Dear Pr. Sluijs,

This is my review of the revised manuscript: "Palaeoclimatic oscillations in the Pliensbachian (Lower Jurassic) of the Asturian Basin (Northern Spain)" by Gomez et al. As stated by the two initial reviewers, the paper present interesting carbon and oxygen-isotope data measured on belemnites from Asturias (Spain), which are interpreted in term of climate changes and carbon cycle disturbances during the Late Sinemurian – Early Toarcian interval. In my opinion, these new data are important and worth being published because they allow new inter-basinal comparisons, essential for deciphering the global vs. local extent of trends previously recorded during the Early Jurassic.

The main question is whether the authors fully address the main comments of the two previous referees. Whereas relevant answers has been done to explain the meaning of geochemical data recorded by belemnites, as well as the diagenetic overprints, I think that the main remark of Referee 1 is not considered. Through my reading of the revised manuscript, I completely agree with him because the new data are not appropriate to elucidate the presence of ice during the Late Pliensbachian. The review of ice proxies in the discussion is inconclusive and should be removed/shorten. The beginning of the abstract seems to tell us that the presence of Jurassic ice is the main debate of the paper but the conclusions relate to another question.

I regret that the authors merely draw general correlations between isotope profiles from Asturias and other NW Tethyan basins, without going beyond in the discussion about processes. I mean that the discussion about the link between temperature changes and carbon cycle are often reduced to enumerations of all possible scenarii previously published. However the new carbon isotope data could be used to clearly discriminate these hypotheses. It is important to be more conclusive.

Basically, for each isotopic event reported by the authors, it is important:
- To be homogeneous in the definition of isotopic trends *vs.* excursions and to use appropriate LOWESS regressions to discuss the long-term trends
- To clearly state what is local, regional (NW Tethyan) or global by discussing data from Panthalassa and other domains.
- To specify (in 4.1. Carbon isotope curve) what are the most relevant processes for carbon isotope fluctuations recorded in the Asturia basin. I think that a strong part of recent literature linking Jurassic carbon cycle and climate changes is lacking. It is very important to explain what bring the new carbon isotope data from Asturias in this debate

For all these reasons, I think that the manuscript needs to be revised once again through major corrections. Please find below detailed comments, which, I hope, will be helpful for the final version.

General remarks:

1. **Appellation problem:** Everywhere in the text, the authors use the term "isotope excursions" (or CIE) for long-term trend spanning several Myr. But an excursion is rather a short-term shift coming back to previous values like the negative Early Toarcian CIE. In the text and on Figs. 5 and 7, I would rather use the term "long-term increases or decreases" to be homogenous. Moreover, I remark that periods are

referred as warming or cooling interval, whereas the isotopic curves show the contrary. For example, the Late Sinemurian is referred as a warming interval, whereas temperature decreases on the long term. In my opinion, there is confusion between general climate states and trends, because the Late Sinemurian is a warm period recording a general cooling trend up to the Early Pliensbachian.

2. **Awkward description of trends in the Results and Discussion parts:** From L287 to 317, the description of trends is not very obvious (e.g., L288 A positive shift … is recorded in the Late Sinemurian Densinodulum to part of the Macdonnelli subchronozone => I rather see a negative excursion during the *raricostatum* zone). Examples are numerous in the text. The problem is that the authors describe long-term trends whereas the carbon- and oxygen-isotope curves link data points referring to different belemnites species based on unequal sampling effort. The authors should add a smoothing curve to clearly visualize the long-term trend that they discuss (e.g. a LOWESS regression can be performed with the free software PAST).

Minor remarks:

L156 and 157: thick or thin?

L179 to 221: This new part concerning the paleoecology of belemnites is a good point to discuss the significance of geochemical trends. But this part should be in the first par of the discussion (as 4.1. Reliability of belemnite isotope records) and not in the Methodology. Moreover, specify if belemnite species have been determined (I suppose that they correspond to Nannobelus, Hastites, Passaloteuthis, Acrocoelites, Gastrobelus. As discussed by Ullmann et al. (2014), this can change the interpretation of short-term results (that is why directly linking the data points is problematic => rather use a LOWESS curve as explained above).

L183: discrepancies? I do not understand this word in the sentence.

L187: “life habits” instead “mode of life”

L189, 190, 192, 201, 205, and after: “Nectonic” or “Nektonic” but not “Necktonic”

L194: “it is unclear”

L198: This sentence is not clear:”some belemnite species inhabited environmental niches that remain unchanged”. Do you mean that these stenotopic species are restricted to specific environments?

L204: “The Ullman et al. (2014) work” is awkward. Write it in another way.

L233: “diagenetic”

L234: remove “analyzed”

L313: “a well-marked in the order of …” is awkward. Write it in another way.

