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Interactive Comment

Interactive comment on "Middle Jurassic-Early Cretaceous high-latitude sea-surface temperatures from the Southern Ocean" *by* H. C. Jenkyns et al.

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Statements such as though provided in this manuscript are not supported by the data presented and/or by the biostratigraphic and chemostratigraphic data that are also reported. The organic geochemical dataset is interesting to the scientific community, however, as Referee 1 stated I also agree that this manuscript can not be published and should be rejected.

Hopefully I can provide comments and criticisms below that will assist Jenkyns et al. in revising their manuscript and getting the TEX86 data published.There are so many significant issues regarding the quality of the data, the stratigraphic sections and correlations used that I can not support publication in its current form. In addition to this, there are many assumptions and interpretations made that have no foundation or justification based on papers cited and the data presented.

In the methodology Jenkyns et al. use an assumption about the calibration of the TEX86 data that already is biasing the reconstruction. Jenkyns et al. state that "the calibration is applicable for regions with SST > 15 degrees C". Hence they are already making the incorrect assumption here that the Antarctic region was greater than 15 degrees Celsius. Also, within the methodology there is no statement about how long Jenkyns et al. have decalcified the sediment, and how the TOC measurements were determined. Was TOC determined through the mass spectrometer or through a Leco TOC analyser? The error stated on the TOC measurements is extremely high, thus suggesting that samples where the TOC concentration is below 1% or even 2% are all within analytical uncertainty. Typical error measurements on TOC should be less than 0.3%. If there are two groups of TOC measurements these should be highlighted in Figure 3 considering the Deroo et al. (1983) data are also cited. During the isotopic analysis of the organic matter, what international standards and internal standards were used? Was a range in known isotopic values (i.e., –40‰ to –10‰ used to correct the isotopic data or was a single standard point used for correction? I would advise Jenkyns et al. however that the latter is thwart with danger as it assumes that the d13Corg values that are being measured do not vary far from the value you are correcting to. And during isotopic excursions such as those recorded in the Jurassic and Cretaceous it is best to use a linear correction using a range of d13Corg values from international and internally-calibrated standards. How can Jenkyns et al. obtain analytical error of <0.1‰ on the d13Corg when the reported error on some international standards is 0.1‰ as defined by the IAEA certificates?

The biostratigraphy of the sections shows that Jenkyns et al. clearly can not make any inference about short-term events (as it is often stated in the manuscript, especially with the term, "cold snaps") when the precise age is not known and the sampling

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density is so low. It has almost become the standard (over the past 5–10 years!) for isotope stratigraphy in the Cretaceous and Jurassic that the sampling resolution is below 10 cm and in some cases even 1cm spacing. Only when one does this type of sampling strategy can one make inferences about short-term (i.e., Milankovitch to sub-Milankovitch timescales), rapid climate events in the deep time. Such biostratigraphic concerns can be derived from the manuscript for example, when Jenkyns et al. state "boundaries are not rigorously defined", "probable late Aptian age", and "tentatively fixed".

Even with the errors, or at least uncertainties, in the biostratigraphic framework of the sections investigated Jenkyns et al. should acknowledge such uncertainties and show them against the stratigraphic sections in the manuscript figures. How can one provide an Aptian and Albian Age for Site 693 and then provide a chronological age in the column also (e.g., 112 Ma). This is even more concerning when there is no boundary provided in the Age column of the section. This section contains 70 m of continuous sedimentary deposition (is it really continuous with no gaps?) and there have only been eleven samples analysed. If this section is so complete it would ideal for conducting a high-resolution record over the Aptian–Albian interval, and I would encourage the authors or others to undertake such a study. Especially a group that involves biostratigraphic workers so that the age can be constrained at much higher precision. Within the Figure 2 caption Jenkyns et al. even go so far as stating that the carbon isotope values are comparable with the "biostratigraphically assigned Aptian-Albian age". These values are typical of 'Mesozoic' sequences and not typical of this poorly assigned age, thus using the values to help constrain age at such a low resolution is not a tool for dating: it actually belittles the years of work conducted by researchers using isotope stratigraphy for correlation and dating. Additionally, the positive excursion in Figure 2 is determined by a single point where the sample spacing is $\sim$30 m. How can anyone state that this is a positive excursion that is typical of this age range and the Lower Albian? Jenkyns et al. even state about the carbon isotope curve in Site 693A that it "has little chemostratigraphic significance". My response to this would be: Why is the data even presented? Why have you not done some more work and constrained the age of the stratigraphic section? Was this just pilot work to see if TEX86 was preserved? Thus, the quality of the data and the interpretation have a lot to be desired; and not only for Site 693A.

