMMENT: 1.14) Anything that is not exactly correlated with the base of the Alpina Subzone can be said to be in the J/K interval, but not at the boundary. The reader is sometimes not sure what interval is referred to, or what horizon. Many times a fossil or date is somewhere in the J/K interval, but, to be accurate, nowhere near the actual boundary.

REPLY: We do understand that we are introducing a new concept (the JKB interval) to a field that already has a plethora of analog terms. However, we want to make it clear that the JKB interval is NOT a substitute for the JKB and the JKB is not the JKB interval. The idea for the JKB interval 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 #2 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. Nevertheless, we will try to make a great effort to make this distinction very clear in the revised version of the manuscript. To supplement this reply, we refer the reviewer to comment 2.16 (i.e., in reply to reviewer #2, J. Pálffy).

Closing remarks by the authors

In summary, we thank the review from W. Wimbledon. We believe that many of the reviewer's comments, although very interesting, do not pertain or are beyond the limits of interpretation of our data. Some request we deem unnecessary because they would force the manuscript to steer away from the primary focus of the manuscript for no plausible reason. It was not clear to us the reasoning behind these comments. For instance, the reviewer's comments on magnetostratigraphy are incongruous with the focus of the manuscript. Dating magnetozones, laying down the assumptions about using the magnetostratigraphic scale as a timescale, It seems like the reviewer did not grasp the aims of the manuscript. Additionally, the comments about the revision of the biostratigraphy we consider to be unnecessary and would make the manuscript too long. Therefore, we disagree with the reviewer that substantial improvement is

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required. Some comments on spelling, English misuse, and development of the prose are on point. And we thank W. Wimbledon for his thorough correction of these mistakes which will undoubtedly improve the readability of the revised version and make it much more precise. Nevertheless, these can be considered minor adjustments to the revised version. We welcome these suggestions and will take them into account for the revised manuscript. Finally, we are confident that we have dealt with all of the reviewer's comments and suggestions appropriately. Hopefully, the manuscript can be approved for a revised version.

Reply to W. Wimbledon - Supplementary Comments

Page 1, line 28 This is geochronological jargon: "final" usually means the reported age. We will replace the word "final" with "reported age".

Page 2, line 1 to 5 The reviewer pointed to a problem with the construction of the sentence. What we want to imply is that reported ages from a previous publication are imprecise and they do not overlap, which means that they do not match, agree, or have the same age. That is what is implied by no overlap. The main difficulty in finding a (numerical) age for the JKB has been the choice for the base of the Berriasian. Of course, this has been solved, but back in the day when the first attempts to date the boundary (1985, 1995) this was still an issue, and this had significant implications towards the numerical age of the JKB. Maybe this has been clarified for the reviewer. "... and this level has been the most popular boundary marker for around 30 years". The Killian group in their 2014 report (Reboulet et al., 2014) still recommend the base of the Barriasella as a marker for the JKB. This might not be the case for the Berriasian Working Group, where 76% have chosen the base of the Calpionella alpine Subzone, but this is apparently not an overwhelming consensus within the entire community.

Page 2, line 13 What we are implying here is that the base of the JKB is assumed to be the base of the Calpionella alpine subzone, not the ash bed. We will rephrase for clear meaning.

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Page 2, line 16 “Recent years” will be deleted.

Page 2, line 19 We will rephrase it to “We also report new nannofossil results from Mazatepec section.”

Page 2, line 22 See reply above on the usage of the word “age” in reply to comment 1.12.

Page 2, line 24,25 We will rephrase it to “which in turn also validates our age for the early Berriasian and the JKB.”

Page 2, line 27 We will replace “JKB” for “boundary” to avoid repetition. We want to avoid the use of the other many synonyms for JKB to prevent any confusion.

Page 3, line 3 “a” replaced by “the”

Page 3, line 5 .. of the Eastern... will be added

Page 3, line 6 Replaced outcrops by exposed, since fossils do not crop out.

Page 3, line 8 Gray is the American spelling. See comments on spelling in reply to comment 1.11

Page 3, line 15-16 We disagree with the reviewer. The sentence is well constructed. Zircon does not need to be pluralized since it related to the behavior of the mineral zircon in general.

Page 3, 17 Will deleting the word dated as suggested, because of the precision of language.

Page 3, line 21 Mazatepec will be inserted instead of the vague term “the section in Mexico.”