L327-328: I don’t feel comfortable with the term “Sinemurian positive CIE”. It would mean

that previous data are lower, abruptly became higher, and come back to lower values. As we have no idea of what happened before the Late Sinemurian in the Asturias and UK data, we would rather talk about "high values decreasing on the long term". Moreover, this discussion is very poor. Compare this recent data from American Panthalassa (Porter et al. 2014, EPSL, New High resolution geochemistry of Lower Jurassic marine sections in western North America: A global positive carbon isotope excursion in the Sinemurian?). What brings this comparison ? what is the trigger of this change?

L331: not a "negative excursion" but a "decrease of values"

L345-349: So what could be the alternative scenario? For example, the authors should mention the recent paper of Martinez and Dera (2015), which explains the long-term decreases in Late Sinemurian $\delta^{13}C$ values by an obvious orbital favouring contrasted seasonal climates and low sea level (both leading to low productivity rates and low organic matter burial [Martinez and Dera, 2015, PNAS, Orbital pacing of carbon fluxes by a ~9-My eccentricity cycle during the Mesozoic]).

L350-354 : The authors need to compare to data from panthalassa (Caruthers et al. (2014) The Pliensbachian–Toarcian (Early Jurassic) extinction: a North American perspective. GSA Special Papers, v. 505). Is there a global positive CIE during the Ibex zone ? Moreover, compare with Morettini et al. (2002) Carbon isotope stratigraphy and carbonate production during the Early Middle Jurassic: examples from the Umbria Marche Sabina Apennines (central Italy), Palaeo3)

L355-367 : What brings this comparison about the global vs. region nature of the carbon isotope trend ? what could be the mechanism? Use the paper of once again, this increase is predicted by astroclimatic controls (Martinez and Dera 2015)

L374:Attach this paragraph to the previous one.

L385: OK but the geochemical expression of the global carbon cycle may be recorded at regional scale (even if sedimentary evidences are lacking).

L409: What is a "normal" temperature? Please, give a range.

L419: As I stated above, the Late Sinemurian is a warm period but the general trend is decreasing (very obvious on Fig. 7)

L420-421: I try to see this negative excursion in your data but it is impossible since we have no previous values. For me, $d^{18}O$ values rise on the long term.

L427: Raricostatum

L431: "warm" but not "warming"

L439-444: This discussion is awkward and confused, especially because the authors are talking about "excursions" and not long-term trends. Other hypotheses could be discussed (Martinez and Dera 2015). On fig 5., $d^{18}O$ values rise from the Late Sinemurian to the *jamesoni* zone whereas $d^{13}C$ decrease of 3 permils. This means that we record a long-term cooling in a warm period. Maximal temperatures are recorded during the Densinodum

subzone, when $d^{13}C$ are the higher. Do you think that higher burial rates of organic matters could have decrease pCO_2 and triggered this long term cooling?

L463: As you mention it, try to explain the link between this warming event and the positive CIE recorded in the Valdani-Luridum subzone. This is important because it is the first time that such a link is shown.

L476: Similar trends in NW Tethyan data indicate that it is "at least" NW Tethyan. We have no evidence from worldwide basins for the moment. Moreover, Nd isotope data from Dera et al. (2009) support the northward intrusion of warm NW Tethyan currents, which could be regional in extent. Please, discuss these points.

L478-479: I would write "recorded by belemnites from the late Pliensbachian to the Early Toarcian…"

L483-485: "calculated by supposing the absence of ice caps"

L509-511: The reference Dromart et al. (2003) is not appropriate as it concerns the Middle-Late Jurassic. Use the references Suan et al. (2010) Secular environmental precursors to Early Toarcian (Jurassic) extreme climate changes, EPSL as well as Silva and Duarte (2015) Organic matter production and preservation in the Lusitanian Basin (Portugal) and Pliensbachian climatic hot snaps. Note that these two authors explain the decrease of CO2 levels by high burial rates of organic matter during the Ibex-Davoei zones (reflected by rising $d^{13}C$ values). You should discuss your data in the same way. In addition, it is important to mention that the general decrease in $d^{13}C$ values was interpreted as a global decrease of productivity rates under orbitally-paced changes in climate seasonality and eustasy (Martinez and Dera 2015).

L623: I do not agree. The atmospheric model of Chandler et al. (1992) shows that polar winter temperatures reached -32°C but rose up to 10°C during summer. These conditions are incompatible with the presence of "permanent" polar ice on the long term. "Seasonal" is enough

L522 to 623: As stated by the first reviewer, I believe that the discussion about glendonites, ice-rafted clasts, glacio-eustasy and temperature gradient may be reduced and mixed with the part 4.2.4. because it is inconclusive about the presence of polar ice caps.