Again I repeat my point here about TOC in that the range of TOC reported for Site 693A is between 0.5 and 1.5 wt%, and yet in the methodology you state that the range in error for TOC is between 0.3 and 2 wt%. This is very similar and would suggest that any interpretation of TOC changes stratigraphically is all within analytical error. Another point about Figure 2: why have Jenkyns et al. not plotted the error on the TEX86-derived palaeotemperatures. If the authors did it would indicate that below 430 mbsf there is no distinct variation within analytical uncertainty.

As shown in Price and Gröcke (2002) a summary of the biostratigraphic framework for Site 511 is provided. In Figure 5 of Price and Gröcke (2002) the values are plotted relative to the Sr-isotope curve of the Late Jurassic and Early Cretaceous curve. The reasoning behind not assigning the Sr-isotope curve in the upper part of Site 511 in the Barremian/Aptian interval is that samples from between 540–510 mbsf indicate an upward trend in Sr isotopes, and thus if the lower samples within this stratigraphic interval were assigned to the lower Aptian Sr values (e.g., ∼0.7073), then this would indicate there is extreme condensation in this part of the section. Of course the Price and Gröcke (2002) paper is always open to reinterpretation regarding the Sr-isotope stratigraphy and age that was assigned to the succession, especially in the upper part: again this highlights the need for more detailed investigation of the core using a selection of stratigraphic methods to constrain the age (e.g., isotope stratigraphy at high-resolution, updated biostratigraphic investigations). It should be noted however that Bralower et al. (1997: not cited) indicate through Sr-isotope stratigraphy and biostratigraphy an Aptian age in Site 511 from ∼482 and ∼520 mbsf. Using Bralower et al. (1997) and Price and Gröcke (2002) this would suggest the occurrence of the Barremian/Aptian boundary between ∼525 and ∼520 mbsf. If this is correct then the Lower Aptian oceanic

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anoxic event (Livello Selli) would occur above ∼520 mbsf where TOC decreases from previously elevated values to values below 2 wt%.

Jenkyns et al. go on further to state that the Falkland Plateau (Site 511) data records the Lower Aptian oceanic anoxic event (Livello Selli), however there is no evidence to distinguish this event in Site 511 with the data and biostratigraphy that is presented. The occurrence of an oceanic anoxic event can not be assigned based on TOC contents, as TOC is a response to environmental and regional factors and not a global phenomenon (e.g. Trabucho Alexandre et al. 2011; not cited); as depicted by Tsikos et al. (2003) who showed that the timing of TOC elevation through the Cenomanian/Turonian oceanic anoxic event did not occur synchronously between basins based on carbon-isotope stratigraphy and biostratigraphy. Furthermore, the TOC contents of Site 511 are relatively high (between 2 and 6 wt%) for the entire interval depicted in Figure 3 (over 40 million years), except for the upper most interval that is assigned an age of Upper Aptian where TOC is typically less than 2 wt%. In addition, Jenkyns et al. state that the isotopic curve is characteristic of the Aptian and thus "biostratigraphy and carbon-isotope stratigraphy are hence in agreement". I again would counter argue this statement by highlighting the fact that TOC is relatively constant through the interval (with some relatively low values below 2 wt% in parts), and that the isotopic curve does not resemble the Livello Selli as depicted by Menegatti et al. (1998). In the figure caption Jenkyns et al. further state that the record of the oceanic anoxic event is fixed by both the shape of the d13C curve and stratigraphic enrichment in TOC. Both of which are not like the oceanic anoxic event as depicted in many studies since Menegatti et al. (1998). If this interval is the Livello Selli then the d13Ccarb results from the belemnites as reported in Price and Gröcke (2002) show no such isotopic shifts similar to the characteristic shape depicted in Menegatti et al. (1998); they are relatively consistent from +0.4‰ to –0.6‰ with the uppermost sample having a value of ∼–1.2‰ and do not show the negative values indicated by the d13Corg samples in this report. The single point of -18.5‰ in Figure 3 in Jenkyns et al. is stated to be fixed biostratigraphically as the lower-upper Aptian boundary, and yet this is a single point and not indicative of a trend in the curve.