Page 3, line 30 The comma will be added.

Page 3, line 31 The definite article “the” will be before the noun R (as in the statistical package)

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Page 4, line 3 “The section” will be replaced by “The Las Loicas section” as suggested
Page 4, line 4-5 “Found in the Las Loicas section” will be deleted for it was redundant, as pointed out by the reviewer.

Page 4, line 14 “(ca. 15m stratigraphic height)” is there to facilitate and aid the reader to locate the position in Figure 4.

Page 4, line 19 Bralower’s thirty year old results must be seen as totally overtaken by more recent results, and to a lesser extent it is true of Casellato 2010. You quote Wimbledon 2017 which shows a more recent situation Please see reply to comment 1.4

Page 4, line 19 T. remanei and C. massutiniana are decidedly not typically Berriasian Please see reply to comment 1.3

Page 4, line 29 Magnetozones will replace Magnetostratigraphic zones.

Page 4, line 30 No. This is very very vague. In numerous sections the base of the Alpina Subzone is proved in the middle of M19n.2n We will change from coincident to the middle of M19.2n be more precise.

Page 5, line 5 Rather unsafe. Authors present no evidence on Arroyo Loncoche. They cannot interpret what is or is not M19n.2n at LL, as they say. How can the authors’ results be close to those of Inglesia Llanos when they have no magnetostratigraphy to present at Las Loicas and do not work on AL? Please see reply to comment 1.2 and also 2.9 (in reply to J. Pálffy)

Page 5, line 25 Again, surely this is obsolete work to cite? More up to date references required. The Italian data has been superseded. By the way, Ogg et al. 2016 is not original reserch but a compilation The authors do not agree with Wimbledon (2017) where he said that N. kamptneri kamptneri and N. steinmannii steinmannii bioevents, previously used as infallible biozonal indicators in M17r, have been found widely in lower M18r and the upper half of M19n (Figs. 1, 2). Based on Wimbledon (2017) fig-

ure 2 only in Puerto Escano these bioevents are correlated with the upper part of *C. alpina*. (M19n). Besides, according to Svobodova and Kostak 2016 (cited by Wimbledon (2017) only in “one” sample they recognized this bioevent in M19n1r and other is correlated with M18r. The record of *N. steinmannii* and the biozone NK1 are correlated with the Calpionella Zone without specifying the subzone and it is recorded nearly one meter above the acme of *C. alpina*.

Page 5, line 30 This does not match evidence from lots of sites *N. steinmannii* is not a marker for the Elliptica Subzone, especially when it occurs as low as the Alpina Subzone. *N. steinmannii* defines the base of the NK1 zone and nowhere in the text have the authors considered this marker as a bioevent of the Elliptica Subzone. The authors explain that it is found associated with this calpionellid in the Mazatepec section of Mexico, in the same way of other sections cited and that the NK1 Zone has been correlated in different section with the Elliptica biozone. The authors does not state that this bioevent is a marker of this calpionellid biozone. You quote Wimbledon 2017? Citation will be deleted

Page 6, line 9-11 We will try to rewrite to make it clearer.

Page 6, line 18 “loc” was supposed to be located

Page 6, line 19 Tethys regions will be replaced by Tethys Ocean

Page 6, line 23 We agree the sentence does not read well. What we want to imply is that the age of ash bed LY5 is an age in the Tithonian. We will rephrase to make it more clear.

Page 7, line 1 Perhaps the sentence would read better if stated: “Therefore, our new ages for the base of the Berriasian and the early Tithonian yield an expected duration for Tithonian.”

Page 7, line 1 How is it “recommended”???? Ogg is just another publication. And not an ICS publication. We will try to make a clearer distinction between Ogg et al. 2016

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and the ICS.

Page 7, line 8 subsection title We feel that this is a great subsection title, it instigates the reader to pose the question: Do the ages presented here present the age of the boundary globally? Meaning, if we could measure the age of the JKB in every section, would we find the same age everywhere? Although this is impractical because not every section has table horizons close to the boundary, we argue for the fact that the Las Loicas and the Mazatepec agree favorably our ages can be considered as the age of the JKB globally. As a hypothetical, suppose that the age of the Las Loicas was 140 Ma and that the age of the Mazatepec was 143 Ma, then it would be hard to argue that their age agrees. However, they are off by 600 ka, which is a short interval.

