

Interactive comment on “Palaeoclimatic oscillations in the Pliensbachian (Lower Jurassic) of the Asturian Basin (Northern Spain)” by J. J. Gómez et al.

Anonymous Referee #1

Received and published: 4 September 2015

Palaeoclimatic oscillations in the Pliensbachian (Lower Jurassic) of the Asturian Basin: Gómez et al.

The paper presents a detailed carbon- and oxygen-isotope stratigraphy based on Lower Jurassic belemnites from northern Spain and discusses the data in terms of palaeotemperatures and disturbances in the carbon cycle. The biostratigraphic base seems to be sound and the data are a useful addition to the lexicon of studies of Early Jurassic environmental change. However, the paper does skirt over some important issues, which are first flagged up in the abstract. The possibility of Pliensbachian ice (or even icecaps) is mentioned but nowhere is the authors' final opinion given: 'an outstanding cooling event' doesn't really cut it! How do they weigh the evidence? Even

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in the text itself the argumentation is inconclusive: being rather critical of ice being the causative agent behind sea-level fall (p.4055), more favourable to the presence of ice on pages 4056–4057. The so-called 'exotic clasts rafted by ice' could equally well be tree-rafted (they are common in Lower Jurassic pelagic sediments in the Alps deposited in mid-latitudes), and interpretation of glendonites is problematic, as the authors themselves admit, and refers to bottom-water temperatures in any case. Overall, the evidence for relatively cool conditions during the Late Pliensbachian is persuasive but just how cool did the climate become in the mid- and high latitudes? Do the data from the Spanish section add much to this story?

Here, the authors must evaluate critically their belemnite isotopic data. There is a wealth of evidence that many of these molluscs were effectively nektobenthonic and lived for most of their life cycle below the thermocline. Where coeval TEX-86 and belemnite palaeotemperature data exist for the Mesozoic (Mutterlose et al., EPSL, 298, 286–298, 2010; Jenkyns et al., Clim. Past, 8, 215–226, 2012), the belemnites consistently yield lower figures than does the organic geochemical proxy. The data from the Falkland Plateau show, for Upper Jurassic and Lower Cretaceous sediments, a relatively consistent offset of $\sim 14^{\circ}\text{C}$ between the belemnite $\delta_{18}\text{O}$ proxy and TEX-86, suggesting that a similar figure could be added to other molluscan data to approximate sea-surface temperature. If you follow that line of argument, where does that leave your ice?? The paper of Ullmann et al. (2014) needs to be read and digested in conjunction with the above because it suggests that some belemnites migrated into shallower warmer waters following the Toarcian Oceanic Anoxic Event, potentially piling complexity upon complexity!

As I understand it, the McArthur et al. model (p.4048–4049) copied Küspert in assuming local recycling of isotopically depleted waters from below the chemocline to produce a negative excursion; to produce a positive excursion, recycling of isotopically heavy CO_2 (methane being removed without being oxidized) was required. Jenkyns (2003), following Hesselbo et al. (Nature, 406, 392–395, 2000) assumed that a global increase

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in carbon burial produced the overarching positive excursion and that the negative excursion was due to introduction of isotopically light carbon into the ocean–atmosphere system, possibly from a methanogenic source. There is thus a basic split between local and global models for the isotopic signatures of the Late Pliensbachian and Early Toarcian.

So the question is: where is the negative excursion characteristic of the T–OAE in the Spanish section? In many sequences belemnites are missing over the interval of this negative excursion – the belemnite ‘gap’ - probably because of environmental deterioration. Is that the case here? Again, this matter is taken up by Ullmann et al., who report the negative excursion in organic matter from belemnites, even though it is largely lacking in the skeletal carbonate itself. Given that the negative and positive carbon-isotope excursions associated with the T-OAE are found in terrestrial wood (Hesselbo et al., 2007; Hesselbo and Pienkowski, *EPSL*, 301, 365–372, 2011) the whole surface-ocean-atmosphere was clearly affected by these disturbances in the carbon cycle, so the absence of organic-rich facies in one or two localities is irrelevant. And if there is no negative excursion, could the critical black-shale level be lost in a hiatus?

It seems clear that global oceanographic controls must be most important in governing carbon-isotope trends. The widely registered stratigraphic record of Lower Toarcian black shales is testament enough to globally increased rates of organic carbon burial during the T-OAE. The authors cite Kump and Arthur but avoid their principal message, namely that ‘Variations in the carbon isotopic compositions of marine carbonate and organic carbon provide a record of changes in the fraction of organic carbon buried through time’.

Abstract and throughout the paper: the authors consistently confuse rock and time: Upper Pliensbachian sediments were deposited in Late Pliensbachian time characterized by Late Pliensbachian cooling, etc, etc.

Page 4041, lines 14–15: why was warming one of the main causes of Early Toarcian

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faunal turnover? Association does not prove cause and effect. Was creeping anoxia not important?

Page 4043, line 7: Shackleton and Kennett; line 27: Sinemurian

Page 4044, line 20: ‘locally’ not ‘occasionally’; section 3.2 – spell ‘biochronostratigraphy’ correctly

Page 4046, line 8: what does ‘new’ mean in this context? Newly recognized??; line 12: rewrite as ‘A well-marked positive excursion in the order of 1.5‰ . . .’. Similar word-order problems exist elsewhere in the manuscript, including lines 15–16 on this page.

Page 4047, lines 5–6: should be Jenkyns and Weedon (2013). The citation is also given incorrectly in the reference list (p. 4064).

Page 4048: several word-order problems on this page

Page 4051: best to use ‘scattered’ rather than ‘noisy’.

Page 4055: the SO₂ may be a red herring: it would produce acid rain but any long-term effect on climate would probably be trivial compared with addition of volcanogenic CO₂ to promote global warming. Probably better to reference the Sell et al. paper (*EPSL*, 408, 48–56, 2014) by the same authors. Also see Percival et al. (*EPSL*, 428, 256–280.) for direct geochemical evidence (Hg) of the role of Karoo-Ferrar LIP in the genesis of the T-OAE.

Page 4056: the very cold continental temperatures in the Chandler et al., model may not agree with the palaeobotanical evidence. What do the more recent models of Valdes and others say?? This section could benefit from a more detailed and critical discussion . . . if it is to be included in a revised manuscript.

Page 4071: ‘diagenetic’ not ‘digenetic’

Page 4072: ‘Rhaetian’ is correct spelling.

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4073: some of the text is so small as to be illegible. Perhaps have this figure as landscape rather than portrait?

In summary: an interesting and publishable record that could be profitably focused more on direct implications of the data without reviewing in such detail the contentious nature of Early Jurassic climate reconstructions.

Interactive comment on Clim. Past Discuss., 11, 4039, 2015.

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