Some other comments regarding Figure 3 (as Figure 4 I feel is redundant). There is a considerable amount of TEX86 data and yet the d13Corg curve is very low-resolution (21 samples over 140m), so why did Jenkyns et al. not do the same resolution for d13Corg, especially when they had the samples for TEX86. With those relatively high TOC concentrations through the core the amount of material required for d13Corg is very small. My other concerns with these data and figure are the errors provided for the TEX86 measurements. Many of the samples have only been analysed once, and those that are analysed twice show some considerable error (up to 4 degrees Celsius). Why is the rigorous analytical protocol excused on many of these samples, when it is standard procedure? The authors assume a standard error (which is not drawn accurately) for the entire TEX86 curve. Having said this Jenkyns et al. are not honest about the quality of belemnite d18O data. Price and Gröcke (2002) analysed belemnites that were found within the Site 511 core and those that looked to be of good preservation. As reported by many studies by Price, McArthur and others the range in belemnite d18Ocarb values from single horizons can be on the order of up to 6‰ (even shown in Figure 3 of Mutterlose et al. 2010: cited), and yet the variation depicted in Figure 3 of ∼6‰ assumes an average value throughout large sections of the interval: not on single data points at a specific depth. If such a Figure was redrafted with these potential errors and variations the difference between TEX86 and belemnite d18Ocarb would, in some intervals, be on the order of only 2 degrees Celsius. Huber et al. (1995: cited, 2002: cited) suggest that planktonic foraminifera data in the Albian from Site 511 range between 14 to 20 degrees Celsius: thus overlap the belemnite palaeotemperatures. The palaeotemperatures estimated from the belemnite d18O data is assuming an oxygen isotope composition for seawater of –1‰ . However the oxygen-isotope composition of seawater is highly variable around modern oceans and if a seawater composition of 0‰ was used this would warm the belemnite palaeotemperature estimates by ∼4 degrees Celsius, and thus would bring the palaeotemperatures even closer and in many cases even overlap. The other factor that could alter the palaeotemperature reconstruction on belemnites is the salinity of the oceans. Another aspect not discussed or considered in this paper.

Jenkyns et al. state in the abstract the occurrence of "cold snaps" in the Jurassic and Cretaceous, and the evidence they provide for this is from TEX86 data, as depicted in their Figure 4. These "cold snaps" are recorded by a drop in a 1–1.5 degrees Celsius in the TEX86 data. Three "cold snaps" are postulated, which based on the resolution of the data should clearly not be termed "snaps" since that indicates a short-lived period/event. The late Callovian cold event depicted in Figure 4 is only defined by two points, with no older points below in order to determine if it is rapid or different from older sediments from this region. Also if the errors are plotted on these measurements then they overlap with the points above at ∼620 mbsf in the Middle Oxfordian. Therefore are they distinctly different? The next cold snap is at ∼521 mbsf and is defined by a single point (during the Livello Selli), which is dubious in its own right. Single points could be classified as fliers and may not represent what is really occurring through a time interval. Throughout the TEX86 record there are other single point deviations/fliers and yet these are not interpreted as "cold snaps". For example, at ∼617 mbsf and ∼597 mbsf, and equally three warm events at ∼596 mbsf, ∼569 mbsf and ∼504 mbsf that are followed by cooling. However, these are all single point deviations/fliers. The third "cold snap" is given as the the "latest Aptian/earliest Albian" and is only defined again by the single warm point preceding this part of the section. This should not be defined by Jenkyns et al. because it is only based on a single point deviation and why is it stated here that it could be earliest Albian when that occurs at ∼482m mbsf (some 20 m above this "cold snap") based on Bralower et al. (1997). But one of the largest concerns I have with this Figure and interpretation (essentially the focus of the paper: "cold snaps") is that in Figure 3 a broad general envelope is shown for the TEX86 data which incorporates error in the measurement and would indicate that palaeotemperatures for Site 511 are relatively constant and warm for the entire stratigraphic record (which is even stated in the reply by Jenkyns et al. to Reviewer 1): upper Oxfordian to upper Aptian. Over-interpretation is also evident with the TEX86 data from Site 693A where Jenkyns et al. state a warming trend in the Lower Albian, but this is defined only by two points with a sample spacing of ∼10 m between samples. Some of the lower TEX86 samples have errors associated with them that would suggest very little difference in temperature with the uppermost TEX86 data point at ∼406 mbsf.