Page 7, line 29 The reviewer says the FAD of *R. asper* is much older. How older is the FAD *R. asper*? Can he precise how much older?

Page 8, 1st paragraph “And yet for 200 years geologists have divided up the geological column quite successfully, with no magnetic markers and with no geochemistry, and the bulk of agreed GSSPs do not rely on these. Replace this sentence?” High-precision geochronology has enabled the understanding of Earth processes in great detail. The time scales at which we deal in the manuscript are in the order of 50 ka, in which preservation of the paleontological markers becomes of extreme importance. We never suggested that GSSPs do not rely on secondary markers, but rather a valuable tool. Additionally, we draw W. Wimbledon’s attention to the comments of J. Pálfy (reviewer #2), where he suggests that we should use our high-precision ages in both sections to show how problematic it can be to assume time-equivalency of biozones. We also share Pálfy view. Our data clearly shows a slight mismatch at the sub 100 ka level. In this scenario, the diachroneity of FAD and LAD’s becomes evident and thus the dating of the stratigraphic record using high-precision U-Pb geochronology becomes a powerful tool in unraveling such nuances. It is undebatable that paleontology has been successful in dividing the geological timescale in the past. However, integrating geochronology, stratigraphy, paleontology, geochemistry, and magnetostratigraphy can

push the limits of correlations and calibrations of the geological timescale and is the best way forward. Perhaps, in the suggested sentence, we could state that in the context of calibrating the age of stage boundaries at the sub 100ka level, preservation of paleontological markers is an issue. Maybe this way it would be made clearer.

Page 8, line 5 meaning? one level but rest of sentence is about a set of biological events that took place across the Upper Tith-lower Berriasian interval We did not understand the reviewer's comment.

Page 8, line 7 what 'explosions'? bloom of small C alpina? It came after diversification of nannoconids We meant the bloom of small Calpionell alpine

Page 8, line 19-20 Vague, no justification shown Through out the paragraph we cite publications to support this last sentence.

Page 8, line 24 Its proper name is the "International Chronostratigraphic Chart"? Will make the modification to "International Chronostratigraphic Chart".

Page 8, line 24 meaning? It is beyond the scope of the manuscript to go into detail on the issue of offset between Ar-Ar and U-Pb ages. However, it is an important statement to be made from a geochronological perspective.

Page 8, line 31 it is a hole in the sea bed, there is no section We will refer to it as core, as was done previously in the paragraph, instead of section.

Page 8, line 31 vague We will incorporate examples of the JKB markers in the sentence, even though at this point in the manuscript we are deep into the discussion and have stated and cited what the markers are and expect the reader to be following along.

Page 9, line 1 As a concluding sentence it is not effective. It says, more or less, our age agrees with othe ages. Not a very weighty ending We disagree, this last sentence sums up that our age agrees with more recent ages for the JKB, and can be considered the age of the JKB globally. The sentence, in our opinion, is actually quite important sentence and carries a lot of weight. No other study dealing with the age of the JKB

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could make such a big claim.

Page 9, line 3 - comment of the title of section 5 – Conclusions and Summary Cretaceous rock/time is base Berriasian stage and start Berriasian age. What you discuss is geochronology and radiometric dates We are not sure what the reviewer meant by this comment. Not very clear.

Page 9, line 8 what interval, you just presented numbers The reviewer missed the point of the the meaning of the JKB interval. We talk about the JKB interval previously in the manuscript. For clarification with regards to the JKB interval, we refer to comment 2.16.

Page 9, line 10-11 This ammonite biozone is enormously long, what can it bracket or corroborate? Precision? We did not imply that we could bracket anything using an ammonite zones. Our bracketed interval is the JKB interval, which is bracketed with U-Pb ages which is staed in the manuscript.

Comments on Figures

Page 17, Figure 2 The main aim of figure 2 is to display our U-Pb data. The biozones are displayed merely conjecturally, since the exact age and duration of the biozones are not known. Therefore, adding boundaries to the biozones would be unrealistic and wrong. Spelling will be rectified.

Page 19, Figure 4 Spelling of species names will be rectified. Boundary abbreviations have been commented on previously in this reply. Additionally, abbreviations were adopted to make the figures more clear, less clustered, and easier to read.

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Please also note the supplement to this comment:

<https://www.solid-earth-discuss.net/se-2018-57/se-2018-57-AC2-supplement.pdf>

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2018-57>, 2018.

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