Although I am no expert on TEX86, I am intrigued by a few issues that the authors have not discussed or ignored. When using the Kim et al. (2010) calibration but using the Liu et al. (2009) assumption the temperatures are reduced: once again bringing them closer towards the uncertainties and errors associated with the palaeotemperature reconstruction. Also, during the OAE interval as defined by Jenkyns et al. in Figure 3 there is a good correspondence between TEX86 and TOC wt% (r2 = 0.84). Why have Jenkyns et al. not discussed this feature, and although it only consists of 4 points I think it requires discussion, especially with respect to the quality of the organic matter in this interval (i.e., Rock Eval). Does the type of organic matter and mode of preservation affect TEX86?

There are a number of extrapolations that I do not find are justifiable. Jenkyns et al. cite one paper on belemnites that overlap benthic bivalves from the Callovian (prior to the study interval investigated in this report), and tend to dismiss other studies (Zakharov et al. 2011; Moriya et al. 2003) and ignore others (e.g., Voigt et al. 2003; Brigaud et al. 2008; Price 2009; all not cited). In order to justify the benthic habitat of the belemnites from the studied interval it would be advisable for the authors to focus on studies that have compared belemnites with other benthic organisms through the Late Jurassic and Early Cretaceous. By looking at one study in the Callovian, we are then led to believe that changes in evolution and environment/climate have not affected the life habitat of the belemnite into the Early Cretaceous. Assuming the belemnite palaeotemperatures are correct these do overlap with estimated Albian planktonic data (Huber et al. 1995, 2002: estimated between 14–20 degrees Celsius). Another study that is missing from the discussion about high-latitude belemnite palaeotemperatures is the Late Jurassic record from New Zealand reported by Gröcke et al. (2003). They suggest that belem-

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nite palaeotemperatures may have been affected by meltwater runoff near NZ because the belemnites record elevated palaeotempertures near 25 degrees Celsius: however, in hindsight maybe these data are accurately reflecting palaeotemperature and not meltwater runoff. If this was the case then the belemnite palaeotemperatures off NZ would be in line with SST temperatures from TEX86. This would then dismiss the idea that belemnites lived below the thermocline. Another major issue with this kind of interpretation with regard to belemnites, is that Jenkyns et al. are assuming the depth of the thermocline where they have no evidence for it. The studies cited regarding belemnite d18O are not time-equivalent.

I am slightly confused with the reference of Clarke and Jenkyns (1999: cited) and a palaeolatitude of 47 degrees South with palaeotemperatures as low as 12 degrees Celsius. Table 2 in Clarke and Jenkyns (1999) provides Late Aptian and Early Albian palaeolatitudes of 54 and 53 degrees South providing a palaeotemperature of 12.2 and 15.6 respectively: there is no reference to 47 degrees South and 12 degrees Celsius. So why is this paper being used to suggest sea-floor re-equilibration? Because in the next paragraph Jenkyns et al. cite Kuhnt et al. (2011) who also produce a bulk carbonate d18O record as evidence for a 3 degree Celcius shift during the Aptian OAE (equivalent to a shift of only $\sim$0.8‰ in d18O) as evidence for mid-latitude temperature drop. So why has the Kuhnt et al. (2011) bulk carbonate data not re-equilibrated? Such confusion is consistent throughout the manuscript where at one point Jenkyns et al. use planktonic foraminifera d18O data (Turonian of Falkland Plateau) for palaeotemperature estimates, and later state, "Because oxygen-isotope values from planktonic foraminifera are typically reset by recrystallization on the sea floor, hence producing spuriously low temperatures (Pearson et al., 2001), benthonic foraminifera records are potentially more reliable. . ..". So I ask myself, which one is it? Are planktonic foraminifera d18O data only good when it supports your data and bad when it doesn't, or should we assume planktonic foraminifera data are always reset by recrystallization? And furthermore I ask, what determines which bulk d18O record has been re-equilibrated or not? Is it just a matter of what fits the data and story of the manuscript, rather than scientific scrutiny?

With respect to the early Aptian oceanic anoxic event, Jenkyns et al. state that the record of the oceanic anoxic event is identified on lithological and biostratigraphical grounds. How does one define an oceanic anoxic event based on lithological grounds? This is at odds to Jenkyns (2010: not cited) who states, "the isotope curve, given that it largely represents a global summation of changes in the rate of carbon burial, potentially supplies a more objective descriptor of an OAE". Clearly this is not the case for the section investigated and reported in this study as the stratigraphic resolution is so poor that distinct isotopic perturbations can not be discriminated against background Mesozoic d13Corg values (see discussion above). Again Jenkyns et al. state that the early Aptian is "fixed by TOC": based on the data presented in Figure 3 would this then suggest that the entire record for this site is one expanded oceanic anoxic event from the Oxfordian to Aptian? After this discussion Jenkyns et al. cite a north Pacific Ocean temperature drop of 4 degrees Celsius for the early Aptian oceanic anoxic event. However, if we rely on the single data point in this study for the Aptian as a cooling episode, it is difficult to understand how the tropics would cool more in an equable climate to the polar regions. One would expect the polar regions to be affected more by changes in global climate than equable warm low-latitude sites.

A further extension of the data in this paper with other data is in Section 8. The Callovian/Oxfordian boundary cool interval is evidenced from Russian and English belemnites depicted in Jenkyns et al. (2002; cf. Fig. 10), but as discussed previously with d18O ranges in belemnites from single beds and horizons, this again is very clear in Figure 10 of Jenkyns et al. (2002). From similar intervals in the Jurassic the palaeotemperature record can be highly variable (up to 2‰ which is equivalent to ~8 degrees Celsius), and the amount of data before, during and after the Callovian/Oxfordian boundary is sparse (see Fig. 10 in Jenkyns et al. 2002). Hence, using the data as reported in Jenkyns et al. (2002) to confirm a cool interval is misleading. One must be careful again using the Lecuyer et al. (2003) dataset since this is a composite of many sites

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around Paris and the UK without strict stratigraphic data against each sample. One must think about questions regarding the correlation between sites and accurate dating of the stratigraphic successions being investigated to produce this composite d18O data from fish phosphate. Notwithstanding that the cool shift (if accurate) occurs over less than 1 million years based on Figure 8 of Lecuyer et al. (2003): the Jenkyns et al. data are no where near this type of resolution. The next paragraph after this discusses the Early Cretaceous and ikaite and yet no reference is really provided against the TEX86 data produced in this report. What is the relevance of this paragraph? The discussion on nannofossil data should cite Mutterlose et al. (2009) not (2010). And when can one in science state, "The cooling trends are indisputable…"? Clearly the scientific method ensures that our ideas are developed and questioned, and in fact the Earth Sciences and understanding deep geologic time is always going to be open to dispute, with new ideas, new data and new techniques…

Now I have some comments in response to the Jenkyns et al. manuscript after reviewer 1 (anonymous) and the reply by Jenkyns et al.: Starting somewhere is ok, but surely with the knowledge and experience of the authors on the Jenkyns et al. manuscript the study would have been more focused to address a scientific question rather than just to state, "What this report does is extend the use of the TEX86 proxy back to the Jurassic for the very first time." This is not telling us more than that and the quality of the biostratigraphy and chemostratigraphy clearly highlights this. If Jenkyns et al. were forthright about the sections they would draft and plot the biostratigraphy against the DSDP/ODP cores, also they would show where the core gaps are thus precluding the acquisition of data from those intervals. And elaborating on their comment, "springboard". In practice one would have produced such a record as depicted in Figure 3 and then be more thorough and investigate one of the so-called "cold snaps" to produce a high-resolution record, for organic geochemistry (TEX86), isotope geochemistry (d13Corg), palaeoceanography (benthic and planktonic foraminifera isotope and elemental data, including Sr-isotope stratigraphy) and biostratigraphy. So why should it be another paper? We are beyond the scope in science of publishing many small papers that eventually could be collated into one paper.

There is very little discussion and introduction about TEX86 and in light that it is a new proxy I think the authors should still highlight all the assumptions and conundrums of this proxy. Just because it has been stated previously in other papers it does not dismiss one from not being aware of that kind of discussion. In fact I find the discussion on TEX86 very brief and in a tone that is matter of fact. Since TEX86 is a relatively new proxy why do Jenkyns et al. (and others) take that palaetemperature data as factuality, and dismiss other previous datasets derived from geochemical proxies? This is a debate occurring with Cenozoic researchers and the discussion about why TEX86 is always producing warmer temperatures than planktonic foraminifera. I think Jenkyns et al. should convince the reader that the "reconstructed Jurassic–Early Cretaceous temperatures are warm and remarkably uniform over a long period of geological time" are so based on TEX86 and why nearly all other geochemical and paleontological data has not alluded to this.

In corroboration with Reviewer 1 I also must agree that the extrapolation of Late Cretaceous TEX86 from the Arctic Ocean to this Late Jurassic and Early Cretaceous TEX86 record is in fact "Geophantasy". What is the relevance of that paper and data to this report? The Jenkyns et al. (2004) paper extrapolates 5 TEX86 values over $\sim$60cm of core to then recalibrate the entire Late Cretaceous d18O chalk record (almost 30 million years): this paper has its own major pitfalls and assumptions which are beyond the scope of this short comment. The BIT data should be shown for every sample in the supplementary information/data for this report. I would disagree with Jenkyns et al. that the relationship between TEX86 and belemnite d18O is clear. In fact using the caveats of Reviewer 1 and those I discuss above there is not a clear offset. For example how sure are we on an extinct fauna how far up and down the water column they move. Modern Nautilus are known to move up and down the water column (hundreds of metres), and cuttlefish are also known to migrate up and down the water column to even the surface. Why should we not hypothesise that belemnites also migrated up and down the water column? Nunn et al. (2010: Geophysical Research Abstracts Vol. 12, EGU2010-8352) show using high-resolution isotope sclerochronology of belemnites that single belemnites record a seasonal signal from the Early Cretaceous of Russia, which ranges between 6–16 degrees Celsius. Apart from a seasonal signal in SST these patterns could also reflect migration within the water column. Furthermore, Jenkyns et al. state that the difference between sea-surface temperatures and the sub-thermocline "are entirely compatible with what one would expect in seas and oceans of a greenhouse world where high latitudes experienced sub-tropical conditions." Could they provide the references that support such a statement for the Late Jurassic and Early Cretaceous. Our understanding of the difference between sea-surface temperatures and the sub-thermocline are poorly understood for younger oceans let alone the time interval that Jenkyns et al. are discussing.

The Sr-isotope stratigraphy at first looks to compound the problems of biostratigraphy but as discussed above it may help, especially with a more up-to-date investigation of these cores for biostratigraphic zonation. Any paper that deals with biostratigraphy and correlation with other sections to interpret climate/environment must provide all the available biostratigraphy and the various positions of boundaries (for example, see Figure 3 and 4 in Gröcke et al. (2003)). If one is to use a "best-fit" then one should graphically represent this along side the stratigraphic column, as is standard practice. Based on the resolution of the carbon-isotope stratigraphy I would clearly state that Jenkyns et al. do not convincingly show the Early Aptian oceanic anoxic event excursion in detail enough to be certain it is this event. In combination with poor biostratigraphy, and the fact they could not recognise the isotopic features (C1-C8) of the Early Aptian excursion (as depicted in Menegatti et al. 1998 and Kuhnt et al. 2011) in detail, I would need to see these greatly improved before being convinced that the oceanic anoxic event from Site 511 occurs between 540 and 516 mbsf as depicted in Figure 4.

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